This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

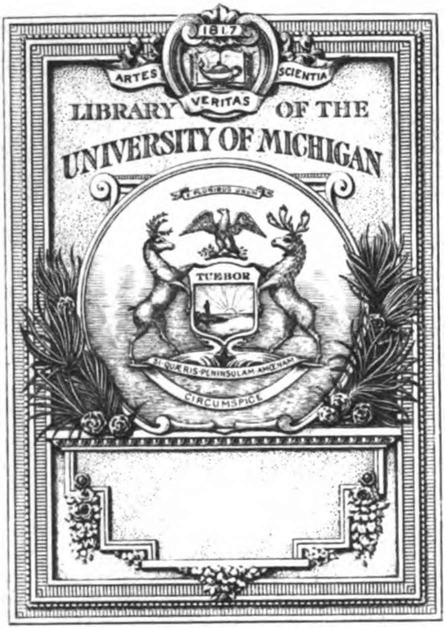
+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

B 1,059,215


1817
ARTES
VERITAS
SCIENTIA
LIBRARY OF THE
UNIVERSITY OF MICHIGAN
TUEBOR
SI QUÆRIS PENINSULAM AMŒNAM
CIRCUMSPICE

Q
1
.L85

16123

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

GENERAL LIBRARY
University of
MICHIGAN

CONDUCTED BY

SIR DAVID BREWSTER, K.H. LL.D. F.R.S. L. & E. &c.
RICHARD TAYLOR, F.L.S. G.S. Astr.S. Nat.H. Mosc. &c.
RICHARD PHILLIPS, F.R.S. L. & E. F.G.S. &c.
AND
ROBERT KANE, M.D. M.R.I.A.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

VOL. XVIII.

NEW AND UNITED SERIES OF THE PHILOSOPHICAL MAGAZINE,
ANNALS OF PHILOSOPHY, AND JOURNAL OF SCIENCE.

JANUARY—JUNE, 1841.

LONDON:

RICHARD AND JOHN E. TAYLOR, RED LION COURT, FLEET STREET,
Printers and Publishers to the University of London;
SOLD BY LONGMAN, ORME, BROWN, GREEN, AND LONGMANS; CADELL;
SIMPKIN AND MARSHALL; S. HIGHLEY; WHITTAKER AND CO.; AND
SHERWOOD, GILBERT, AND PIPER, LONDON: — BY ADAM AND
CHARLES BLACK, AND THOMAS CLARK, EDINBURGH; SMITH
AND SON, GLASGOW; HODGES AND SMITH, DUBLIN:
AND G. W. M. REYNOLDS, PARIS.

CONTENTS OF VOL. XVIII.

NUMBER CXIV.—JANUARY, 1841.

Page

Mr. Airy on the Diffraction of an Annular Aperture 1

Mr. R. Potter's Reply to Mr. Tovey's Remarks on a Paper on the Application of Huyghens's Principle in Physical Optics, inserted in the Lond., Edinb. and Dublin Phil. Mag. for October last; vol. xvii. p. 243 11

Mr. J. O. Halliwell on the Impossibility of the Boetian System of Numerical Contractions, and the Alabaldine Notation having had a common Origin 13

Dr. C. Schafhaeutl on Steam considered as a Conductor of Electricity 14

Mr. W. C. Redfield's Explanation of a Map, showing the Direction of the Wind at Noon, as observed at various Places, in the Storm of December 15, 1839...................... 17

Mr. W. C. Redfield's Remarks relating to the Tornado which visited New Brunswick in the State of New Jersey, June 19, 1835, with a Plan and Schedule of the Prostrations observed on a Section of its Track 20

Mr. J. Stenhouse on a new Compound of Chlorine and Cyanogen... 29

Dr. H. Will on the Composition of Chelidonin and Jervin.... 32

Mr. S. M. Drach on some new and curious numerical Relations of the Solar System 37

Prof. J. C. Poggendorff on the surprising Intensity of Current of the Zinc Iron-circuit, its causes, and some allied subjects 42

Prof. A. Connel's Additional Observations on the Voltaic Decomposition of Alcohol 47

Mr. W. G. Armstrong on the Electricity of Effluent Steam .. 50

Proceedings of the Royal Society 57

Lepidomelane—a new Mineral........................ 77

Hydrotelluric Æther, by M. F. Wöhler 78

Meteorological Observations for November 1840 79

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall at Boston; and by Mr. Dunbar at Applegarth Manse, Dumfries-shire 80

NUMBER CXV.—FEBRUARY.

Mons. A. Rose on the Combination of Hydrated (the Hydrate of) Sulphuric Acid with Nitric Oxide 81

Page
Prof. Fuchs on a new Method of analysing the Ores of Iron, and Crude Iron 90
Mr. J. Williams on the Electricity of Steam 93
Dr. C. Schafhaeutl on the Circumstances under which Steam developes Positive Electricity 95
M. Peltier on the Phænomena of the Electricity of Steam, observed by Mr. Pattinson and Mr. Armstrong 100
Dr. L. Playfair on a new Fat Acid in the Butter of Nutmegs.. 102
Abstract of recent Researches on the Constitution of the fatty Substances made by MM. Redtenbacher, Varrentrapp, Mayer and Bromeis 113
Mineralogical Notices from Foreign Journals: Antigorite, by MM. Wiser and Schweizer; Pennine, by Prof. J. Fröbel and M. Schweizer; Chlorospinelle and Xanthophyllite, by Prof. G. Rose; Pikrophyll, by A. F. Svanberg 120
Mr. J. Stenhouse on Artificial Oil of Ants 122
Mr. R. G. Latham's Facts and Observations relating to the Science of Phonetics 124
The Rev. J. Challis's Reply to Mr. Airy's Remarks on Professor Challis's Investigation of the Motion of a small Sphere vibrating in a resisting Medium 130
Mr. Airy's Correction in a paper published in the Philosophical Magazine for January 1841 132
Mr. W. G. Armstrong on the Electricity of expanding Air, as connected with the Electrical Phænomena of Effluent Steam 133
Dr. Aquila Smith on Irish Tin Ore 134
Prof. J. J. Sylvester's Introduction to an Essay on the Amount and Distribution of the Multiplicity of the Roots of an Algebraic Equation 136
Proceedings of the Royal Society.......... 139
———— Royal Astronomical Society 141
Dr. G. O. Rees's Analysis of Chyle and Lymph.......... 156
New Mineral from Langbanshytta, near Fahlun; described and analysed by Professor O. B. Kühn of Leipzig 157
Remarkable Solar Bow 158
Meteorological Observations for December 1840 159
———— Table.......... 160

NUMBER CXVI.—MARCH.

The Rev. B. Powell on certain points in the Theory of Undulations, related to the Hypothesis of the Symmetrical Arrangement of the Molecules 161
Mr. W. S. Harris on Lightning Conductors, and on Experiments relating to the Defence of Shipping from Lightning 172
Mr. R. Thomas's Remarks on Prof. Whewell's Paper on the Mean Level of the Sea 183

Page
Mr. J. Stenhouse's Analysis of the Oils of Elemi and Olibanum 184
Mr. J. Stenhouse's Chemical Examination of Palm Oil and Cacao Butter 186
Dr. S. Marianini's Examination of a Fourth Experiment adduced by Professor Faraday in support of M. de la Rive's Theory, and regarded by Dr. Fusinieri to be demonstrative.. 193
Messrs. W. Francis and H. Croft's Notices of the Results of the Labours of Continental Chemists 202
Proceedings of the Geological Society 212
——————— London Institution—Conversazioni 234
On the Fossil Wax of Gallicia, by M. P. Walter.......... 235
Phosphate of Copper from Hirschberg, on the Saale, in Russian Voigtland; analysed by Kühn.......... 236
On Minium, by M. Levol.......... 237
Action of anhydrous Phosphoric Acid upon anhydrous Camphoric Acid, by M. P. Walter 238
Meteorological Observations for January 1841 239
——————— Table 240

NUMBER CXVII.—APRIL.

Prof. A. Connel on the Voltaic Decomposition of Aqueous and Alcoholic Solutions 241
Prof. J. J. Sylvester on a new and more general Theory of Multiple Roots.......... 249
Dr. H. Kopp on the Atomic Volume and Crystalline Condition of Bodies, and on the Change of Crystalline Form by means of Heat 255
Dr. C. Schafhaeutl's further Remarks on some of the Circumstances under which Steam developes Electricity.......... 265
Prof. A. De Morgan on a Suggestion relative to Barrett's Method of computing the Values of Life Contingencies 268
The Rev. B. Powell's Note to a Paper "On certain points in the Undulatory Theory, &c." in the last Number of the Philosophical Magazine.......... 270
M. Cantraine's Report on the Memoir on Electric Currents in Warm-blooded Animals, by Prof. Zantedeschi and Dr. Favio, presented to the Royal Academy of Sciences and Belles Lettres of Brussels, on the 4th of April, 1840 271
Messrs. W. Francis and H. Croft's Notices of the Results of the Labours of Continental Chemists (*continued*) 276
Dr. R. Kane's Abstract of the History of a new class of Platina-salts, discovered by M. Gros, with some Remarks on their true Constitution 293
Some Remarks on Messrs. Francis and Croft's Abstracts from the Foreign Journals. By a Correspondent.......... 296

Page
Mr. M. J. Roberts on the Cause of the Production of Daguerreotype Pictures.... 301
New Books:—Griffin's System of Crystallography.—Dr. Kane's Elements of Chemistry 302
Proceedings of the Royal Society 307
———— Geological Society 311
———— Cambridge Philosophical Society 318
Scientific Works lately published.... 319
Meteorological Observations for February 1841 319
———— Table.... 320

NUMBER CXVIII.—MAY.

Mr. Airy's Remarks on Professor Challis's Reply to Mr. Airy's Objections to the Investigation of the Resistance of the Atmosphere to an Oscillating Sphere 321
Mr. W. G. Armstrong on the Electrical Phænomena attending the Efflux of Condensed Air, and of Steam generated under Pressure 328
Mr. W. Kemp on the supposed Moraines of Ancient Glaciers in Scotland, introduced by a Letter from Mr. J. E. Bowman 337
Mr. R. Potter's Examination of the Phænomena of Conical Refraction in Biaxal Crystals 343
Prof. A. Connel on the Voltaic Decomposition of Aqueous and Alcoholic Solutions (*concluded*) 353
Mr. H. Wedgwood on the Natural Arrangement of the Consonantal Sounds.... 363
Messrs. W. Francis and H. Croft's Notices of the Results of the Labours of Continental Chemists (*continued*) 367
Mons. F. de la Provostaye on the Isomorphism of Oxamethane and Chloroxamethane 372
Mr. T. Weaver on the Composition of Chalk Rocks and Chalk Marl by invisible Organic Bodies: from the Observations of Dr. Ehrenberg 375
Mr. J. Prideaux's Notice of an undescribed Native Subsulphate of Iron from Chili.... 397
Proceedings of the Geological Society.... 398
———— London Electrical Society 409
———— Chemical Society of London 410
New Books:—A Collection of Letters illustrative of the Progress of Science in England from the Reign of Queen Elizabeth to that of Charles the Second 411
Orthodox Geography.... 414
Cumyle—Oil of Cummin 415
Meteorological Observations for March 1841 415
———— Table.... 416

NUMBER CXIX.—JUNE.

Page
Mr. T. G. Tilley on some of the Products of the Action of Nitric Acid on Castor Oil.......................... 417
Mr. Detmer on Bleaching Salts 422
Prof. J. J. Sylvester on a linear Method of Eliminating between double, treble, and other Systems of Algebraic Equations.. 425
Messrs. W. Francis and H. Croft's Notices of the Results of the Labours of Continental Chemists (*continued*).......... 436
On the Manufacture of Platinum. By a Correspondent...... 442
Mr. J. Marsh on testing for Arsenic and Antimony by Hume's Process 442
Mr. T. Weaver on the Composition of Chalk Rocks and Chalk Marl by invisible Organic Bodies: from the Observations of Dr. Ehrenberg (*concluded*) 443
Second Letter to Prof. Faraday, from Robert Hare, M.D., Professor of Chemistry in the University of Pennsylvania .. 465
The Rev. J. Challis on the Principles of the Application of Analysis to the Motion of Fluids 477
Prof. J. Henry's Contributions to Electricity and Magnetism. No. IV. On the Electro-dynamic Induction 482
Sir David Brewster's Correction of an Error in Prof. Dove's Letter on the Law of Storms 514
Proceedings of the Chemical Society of London............ 515
——————— London Electrical Society 520
——————— Cambridge Philosophical Society........ 520
——————— Geological Society 522
Meteorological Observations for April 1841 527
——————— Table.................................. 528

NUMBER CXX.—SUPPLEMENT TO VOL. XVIII.

Dr. S. Marianini's Examination of a Fourth Experiment adduced by Professor Faraday in support of M. de la Rive's Theory, and regarded by Dr. Fusinieri to be demonstrative 529
Messrs. W. Francis and H. Croft's Notices of the Results of the Labours of Continental Chemists (*continued*).......... 544
Proceedings of the Royal Society......................... 547
——————— Geological Society....................... 561
——————— Royal Astronomical Society 590
On the Limit to the Action of certain Chemical Reagents.... 604
Analysis of Anilin....................................... 607
Notice by Prof. Dove respecting the Error in his Letter on the Law of Storms, pointed out by Sir David Brewster at p. 514. 608
Index 609

PLATES.

I. and II. Illustrative of Mr. Redfield's Researches on the Phænomena of Storms and Tornadoes.

III. Illustrative of Mr. Kemp's and Mr. Bowman's Papers on the Evidence of the former existence of Glaciers in Scotland.

Errata.

P. 37, line 11 from the bottom, for q read g.

P. 65, line 17 from the bottom, *for* Mohammed Ghizni *read* Mohammed Ghori.

P. 247, line 44, *for* non-negative *read* now negative.

A correction in Mr. Airy's paper, p. 9, line 6, will be found at p. 132.

A List of Errata in Messrs. Francis and Croft's Notices of the Results of the Labours of Continental Chemists will be found at p. 546.

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

◆

[THIRD SERIES.]

JANUARY 1841.

I. *On the Diffraction of an Annular Aperture.* By GEORGE BIDDELL AIRY, *Esq.*, *M.A.*, *F.R.S.*, *Astronomer Royal.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAD no wish to make a communication to you which should assume the form of a discussion with Mr. Potter, and I proposed, therefore, in adverting to the subject to which Mr. Potter has alluded in your Number of October last, rather to add something to the investigation of a point which has perhaps been passed over too lightly by writers on the Undulatory Theory, than to employ myself specially in indicating what I consider to be the failing steps in Mr. Potter's reasoning. The calculations necessary for the first of these objects were troublesome, and (with the little leisure that I can command) occupied a considerable time. Having, however, finished these, I was preparing to address you when I saw Mr. Tovey's paper in your Number for December. I am most happy to find that the very clear explanation given by Mr. Tovey has delivered me from the necessity of remarking on the reasoning in Mr. Potter's paper, and I shall now proceed with my supplementary investigation as an attempt at the extension of the received theory.

PROBLEM.—A plane wave, whose exterior boundary is circular, is interrupted in its course by a circular plate, whose plane is parallel to the front of the wave, and whose centre coincides with the centre of the wave-boundary: it is required to find the intensity of light at any point *near* the line which is normal to the plate at its centre.

Let a be the radius of the plate, b the radius of the boundary of the wave, h the distance of the point from the circular plate, c the distance of the point from the normal line. From the point draw a normal to the plate: the distance of the foot of this normal from the centre of the plate will evidently $= c$. Take the foot of the normal as the origin of polar coordinates for the front of the wave: suppose the whole front of the wave divided into truncated sectors by radii drawn from the foot of the normal: let the angles made by two of these with the line joining the centre of the plate with the foot of the normal be θ and $\theta + \delta\theta$: and let the small truncated sector included between these be divided into small parts by arcs of circles described with the origin as centre: let the radii of two of these arcs be r and $r + \delta r$. The area included between these two arcs is ultimately $= r\delta\theta \times \delta r = \delta\theta \times r\delta r$, and the distance of this area from the point in question is ultimately $= \sqrt{(h^2+r^2)}$. If then the amplitude of the vibration which it excites at the point in question be a function of its distance, but independent or nearly independent of the angle which the line of distance makes with the front of the wave, the amplitude of the vibration produced by the small area $\delta\theta \times r\delta r$ will be represented by

$$\delta\theta \times \phi(\sqrt{h^2+r^2}) \times r\delta r,$$

and the phase being $\frac{2\pi}{\lambda}\left\{vt - \sqrt{h^2+r^2}\right\}$, the absolute disturbance in the ether at the point in question, produced by the small area $\delta\theta \times r\delta r$, at the time t, will be represented by

$$\delta\theta \times \phi(\sqrt{h^2+r^2}) \times r\delta r \times \sin\frac{2\pi}{\lambda}\left\{vt - \sqrt{h^2+r^2}\right\}$$

$$= \sin\frac{2\pi vt}{\lambda} \times \delta\theta \times \phi(\sqrt{h^2+r^2}) \,.\, \cos\frac{2\pi}{\lambda}\sqrt{h^2+r^2} \,.\, r\delta r$$

$$-\cos\frac{2\pi vt}{\lambda} \times \delta\theta \times \phi(\sqrt{h^2+r^2}) \,.\, \sin\frac{2\pi}{\lambda}\sqrt{h^2+r^2} \,.\, r\delta r.$$

And these expressions are now to be integrated with respect to r; then the limits of r, defined by the intersection of the eccentric radius with the circumferences of the two circles (and therefore functions of θ) are to be substituted, and then the whole is to be integrated with respect to θ.

The expressions involving r cannot be integrated in a finite form so long as the general symbol is retained for the function. If, however, we expand it in the form

$$\phi\left\{h + (\sqrt{h^2+r^2} - h)\right\} = \phi(h) + \phi'(h)\frac{\sqrt{h^2+r^2} - h}{1}$$
$$+ \phi''(h)\frac{(\sqrt{h^2+r^2} - h)^2}{1 \,.\, 2} + \&c.$$

they may always be integrated.

We shall, however, at present confine our attention to the case where $\phi\,(\sqrt{h^2+r^2})$ may be represented by $\dfrac{1}{\sqrt{h^2+r^2}}$. The disturbance of ether produced by the small area $\delta\theta \times r\delta r$ at the point in question, is now represented by

$$\sin\frac{2\pi v t}{\lambda} \times \delta\theta \times \cos\frac{2\pi}{\lambda}\sqrt{h^2+r^2}\,.\,\frac{r\,\delta r}{\sqrt{h^2+r^2}}$$

$$-\cos\frac{2\pi v t}{\lambda} \times \delta\theta \times \sin\frac{2\pi}{\lambda}\sqrt{h^2+r^2}\;\frac{r\,\delta r}{\sqrt{z^2+r^2}},$$

the integral of which, with respect to r, is

$$\frac{\lambda}{2\pi}\,.\,\sin\frac{2\pi v t}{\lambda} \times \delta\theta \times \sin\frac{2\pi}{\lambda}\sqrt{h^2+r^2}$$

$$+\frac{\lambda}{2\pi}\,.\,\cos\frac{2\pi v t}{\lambda} \times \delta\theta \times \cos\frac{2\pi}{\lambda}\sqrt{h^2+r^2},$$

which, taken between the limits $r_{\prime}$ and $r_{\prime\prime}$, is

$$\frac{\lambda}{2\pi}\,.\,\sin\frac{2\pi v t}{\lambda}\,.\,\left(\sin\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime\prime}^2}-\sin\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime}^2}\right).\delta\theta$$

$$+\frac{\lambda}{2\pi}\,.\,\cos\frac{2\pi v t}{\lambda}\,.\,\left(\cos\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime\prime}^2}\right.$$

$$\left.-\cos\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime}^2}\right)\delta\theta.$$

We have now to find the values of $r_{\prime}$ and $r_{\prime\prime}$ in terms of θ. For this purpose we have only to remark that the equation of the smaller circle, referred to the foot of the normal as origin, and expressed by means of rectangular coordinates (x being measured through the centre of the circle), is

$$(x-c)^2+y^2=a^2,$$

$$\text{or } (r_{\prime}\cos\theta-c)^2+(r_{\prime}\sin\theta)^2=a^2,$$

$$\text{or } r_{\prime}^2-2c\cos\theta\,.\,r_{\prime}=a^2-c^2,$$

the positive solution of which is

$$r_{\prime}=c\,.\,\cos\theta+\sqrt{a^2-c^2\sin^2\theta};$$

or supposing c so small that the first power only is worth retaining,

$$r_{\prime}=a+c\cos\theta.$$

Therefore

$$\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime}^2}=\frac{2\pi}{\lambda}\sqrt{h^2+a^2}+\frac{2\pi\,.\,ac\,.\cos\theta}{\lambda\sqrt{h^2+a^2}}$$

to the first power of c.

In forming the sine of this quantity we cannot, as before, proceed by expansion in powers of c, neglecting all after the first; and the reason for this prohibition deserves attention, as showing what extreme care is necessary, in a process of approximation, to adopt no method at any one step without due consideration of the effect of rejected terms at that step. Suppose, for instance, the terms depending on higher powers of c in the expression for $\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime}^2}$ were $\frac{1}{1000}$th part of that depending on the first power, and suppose that $\frac{2\pi \,.\, ac \,.\, \cos\theta}{\lambda \,.\, \sqrt{h^2+a^2}}$ were nearly equal to 10π. The rejection of the terms depending on the higher powers would only produce in this angle an error of $\frac{\pi}{100}$, which would be wholly unimportant in these investigations. But the expansion of

$$\sin\left\{\frac{2\pi}{\lambda}\sqrt{h^2+a^2}+\frac{2\pi \,.\, ac \,.\, \cos\theta}{\lambda\sqrt{h^2+a^2}}\right\},$$

in a series proceeding by powers of c, would give a succession of rapidly diverging terms when $\frac{2\pi ac\cos\theta}{\lambda\sqrt{h^2+a^2}}$ had any such value as 10π; and the expression produced by retaining the first and rejecting the higher powers would not in any degree represent the whole. From this point, therefore, we must use methods which do not imply the rejection of any powers of c.

For convenience, put

$$e_{\prime} \text{ for } \frac{2\pi ac}{\lambda\sqrt{h^2+a^2}}, \text{ and } e_{\prime\prime} \text{ for } \frac{2\pi bc}{\lambda\sqrt{h^2+b^2}}.$$

Then

$$\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime}^2}=\frac{2\pi}{\lambda}\sqrt{h^2+a^2}+e_{\prime}\cos\theta,$$

and

$$\sin\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime}^2}=\sin\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\,\cos(e_{\prime}\cos\theta)$$
$$+\cos\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\,\sin(e_{\prime}\cos\theta).$$

Similarly,

$$\sin\frac{2\pi}{\lambda}\sqrt{h^2+r_{\prime\prime}^2}=\sin\frac{2\pi}{\lambda}\sqrt{h^2+b^2}\,.\,\cos(e_{\prime\prime}\cos$$
$$+\cos\frac{2\pi}{\lambda}\sqrt{h^2+b^2}\,.\,\sin(e_{\prime\prime}\cos\theta).$$

In like manner

$$\cos\frac{2\pi}{\lambda}\sqrt{h^2+r_{,}^2}=\cos\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\cos(e_{,}\cos\theta)$$
$$-\sin\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\sin(e_{,}\cos\theta)$$

$$\cos\frac{2\pi}{\lambda}\sqrt{h^2+r_{,,}^2}=\cos\frac{2\pi}{\lambda}\sqrt{h^2+b^2}\,.\cos(e_{,,}\cos\theta)$$
$$-\sin\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\sin(e_{,,}\cos\theta),$$

and these quantities are now to be substituted in the expression above, which multiplies $\delta\theta$, and the whole is then to be integrated with respect to θ.

If our object were to find the intensity of light produced by a portion of the annulus, these expressions could not be made more simple; but in finding the intensity produced by the whole annulus we may introduce an important simplification; for the expressions will then be integrated, with respect to θ, from $\theta = 0$ to $\theta = 2\pi$. Now it is evident that the expansion of $\sin(e_{,}\cos\theta)$ or $\sin(e_{,,}\cos\theta)$ will produce only odd powers of $\cos\theta$; and the integral of every one of these, from $\theta = 0$ to $\theta = 2\pi$, is zero. The terms depending on $\sin(e_{,}\cos\theta)$, $\sin(e_{,,}\cos\theta)$, may therefore be neglected at once; and thus we have, for this investigation,

$$\sin\frac{2\pi}{\lambda}\sqrt{h^2+r_{,}^2}=\sin\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\cos(e_{,}\cos\theta),$$

and so for the others; and the quantity which we have to integrate with respect to θ is

$$\frac{\lambda}{2\pi}\,.\sin\frac{2\pi vt}{\lambda}\,.\left(\sin\frac{2\pi}{\lambda}\sqrt{h^2+b^2}\,.\cos(e_{,,}\cos\theta)\right.$$
$$\left.-\sin\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\cos(e_{,}\cos\theta)\right)$$
$$+\frac{\lambda}{2\pi}\,.\cos\frac{2\pi vt}{\lambda}\,.\left(\cos\frac{2\pi}{\lambda}\sqrt{h^2+b^2}\,.\cos(e_{,,}\cos\theta)\right.$$
$$\left.-\cos\frac{2\pi}{\lambda}\sqrt{h^2+a^2}\,.\cos(e_{,}\cos\theta)\right).$$

Let

$$\int_{\theta}\cos(e\cos\theta)=2\pi\mathrm{E},\quad \int_{\theta}\cos(e_{,}\cos\theta)=2\pi\mathrm{E}_{,},$$
$$\int^{\theta}\cos(e_{,,}\cos\theta)=2\pi\mathrm{E}_{,,},$$

the integrals being taken from $\theta = 0$ to $\theta = 2\pi$. Then the coefficient of $\sin \frac{2\pi v t}{\lambda}$ is

$$\lambda . \left(\mathrm{E}_{\prime\prime} . \sin \frac{2}{\lambda} \sqrt{h^2 + b^2} - \mathrm{E}_{\prime} . \sin \frac{2}{\lambda} \sqrt{h^2 + a^2}\right) = \mathrm{S}.$$

The coefficient of $\cos \frac{2\pi v t}{\lambda}$ is

$$\lambda . \left(\mathrm{E}_{\prime\prime} . \cos \frac{2\pi}{\lambda} \sqrt{h^2 + b^2} - \mathrm{E}_{\prime} . \cos \frac{2\pi}{\lambda} \sqrt{h^2 + a^2}\right) = \mathrm{C}.$$

and if the two terms be aggregated into one of the form

$$\mathrm{P} \sin \left(\frac{2\pi v t}{\lambda} + \mathrm{Q}\right),$$

it is easily seen that P or the amplitude of vibration at the point in question is $\sqrt{\mathrm{S}^2 + \mathrm{C}^2}$; and if the intensity of light be assumed proportional to P^2, it may be represented by $\frac{\mathrm{S}^2 + \mathrm{C}^2}{\lambda^2}$, or

$$\mathrm{E}_{\prime}^2 + \mathrm{E}_{\prime\prime}^2 - 2\,\mathrm{E}_{\prime} . \mathrm{E}_{\prime\prime} . \cos \frac{2\pi}{\lambda}\left(\sqrt{h^2 + b^2} - \sqrt{h^2 + a^2}\right).$$

Our attention must now be directed to finding the numerical value of the function E for different values of *e*.

By the usual expansion,

$$\cos (e \cos \theta) = 1 - \frac{e^2 \cos^2 \theta}{1.2} + \frac{e^4 \cos^4 \theta}{1.2.3.4} - \frac{e^6 \cos^6 \theta}{1.2.3.4.5.6} + \text{\&c.},$$

a series which is always convergent, and may therefore be safely used for numerical computation.

Integrating separately each term, from $\theta = 0$ to $\theta = 2\pi$, and observing that if $\cos^{2n} \theta$ be expressed by simple powers of cosines of multiple arcs, the constant term is

$$\frac{1}{2^n} \, \frac{1.3.5.7 \ldots 2n - 1}{1.2.3.4 \ldots n},$$

we obtain

$$\int_\theta \cos (e \cos \theta) = 2\pi \times \left\{1 - \frac{e^2}{(2)^2} + \frac{e^4}{(2.4)^2} - \frac{e^6}{(2.4.6)^2} + \text{\&c.}\right\}$$

and therefore

$$\mathrm{E} = 1 - \frac{e^2}{(2)^2} + \frac{e^4}{(2.4)^2} - \frac{e}{(2.4.6)^2} + \text{\&c.}$$

By means of this series I have computed the value of E for every value of e, proceeding by steps of 0·2 from 0·0 to 10·0. Although the series does in all cases ultimately converge with great rapidity, yet for large values of e it commences by diverging rapidly. Thus for $e = 10·0$ the successive terms are

1·0,
− 25·0,
+ 156·25,
− 434·0277,
+ 678·168, &c.

and for this value it was necessary to proceed as far as e^{36}. The calculations have been made to six places of decimals, but I have thought it prudent to retain only four places. I may add, that the calculations have not been examined with great care, but that, from the care with which they were made and the general regularity of progression among the numbers, I am confident that there is no large error in the results.

Table of the values of $E = \frac{1}{2\pi}\int_{\theta} \cos(e\cos\theta)$, the limits of the integral being 0 and 2π.

e.	E.	E^2.	e.	E.	E^2.
0·0	+1·0000	1·0000	5·2	−0·1104	0·0122
0·2	+0·9900	0·9802	5·4	−0·0412	0·0017
0·4	+0·9604	0·9224	5·6	+0·0269	0·0007
0·6	+0·9120	0·8318	5·8	+0·0917	0·0084
0·8	+0·8463	0·7162	6·0	+0·1507	0·0227
1·0	+0·7652	0·5855	6·2	+0·2018	0·0407
1·2	+0·6711	0·4504	6·4	+0·2433	0·0592
1·4	+0·5669	0·3213	6·6	+0·2740	0·0751
1·6	+0·4554	0·2074	6·8	+0·2931	0·0859
1·8	+0·3400	0·1156	7·0	+0·3001	0·0900
2·0	+0·2239	0·0501	7·2	+0·2951	0·0871
2·2	+0·1104	0·0122	7·4	+0·2781	0·0773
2·4	+0·0025	0·0000	7·6	+0·2516	0·0633
2·6	−0·0968	0·0094	7·8	+0·2154	0·0464
2·8	−0·1850	0·0342	8·0	+0·1727	0·0298
3·0	−0·2601	0·0677	8·2	+0·1222	0·0149
3·2	−0·3202	0·1025	8·4	+0·0691	0·0048
3·4	−0·3643	0·1327	8·6	+0·0144	0·0002
3·6	−0·3918	0·1535	8·8	−0·0394	0·0016
3·8	−0·4026	0·1621	9·0	−0·0907	0·0082
4·0	−0·3971	0·1577	9·2	−0·1365	0·0186
4·2	−0·3766	0·1418	9·4	−0·1768	0·0313
4·4	−0·3423	0·1171	9·6	−0·2091	0·0437
4·6	−0·2961	0·0877	9·8	−0·2324	0·0540
4·8	−0·2404	0·0578	10·0	−0·2460	0·0605
5·0	−0·1776	0·0315			

It appears from this table that E vanishes for the values of

e, 2·405, 5·52, 8·64, nearly; and that the successive maximum values of E^2 correspond to the values of e, 0·00, 3·85, 7·01, 10·15, nearly; and that the corresponding maximum values of E^2 are nearly 1, $\frac{1}{6}$, $\frac{1}{11}$, $\frac{1}{16}$.

I will now proceed to point out the practical application of this table.

First, suppose the value of $\frac{2\pi}{\lambda}\left(\sqrt{h^2+b^2}-\sqrt{h^2+a^2}\right)$ to be small, or not exceeding a few multiples of π.

In this case no considerations can be introduced which tend to simplify the expression for the intensity of the light. If the light be heterogeneous, it will be necessary to form the numerical values of $e_{\prime}$, $e_{\prime\prime}$, $E_{\prime}$, $E_{\prime\prime}$, and $\frac{2\pi}{\lambda}\left(\sqrt{h^2+b^2}-\sqrt{h^2+a^2}\right)$ for each different colour, and to substitute them in the expression $E_{\prime}^2+E_{\prime\prime}^2-2\,E_{\prime}\,E_{\prime\prime}\,.\cos\frac{2\pi}{\lambda}\left(\sqrt{h^2+b^2}-\sqrt{h^2+a^2}\right)$. It is easily seen that the greatest intensity of light under any circumstances is $(E_{\prime}+E_{\prime\prime})^2$ and the least is $(E_{\prime}-E_{\prime\prime})^2$.

Secondly, suppose the value of $\frac{2\pi}{\lambda}\left(\sqrt{h^2+b^2}-\sqrt{h^2+a^2}\right)$ to amount to many multiples of π.

Whatever care be taken to make light homogeneous, it is practically impossible to make it homogeneous or even nearly so. Therefore the value of $\frac{2\pi}{\lambda}\left(\sqrt{h^2+b^2}-\sqrt{h^2+a^2}\right)$ will vary, perhaps to the amount of many multiples of π, for the different values of λ corresponding to the different kinds of light in the stream of light which is the subject of experiment. And this may happen even when the portion of the spectrum corresponding to the light used is so short that there is no conspicuous change of colour. In aggregating the effects of these, it is to be observed that, for points near to the central normal of the circular plate, the change produced in $e_{\prime}$ and $e_{\prime\prime}$ by the change of λ is very small, and the only variation which needs to be considered is that of $\cos\frac{2\pi}{\lambda}\left\{\sqrt{h^2+b^2}-\sqrt{h^2+a^2}\right\}$.

As the arc included in this quantity changes its value by several multiples of π, the cosine will have equal values repeatedly positive and negative, and the sum of all may be considered $= 0$. This reduces the expression for intensity to $E_{\prime}^2 + E_{\prime\prime}^2$, which will be easily discussed.

Example 1.—The light enters through a round hole without interruption.

Here $a = 0$, and therefore $e_{\prime} = 0$, and $E_{\prime} = 1$, and the expression becomes $1 + E_{\prime\prime}^2$.

The smallest value of this quantity occurs when $E_{\prime\prime} = 0$. This happens when $e_{\prime\prime} = 2{\cdot}41$, or $= 5{\cdot}52$, or $= 8{\cdot}64$, &c., or when $e_{\prime\prime}$ is large. For all these cases the intensity is represented by 1.

The greatest value occurs when $e_{\prime\prime} = 0$, and the intensity is represented by 2; the next maximum is when $e_{\prime\prime} = 3{\cdot}8$, and the intensity is 1·16; the third maximum is when $e_{\prime} = 7{\cdot}0$, and the intensity is 1·09; the fourth maximum is when $e_{\prime\prime} = 10{\cdot}1$, and the intensity is 1·06.

Thus there will be at the centre a bright spot of double the general intensity, surrounded by rings brighter than the great expanse of light; but the excess of intensity in the rings, even the first, is so small, that it probably could not be seen. The whole diameter of the bright spot, to its very extremities, is $2c$, when $\dfrac{2\pi bc}{\lambda\sqrt{h^2 + b^2}} = 2{\cdot}41$, or $c = \dfrac{\lambda\sqrt{h^2 + b^2}}{2\pi b} \times 2{\cdot}41$; but the visible diameter would be much smaller.

Example 2.—The stream of light is unlimited externally, but an opake circle of radius a interrupts a portion of it.

Here $e_{\prime\prime}$ is very great, and $E_{\prime\prime} = 0$, and the expression for the intensity of the light is simply $E_{\prime}^2$. The intensities therefore at different distances along any radius drawn from the central normal parallel to the plane of the circle are represented by the numbers in the last column of the table above. The first maximum occurs when $e_{\prime} = 0$, and it is then $= 1$, or is the same as the intensity in an uninterrupted pencil; the second when $e_{\prime} = 3{\cdot}8$, and it is then $\frac{1}{6}$ of the former; the third when $e_{\prime} = 7{\cdot}0$, and it is then $\frac{1}{11}$ of the former; the fourth when $e_{\prime} = 10{\cdot}1$, and it is then $\frac{1}{16}$ of the former. The light absolutely disappears when $e_{\prime} = 2{\cdot}41$, or $= 5{\cdot}52$, or $= 8{\cdot}64$. Thus there is a bright spot whose central intensity is equal to that of uninterrupted light, surrounded by rings of much feebler light, whose intensity decreases rapidly till it becomes insensible.

The whole diameter of the bright spot is $2c$, when $e_{\prime}$ or $\dfrac{2\pi ac}{\lambda\sqrt{h^2 + a^2}} = 2{\cdot}41$, or $c = \dfrac{\lambda\sqrt{h^2 + a^2}}{2\pi a} \times 2{\cdot}41$; or the diameter of the spot is $\dfrac{\lambda\sqrt{h^2 + a^2}}{\pi a} \times 2{\cdot}41$.

Suppose, for instance, a circular plate 1 foot in diameter is placed to interrupt a stream of light; a screen is placed 10 feet behind it; to find the diameter of the bright spot.

Here $h = 120$ inches, $a = 6$ inches; λ may be taken for mean light $= 0{\cdot}00002$ inch. Substituting in the expression above, we find the diameter of the bright spot $= 0{\cdot}000308$ inch, or less than $\frac{1}{3000}$ inch, regarding the visibility of which in common experience we need not to disquiet ourselves.

If the diameter of the circular plate had been taken 1 inch, the distance of the screen remaining the same, the diameter of the spot would have been 0·0037 inch, a speck difficult even for a philosopher to discover under these circumstances. If the diameter of the plate were 0·1 inch, the diameter of the spot would be 0·037 inch, a very fit subject for experimental measure. It is on such spots as this that observations have been made, with the purpose of testing the undulatory theory, and of the agreement of those measures with the theory no one acquainted with both has, I believe, doubted.

With regard to any objection that may have been made, either against the undulatory theory generally or against the application of Huyghens's principle in particular, from confronting the result of a simple investigation of the intensity of light at one mathematical point, with the fact of general experience that light is not visible behind an opake screen; the inference from the preceding investigation is, I believe, that the undulatory theory in general, and the application of Huyghens's principle in particular, stand as firmly as they did before appeal was made to this comparison, perhaps even more firmly. For the future I would remark that, however much we may doubt whether one mathematician is entitled to give to another a lesson of *caution*, this principle must, I think, be allowed by every one,—that a theory standing on such varied and such extensive evidence as that which supports the undulatory theory is entitled to the same respect which is given to a private person of high character; it is not to be rashly attacked until the supposed grounds of attack have been most thoroughly examined, although if these grounds are valid, it possesses no immunities beyond other theories.

I am, Gentlemen,
Your very obedient servant,
Royal Observatory, Greenwich, Dec. 4, 1840. G. B. AIRY.

II. *Reply to* Mr. Tovey's *Remarks on a Paper on the Application of* Huyghens's *Principle in Physical Optics, inserted in the Lond. Edinb. & Dublin Phil. Mag. for October last*; vol. xvii. p. 243. *By* Richard Potter, *Esq.*, *B.A.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I BEG to return Mr. Tovey my thanks for keeping alive the discussion on the capability of the undulatory to explain facts; by such discussions we may entertain hopes that some sound progress will eventually be made in physical optics.

Mr. Tovey says I have mistaken a luminous *line* for a luminous *space*, and consequently my conclusions have, in reality, no foundation. He ought unquestionably to have given us his proof.

In the investigations for a small opake disc and a small circular aperture, it was considered, by the most eminent mathematicians who have adopted the undulatory theory of light, as quite sufficient to find the intensity along a line drawn perpendicularly through the centre of the aperture or disc, and their results were brought forward as strong proofs of the truth of the theory. When, in the same way, I have applied a more correct analysis, for all magnitudes of circular discs and apertures, it is objected to, without showing where the limit between a large and a small magnitude lies, and at what point the method becomes inaccurate.

Our knowledge of the phænomena of diffraction leads us to conclude that there can be no maxima and minima in the line before-mentioned without accompanying ones in the plane perpendicular to that line, and such has tacitly been received with respect to former investigations. With this the undulatory theory also accords. How Mr. Tovey could suppose that there is in reality in any case only a luminous line, it is not easy to account for; if he had been acquainted with the phænomena I think he would not have made the objection.

The case of the fixed stars shows that the smallness of the angular magnitude of the luminous body at the eye is no bar to light being perceived when there is adequate intensity.

The following analysis shows that the undulatory theory indicates maxima and minima in the plane perpendicular to the line, for which my former results were investigated.

Taking now a point out of the line (C B) passing perpendicularly through the centre of the circular aperture or annulus, let its distance from that line be small and equal to x.

Let θ be the angle which any radius makes with the plane passing through the perpendicular line and this point. Then $r\,\delta\theta\,\delta r$ will be an elementary area in the aperture, and for simplicity I take the case of plane waves, the other only differing by a constant.

Putting h and a the same as before, we have the displacement of the particle at the point, arising from this element.

$$= \frac{r\,\delta\theta\,\delta r}{\sqrt{r^2+h^2+x^2-2\,r\,x\cos\theta}}$$

$$\times \sin\frac{2\,\pi}{\lambda}\left(v\,t-\sqrt{r^2+h^2+x^2-2\,r\,x\cos\theta}\cdot\right)$$

Expanding and retaining the terms involving the first power of x only, we have the whole displacement

$$= a\int_r\int_\theta\frac{r}{\sqrt{r^2+h^2}}\sin\frac{2\,\pi}{\lambda}\left(v\,t-\sqrt{r^2+h^2}\right)$$

$$+\frac{2\,\pi\,a\,x}{\lambda}\int_r\int_\theta\frac{r^2\cos\theta}{r^2+h^2}\cos\frac{2\,\pi}{\lambda}\left(v\,t-\sqrt{r^2+h^2}\right)$$

$$+a\,x\int_r\int_\theta\frac{r^2\cos\theta}{(r^2+h^2)^{\frac{3}{2}}}\sin\frac{2\,\pi}{\lambda}\left(v\,t-\sqrt{r^2+h^2}\right)$$

$$= a\int_r\frac{r\,\theta}{\sqrt{r^2+h^2}}\sin\frac{2\,\pi}{\lambda}\left(v\,t-\sqrt{r^2+h^2}\right)$$

$$+\frac{2\,\pi\,a\,x}{\lambda}\int\frac{r^2\sin\theta}{r^2+h^2}\cos\frac{2\,\pi}{\lambda}\left(v\,t-\sqrt{r^2+h^2}\right)$$

$$+a\,x\int_r\frac{r^2\sin\theta}{(r^2+h^2)^{\frac{3}{2}}}\sin\frac{2\,\pi}{\lambda}\left(v\,t-\sqrt{r^2+h^2}\right)+\text{C}$$

and taken between the limits $\theta = 0$ and $\theta = 2\,\pi$

$$=2\,\pi\,a\int_r\frac{r}{\sqrt{r^2+h^2}}\sin\frac{2\,\pi}{\lambda}\left(v\,t-\sqrt{r^2+h^2}\right),$$

which is the same expression as on the line itself, and indicates a maximum in the plane perpendicular to the line when there is a maximum in the line.

In examining the terms involving x^2, I shall retain only the principal term in the coefficient of that quantity, which has λ^2 in its denominator, and thus the quantity which is the correction of the last found expression is the following:

$$-\frac{2\,\pi^2\,a\,x^2}{\lambda^2}\int_r\int_\theta\frac{r^3\cos^2\theta}{(r^2+h^2+x^2)^{\frac{5}{2}}}\sin\frac{2\,\pi}{\lambda}\left(v\,t-\sqrt{r^2+h^2+x^2}\right)$$

between the limits $\theta = 0$ and $\theta = 2\,\pi$

$$= -\frac{2\pi^3 a x^2}{\lambda^2}\int_r \frac{r^3}{(r^2+h^2+x^2)^{\frac{3}{2}}} \sin\frac{2\pi}{\lambda}\left(v\,t - \sqrt{r^2+h^2+x^2}\right)$$

which we see is a small quantity as long as x is a quantity of the same order of magnitude as λ, and h is many multiples of r, which would still give the case of a point in the shadow of a large circular disc, the aperture being an annulus; but to take the extreme case which can be urged as an objection to my former results, let h be small compared with r, or the point deeply immersed in the shadow. The integral being separated into two we have

$$-\frac{2\pi^3 a x^2}{\lambda^2}\int_r \frac{r}{\sqrt{r^2+h^2+x^2}} \sin\frac{2\pi}{\lambda}\left(v\,t - \sqrt{r^2+h^2+x^2}\right)$$

$$+\frac{2\pi^3 a x^2}{\lambda^2}\int_r \frac{(h^2+x^2)\,r}{(r^2+h^2+x^2)^{\frac{3}{2}}} \sin\frac{2\pi}{\lambda}\left(v\,t - \sqrt{r^2+h^2+x^2}\right).$$

We now reject the latter as very much smaller than the former, and have only to perform the same integration as in my former paper, and the intensity becomes for an aperture which is an annulus and the radii r_1 and r_2,

$$= 4\,a^2\lambda^2\left(1-\frac{\pi^2 x^2}{\lambda^2}\right)^2 \sin^2\frac{\pi}{\lambda}\left(\sqrt{r_2^2+h^2+x^2}-\sqrt{r_1^2+h^2+x^2}\right).$$

The maximum intensity on the line being $4\,a^2\lambda^2$, there will be a minimum equal to zero when $x = \pm\frac{\lambda}{\pi}$, and then the intensity will increase again, and apparently to a greater magnitude than before, but our having used approximate expressions prevents us pushing our conclusions further.

We see clearly how maxima and minima are indicated by the theory in the plane perpendicular to the line in which I formerly discussed the results of the same method, and how much Mr. Tovey has mistaken the question.

Queens' College, Cambridge, Dec. 8, 1840. RICHARD POTTER.

III. *The Impossibility of the Boetian System of Numerical Contractions, and the Alabaldine Notation having had a common Origin. By* J. O. HALLIWELL, *Esq., F.R.S. &c.*

ON a further examination of the Arundel MS. No. 343, the real explanation of which has already appeared in this Magazine, and on working out all the examples in that manuscript with the tedious notation of the Boetian and Alabaldine contractions, I have met with the following, which shows more clearly than any other I have yet discovered, the accuracy of the views taken by M. Chasles and myself, on the impossibility of the Alabaldine system having commenced with the abacus. I merely give the early steps, which will be quite sufficient to show the correctness of my statement.

Required the result of dextans. siliqua. ceraces.

The reduction of dextans:

$$\text{dextans} = \frac{\text{igin. sipos}}{\text{andras. calcis}} = \frac{\text{andras. quinas}}{\text{andras. calcis}} = \frac{\text{quinas}}{\text{calcis}}$$

$$= \text{semiuncia. quicunx.}$$

The very first step of which uses the decimal scale.

IV. *On Steam considered as a Conductor of Electricity. By Dr.* Charles Schafhaeutl*.

In the last Number of the Philosophical Magazine, the electricity obtained from a jet of high-pressure steam was considered to be of similar origin with that obtained from the insulated and separated positive metallic disc of Volta's Electrophorus.

On this point, the first question which presents itself is, in what relation does steam or water-gas stand with the conductors or non-conductors of electricity.

It is well known that moist air is a conductor of electricity, and dry air, viz. air which contains less water-gas than it is capable of containing according to its temperature, is a non-conductor of electricity; but, besides this, I am not aware of any experiment made to ascertain the conducting power of pure steam without being in contact with water or mercury, and I therefore determined to ascertain this question by experiment.

The ends of a glass tube, about two inches long and a quarter inch interior diameter, were drawn out over a lamp to points, and bent upwards in a right angle. A thick platinum wire, with a small ring formed at each end, was then inserted into one end of the tube, and the glass melted around it air-tight. Water was then poured into the tube and boiled till only about two drops remained, when another platinum wire was inserted at the other end, and the hole quickly hermetically closed as before. The distance between the ends of the two wires in the tube was about one inch and a quarter, and the tube in this state contained of course nothing but water-gas and some liquid water.

This tube was now inserted close to the bulb of a thermometer into a small sand-bath †, and covered with the sand, excepting the two vertical ends.

---

* Communicated by the Author, whose former communication on the subject appeared in our last Number; L. E. and D. Phil. Mag. vol. xvii. p. 449.

† I cannot omit to mention the kindness I experienced from the gentlemen lecturing at the Adelaide Gallery, in allowing me the use of their

One of these platinum wires was then connected with the outside of a Leyden jar, the other with an insulated discharger.

The Leyden jar, containing about 100 square inches armed surface, was now charged by means of fifty revolutions of a twelve-inch glass plate, and then discharged through the tube as usual. The glass tube acted exactly as an imperfect conductor, interrupting the conducting wire, which connects the two surfaces of the jar, like a piece of wet cotton thread, or portion of glass tube moistened inside. The jar was perfectly discharged by the first touch, with that peculiar hissing noise and reddish fascicular stream of electricity which invariably occur under similar circumstances. The temperature of the sand-bath was now gradually raised, and at every 5 degrees a similar electric discharge from the Leyden jar was passed through the tube. The same results were obtained until the thermometer reached 250 degrees. At this point, by discharging the jar, a small red spark was obtained, instead of the former fascicular stream, and the jar was found to be entirely discharged, although the noise occasioned by the spark was scarcely audible, compared with the loud clap produced by the discharge of the jar under ordinary circumstances.

After the temperature had been elevated to 405 degrees, the contents of the jar discharged with the usual brilliant spark and loud report, and at the same time the spark was seen passing through the tube. At this time no moisture could be detected in the tube, and the water-gas contained in it had entirely ceased to be a conductor of electricity, at the same time giving less resistance to the passage of the spark than common air, the striking distance having been elongated from half an inch to one inch and a quarter. When the temperature was reduced below 405 degrees, the discharge passed as before mentioned, either as a small red spark, or in a fascicular stream, according to the temperature. When above 405 degrees the spark passed as usual, until the temperature rose to 443, when the tube burst, which prevented me from ascertaining its weight with and without the water; the difference of which would of course have given me the weight of the water contained in the tube, the cubical contents of which would have been ascertained by filling it with quicksilver. If we assume that there were two drops of water in the tube when it burst, weighing together 0·73 grains, and supposing the contents of the tube to be 500 cubic lines, we have the pressure of 23·5 atmospheres therein.

laboratory and philosophical apparatus for the purpose of making these experiments. Institutions of this description, which are to be found only in England, are invaluable to foreigners occupied in philosophical researches.

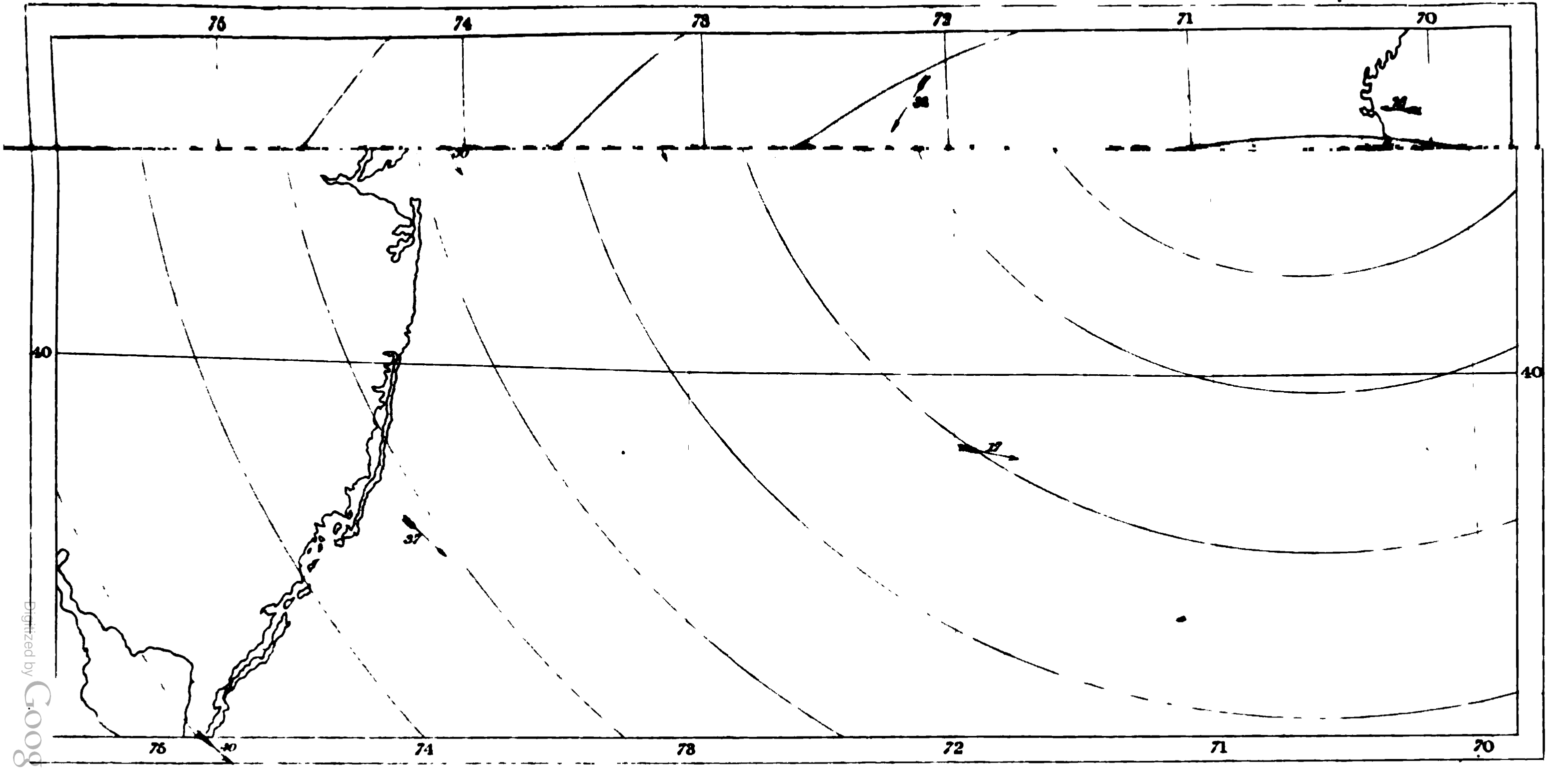
From the preceding experiments we may conclude that steam, pure and free from contact with water, has, like all other gases, the property of being a non-conductor of electricity.

The facility with which the spark passes through the water-gas seems to be worthy of attention; the striking distance of the spark having increased from half an inch to one inch and a quarter; for, according to Mr. Harris's discoveries, the striking distances are, in ordinary cases, in the inverse ratio to the density of the gas.

If we now consider the fact, that the electricity of a jet of condensed steam is, according to Mr. Armstrong's experiments, positive; that the quantity of electricity obtained from a jet of high-pressure steam is in proportion to its condensation; further, that the steam contained in the boiler presents no appearances of free electricity; and that, according to Mr. Pattinson's experiments, both water and boiler are negative, which is a necessary consequence of Mr. Armstrong's experiments;—we perceive a simultaneous development of electric polarity in opposite directions from a central or neutral point, as in the magnetic steel-bar; and this development of electric polarity can only be ascribed to the opposite changes of molecular arrangements, as well as chemical condition of the water and steam column; and we must consider both electric poles as co-existent and not separately.

Volta's electrophorus is only remarkable for the property of retaining its electricity for a lengthened period, and its action is entirely due to *induced electricity*, with which no one will confound the electricity obtained from steam. Besides, the disc of the electrophorus, from which the spark is obtained, must be a perfect conductor well insulated, and shows signs of free electricity only when it is, after close contact with the electrophoric cake and neutralization of its free electricity, removed absolutely from the inducing cake. In a boiler filled with steam and water, neither of the above-mentioned circumstances can take place, and the positive electricity of the condensed steam, and the negative electricity of the boiler are the only points ascertained by experiment. The electricity developed by evaporation, as the source of the observed free electricity, is only hypothetical.

Volta's experiment, of splashing water on ignited charcoal, can scarcely be considered as identical with the evaporation of water in a boiler; in the first instance, a mass of chemical decomposition and changes are taking place which never can occur in the latter, for even the sudden cooling of substances is sufficient to produce signs of free electricity.



DIRECTION OF WIND AT NOON OBSERVED BY W. C. REDFIELD, DEC$^{\text{R}}$. 15 1830.

If we ascribe the electricity of steam to its condensation, the circumstances under which that condensation takes place are likewise of great influence. The smallest jet of high-pressure steam developes more free electricity than 100 times greater quantity of low-pressure steam;—another condition under which electricity is produced from a jet of steam, seems therefore to be its *rapid expansion* when issuing from the boiler; or probably, as I have suggested on a former occasion, the quantity of caloric becoming latent during the expansion of high-pressure steam, has some relation to the quantity of electricity being set free. Even electricity in thunder-storms seems to be ascribable partially to the rapid currents of air whirling towards the centre of the clouds, as caloric is absorbed whilst the thunder-clouds are charging themselves.

V. *Explanation of a Map, showing the Direction of the Wind at Noon, as observed at various Places, in the Storm of December* 15, 1839. *By* W. C. Redfield, *Esq.*

THE arrows on the map, Plate I. denote, approximately, the direction of wind at noon at the several places of observation. The concentric lines, drawn at intervals of thirty miles, were added, not as precisely indicating the true course of the wind, but in order to afford better means of comparison for the several observations.

The assumed axis of the gale, at this time, should probably have been placed more to the westward, on a line with the position of the Morrison and Cape Cod Bay, at which points the gale was then blowing with great violence in opposite directions. The Morrison was from China, bound to New York, and I have reason to believe that her position may be safely relied on. The ship, as I am informed, was lying to at noon with bare poles, and had taken the western side of the gale suddenly at 7 A.M. This gale was severely felt in most of the region comprised in this map, excepting its north-western and extreme northern portion, and excepting also the light winds which were found near the axis of the gale in the vicinity of Buzzard's Bay, &c. in the afternoon and evening. A very heavy fall of snow accompanied the gale in the states of Connecticut, Rhode Island, Massachusetts, New Hampshire and Maine, also in parts of the states of New York and Vermont. Some snow also fell in the western and northern parts of New York and Vermont, but attended with moderate winds, chiefly from the north and north-west.

Abbreviations—N.H. New Hampshire.—Me. Maine.—Ms. Massachusetts.—R.I. Rhode Island, State.—Ct. Connecticut.—L.I. Long Island.—N.Y. New York, State.—N. J. New Jersey. *Note.* My observations on the 15th P.M. have on a former occasion been erroneously printed N.W. by W.; *read* N.W. by N.

Schedule of the Observations on the Direction of Wind in the Storm of December 15, 1839, *as indicated on the annexed Map.*

No.*	Places of Observation.	A.M.	Noon.	P.M.	Observers and Authorities.
1.	Nantucket, Ms....	E.	S.E. at 1 P.M.	S.W.	Report of J. Mitchell, as published by Mr. Espy.
2.	Barnstable, Ms.	N.E. at 7 A.M. Gale from S.E.	(E.S.E.)	E. 2 P.M.—S.E. Sunset ... ... S.W. P.M.—Sunset, clear	Report to the Editor of the Boston Courier. Letter from Wm. H. Brown, Barnstable. I take the mean between E. and S.E. for the true direction at noon.
3.	New Bedford, Ms.	Sunrise N.E. moderate. Sunrise E. fresh.	(E. by N.)	E.N.E. 2 P.M.—S., 3½ P.M. ... E. 2 P.M.—S.S.E., Sunset.	Joseph Congdon's Meteorological Journal. Samuel Rodman's do., as published by Mr. Espy. I take E. by N. as the mean for noon.
4.	Newport, R. I.	N.E.	(N E.)	N.E.	Prof. Caswell's Meteorological Journal.
5.	Cape Cod Bay.	E.S.E.	E.S.E.	E.S.E. at 2 P.M.	Report of Capt. Slemmer, brig Columbus.
6.	Cape Cod, Provincetown, Ms.	E.S.E.	E.S.E.	E.S.E.	Marine reports in the Boston Newspapers.
7.	Providence, R. I.	N.E.	N.E.	N.E.	Prof. Caswell's Meteorological Journal.
8.	Norwich, Ct.	N.E.	N.E.	N.E.	Norwich Courier.
9.	Culloden Point, L. I.... ...	" change to	N.W. at noon."		Capt. Green's account, as published by Mr. Espy.
10.	Boston, Ms.	N.E. at sunrise. E. by N., A.M.	E.N.E. E.byN. } (E. by N. ½ N.)	E.S.E. at sunset. E. by N. P.M.	Wm. Cranch Bond's Meteor. Journal. Robert Treat Paine.
11.	Gloucester, Cape Ann, Ms.	E.S.E.	E.S.E.	E.S.E.	Letter from Gloucester in the Boston Newspapers.
12.	Salem, Ms.	Eastward.	Eastward.	Eastward.	Salem Gazette.
13.	Waltham, Ms.	N.E.	(E.N.E.)	E.	Monthly Met. Journal by C. F., published in the Boston Daily Sentinel.
14.	Worcester, Ms.	N.E.	N.E.	N.E.	Met. Journal kept at the State Lunatic Hospital, published in the National Ægis.
15.	Nashua, N. H.	N.E.	N.E.	N.E.	Nashua Telegraph.
16.	Middletown, Ct.	N. N.	(N. by E.)	N. N.N.E.	Reported by Prof. Smith. Dr. Barratt's Met. Journal.
17.	Ship Morrison, lat. 39°·35 long. 71°·50	S.E. W.N.W.	W.N.W.	W.N.W.	Ship's Log-book, and testimony of Capt. Benson and his Officers.
18.	Portsmouth, N. H.	E.	(E.)	E.	Weekly Met. Journal, published at Portsmouth.

19.	Northampton, Ms. ... Amherst, Ms.	N.E. N. by W.	(N.N.E.)	N.E. N. by W.	W. Atwill and others, from Mr. Atwill. Prof. Snell's Met. Journal, from Professor Hitchcock. Approximate mean assumed N.N.E.
20.	New Haven, Ct.	N. by W. N.N.E.	(Mean N. 3°. E.)	N.N.W. N.N.E. till 1½ P.M.	Report of Capt. Woolsey, Steamer Providence. Judge Darling's Met. Journal.
21.	Concord, N. H.	North-easterly.	N.E.	N.E. & more Northly.	Letter from Concord to S. G. Arnold, from Mr. [Arnold.
22.	Keene, N. H.	N.E.	N.E.	N.E.	Rev. Z. S. Barstow's Met. Journal.
23.	Litchfield, Ct.	Night of 14th, N.E.	(N.N.E.)	N. at night of 15th.	Litchfield Inquirer.
24.	Stratford, Ct...	N. by W.	N. by W.	N. by W.	Rev. J. R. Linsley's Met. Journal.
25.	Fire Island Beach, L. I. ...	At midnight. N.E., veered by N.	W.	N.W.	Captains Cartwright and Skiddy, employed at the Beach.
26.	Portland, Me.	N.E. at 11 E.	(E. 6° S.)	E. by S.	Met. Report of the Keeper of Marine Observatory, published at Portland.
27.	Kinderhook, N. Y.	N.E.	N.E.	N.E.	Silas Metcalf; Met. Journal.
28.	West Point, N. Y.	N.	N.	N.	Met. Journal of the Medical Officer.
29.	New York, City ... ... Fort Wood, N. Y. Harbour	N. by W.—N.N.W. N.	N.N.W. (N.N.W.)	N.W. by N. N.W.	Met. Journal of W. C. Redfield. Met. Journal of the Medical Officer.
30.	Flatbush, L. I.	N.	(N.N.W.)	N.W.	Rev. T. M. Strong; Met. Journal.
31.	Sandy Hook Bay, N. Y. ...	N.	N.W.	N.W.	Log-book of Bark Osceola, anchored in the Bay.
32.	Hanover, N. H.	N.E.	(N.E.)	N.	Prof. Young's Met. Journal, from Prof. Hubbard.
33.	Salem, Washington Co., N. Y.	N.E.	N.E.	N.E.	Wm. Brand and W. Larkin; Met. Journal.
34.	Albany, N. Y. ... ... Lausingburgh, N. Y. ...	N.E. N.N.E.	(N. 28°. E.)	N.E. N.	T. R. Beck, M.D.; Met. Journal. E. T. Foote; Meteorological Journ. Mean assumed N. 28°. E.
35.	Kingston, N. Y.	N.E.	N.E.	N.E.	Isaac Blauvelt; Met. Journal.
36.	Goshen, N. Y.	N.E.	(N.N.E.)	N.	Nathaniel Webb and Dr. John S. Crane; Met. Journal.
37.	Bark Ann Louisa, off Absecund, N. J.	W.N.W.	N.W.	N.W.	Ship's Log-book, and testimony of Capt. Wilson.
38.	Hartwick, Otsego Co., N. Y.	N.E.	N.E.	N.E.	Rev. Geo. B. Miller and L. Sternberg; Met. Journal.
39.	Oxford, Chenango Co., N. Y.	N.	N.	N.	Merritt G. M'Koow; Met. Journal.
40.	Cape May, N. J.	N.W.	N.W.	N.W.	Marine Reports, and Letter in the Philadelphia Newspapers.

New York, July 6, 1840.

* These correspond to the same numbers on the map.

VI. *Remarks relating to the Tornado which visited New Brunswick in the State of New Jersey, June* 19, 1835, *with a Plan and Schedule of the Prostrations observed on a Section of its Track. By* W. C. Redfield, *Esq*[*].

[Illustrated by Plate II.]

IN a paper printed in the American Journal of Science, in which I referred to the support given by Prof. Bache to Mr. Espy's theory of storms, at the meeting of the British Association in 1838, founded upon observations made on the New Brunswick tornado, I have stated, that in my own examinations I had observed numerous facts which appear to demonstrate the *whirling* character of this tornado, as well as the *inward* tendency of the whirling vortex at the surface of the ground; and further, that the direction of rotation was *towards the left*, as in the North Atlantic hurricanes[†]. It was due to Prof. Bache that my observations should be brought forward, a task which has been too long delayed, partly from a desire that he would revise his former conclusions. The facts now presented form part of the evidence to which I then alluded.

If the effects which I present for consideration be due to "a moving column of rarefied air without any whirling motion at or near the surface of the ground," as maintained by Prof. Bache[‡], we might expect to find a relative uniformity in these effects, on the two opposite sides or margins of the track. How far this is the case, may be seen by inspecting the observations which are found upon the annexed figure. (Plate II.)

The occurrence of these tornadoes appears to have been noticed from the earliest antiquity; and their violence has been considered as the effect of an active whirling motion in the body of the tornado; this peculiarity of action having often been supported by the testimony of eye-witnesses.

The whirling motion, however, has not been recognized by Prof. Bache, Mr. Espy [§], or Prof. Walter R. Johnson [||], in their several accounts of the New Brunswick tornado; these writers having been led to adopt or favour a theory of ascending columns in the atmosphere, founded on the supposed in-

---

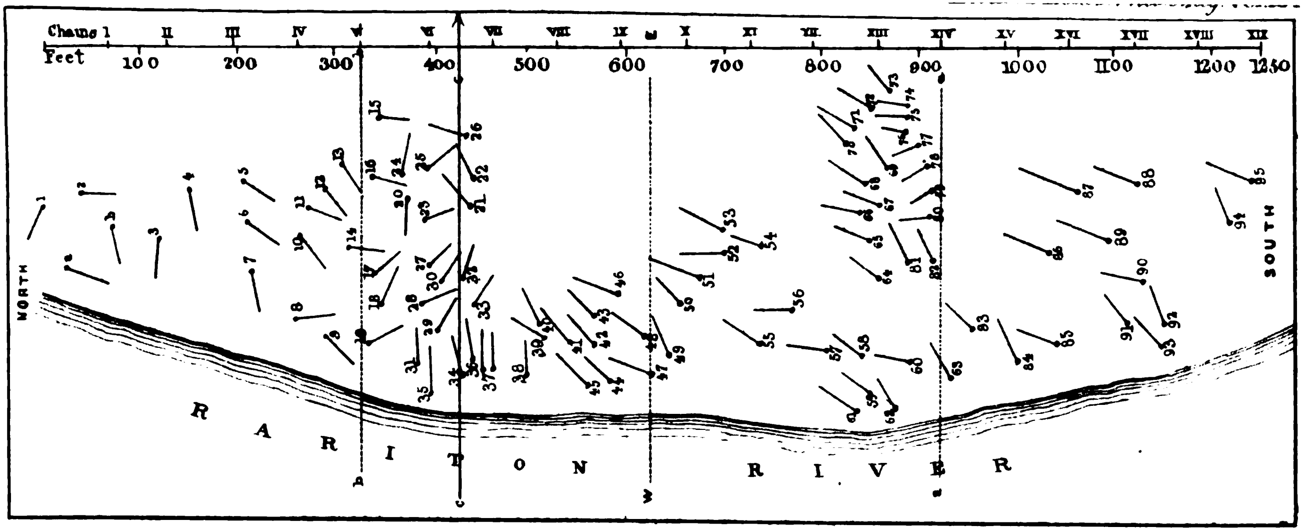
[*] [Communicated by Sir John F. W. Herschel, Bart.] This paper was intended by its author to have been read at the late meeting of the British Association in Glasgow, but was unfortunately detained.—J. F. W. H.

[†] Amer. Journ. of Science, Oct. 1838, vol. xxxv. pp. 206, 207.

[‡] Transactions of Amer. Phil. Society, vol. v. p. 417. New Series.

[§] Trans. Amer. Phil. Society, vol. v. New Series.

[||] Journ. Academy Nat. Sciences of Philadelphia, vol. vii. Part ii.



Sketch of the Prostrations caused by the Tornado of June 19th 1835 in the belt of Wood on the east bank of the Rariton, opposite the City of New Brunswick, in New Jersey: by W. E Redfield.

NOTE *The bank of the river is here covered by an irregular belt of Wood: immediately east of which is a clear field, the western margin of which is equally irregular in its outline.*

⟶ *Represents the course of the apparent axis of the Tornado —* W *West.* E *East.*

The dots show the root end of the trees, part of the roots, in all the cases, being fast in the ground.

Course of the Tornado, East.

J Barre lith.

fluence of calorific expansion accompanying the condensation of vapour.

It is remarkable that up to this period the evidences of the rotation, or other characteristic action of tornadoes, appear not to have been recorded, nor to have received the distinct consideration of scientific observers. We are, therefore, left to seek out the peculiarities of their action, by examining the direction of the prostrations and other effects of the wind: and from a careful induction from the effects which are thus registered as by the finger of the tornado, we may hope to arrive at satisfactory conclusions.

If the numerous prostrations of trees and other objects, which may be observed in the path of a tornado, be the effects of a violent whirlwind, it appears most reasonable to infer that this whirl had the common property which may be observed in all narrow and violent vortices, viz. *a spirally involute motion*, quickened in its gyrations as it approaches toward the centre or axis of the whirl, and there continued (in the case of the whirlwind) spirally upward, but gradually expanding again in its spiral course in ascending towards the extreme height of the revolving mass.

If we now contemplate the action of this whirling body, while in a state of rapid progression, on the several objects found in distinct portions of its path, we may expect to witness effects of much complexity, particularly as regards the lines of direction; and, also, that amid this apparent complexity some clue may be obtained that will serve to indicate or establish the true character of its action. Some of the effects which may be expected, or observed, will be here considered.

1. We may expect to find in the path of the whirlwind, strong evidence of the inward or vorticular course of the wind: the violence of which inward motion is clearly indicated by the force with which various objects, often of much weight, are carried spirally upward about the axis of the revolving body.

Now the effects of this inward vorticular motion at the surface of the ground, are clearly manifested in the cases before us; and are also well illustrated by Prof. Bache in his paper on this tornado, although referred by him to a different action *.

2. As the effects observed at various points in the track were produced at different moments of time and by forces acting in different directions, as well as of various intensities,

---

* Transactions of American Philosophical Society, vol. v.

we may expect to find great diversities in the several directions of the fallen trees and other prostrated bodies: and further, as all the forces, in addition to their inward tendency, have likewise a common tendency in the direction pursued by the tornado, we may expect to find, also, full evidence of this progressive force in the direction of the fallen bodies.

These effects, I need hardly state, are distinctly observed in the case before us; and appear likewise from the observations of Prof. Bache. The results already noticed have been observed also in the tracks of other tornadoes; so that a general inclination, both inward and onward, amid the various and confused directions of the fallen bodies, is distinctly recognized by all parties to this inquiry.

3. It has been often noticed, that where two fallen trees are found lying across each other, the uppermost or last fallen points most nearly to the course pursued by the tornado.

In view of the facts above stated, much pains have been taken to establish, by induction, a central and non-whirling course in the wind of the tornado; first inward and then upward, like that resulting from a common fire in the open air. I do not propose to notice the insuperable difficulties which appear to attend this hypothesis. It is important to state, however, that all the above-mentioned effects, when theoretically considered, are, at least, equally consistent with the involute whirling action of an advancing vortex. This important consideration I have not seen recognized by the advocates of the non-whirling theory; and it seems proper, therefore, to point out, as we proceed, other and more distinguishing effects of the whirling action.

4. It has been noticed, also, that the directions given to broken limbs and other bodies, by the successive changes in the direction of the wind as the tornado passed over, have been found in opposite courses, on the two opposite margins of the track.

This fact, too, has been strongly urged as disproving a rotary motion. But, unfortunately for the objection, this effect accords fully with the rotary action of a progressive mass of atmosphere; as is well known to all who understand the *theory* of rotary storms.

In all such whirling masses the successive changes in the direction of the wind, which result solely from their progressive motion, necessarily take place in opposite directions or courses, on the two opposite sides of the advancing axis. This indication fails, therefore, as a theoretic test: and I now proceed to notice others, which are peculiar to a progressive whirling action.

5. In considering further the effects of such action, we may expect to find that the increased activity of gyration which is observed near the centre of a vortex, will be indicated by a more violent and irregular action in and near the path pursued by the axis of the whirlwind, than is found under its more outward portions.

This effect is often strikingly exhibited in the path of tornadoes; while, in the supposed ascent of a non-whirling column, it would seem that no part of the surface would be so much exempted from its action as that lying near its centre.

6. As the effect of rotation must be to produce, on one side of the advancing axis, a reversed motion which is contrary to the course of the tornado, it is evident that on this side the prostrating power will be much lessened; that the cases of prostration, therefore, will be less numerous; and that some of these, at least, will be produced in a backward direction, more or less opposite to the course of the tornado. By this criterion, not only the whirling movement, but the direction of the rotation also may be clearly ascertained.

This effect is best observed by comparing the two opposite margins of the track; and is strongly exemplified in the case before us. Here we find, that most of the trees prostrated within five chains (110 yards) from the northern or left-hand margin of the track, lie in directions which are more or less backward from the course of the tornado. The prostrations in this part of the track are also for the most part less general than on the opposite side of the axis *, a greater portion of the trees being left standing.

It sometimes happens, owing perhaps to the inward or involute motion having exceeded the progressive motion at a particular point, that some inclination backward will be found in the prostrations on the progressive side of the whirl, as seen on the sketch, Nos. 77 to 80. But these unfrequent cases by no means compare with the numerous backward and sometimes *outward* prostrations, found on the reverse side of the whirl, as illustrated by Nos. 1, 3, 4, 7, 9, 10, 12, 13, &c. on the left side of the track.

Thus we find here a satisfactory indication that this tornado was a whirlwind; and that the course of its rotation was to the *left* in front.

7. It is also apparent, that the prostrating power of a whirlwind on the side of its reversed motion as just consi-

---

* There was a vacant space in the belt of wood, immediately to the right of the line *c c* or axis of the tornado, owing to which the effect mentioned does not appear so obvious in the figure.

dered, will be limited to a shorter distance than on the opposite or progressive side of its axis.

This is seen in the more limited *extent* of the prostrations on the north or left margin of the track, as compared with the extent of those which incline inward on the right side of the apparent axis.—There were many trees standing beyond the northern border of the track, but none had fallen.

8. It follows, in like manner, that on that side of a whirlwind in which the rotary motion coincides with the progressive movement, the prostrating power will not only be increased in its intensity, but will also be effective over a wider space; and that few, if any, of the prostrated bodies will be found to have been thrown backward.

In the case before us, as may be seen in the sketch, the prostrations are found to extend on the southern or right side of the apparent axis to a distance nearly twice as great as on the left side. The same general result has also been noticed in the tracks of other tornadoes which I have examined.

The facts here considered are too important to be overlooked, and seem fully to establish the course of rotation.

9. If a rotative action be exhibited, the mean directions of all the prostrations, on each of the two opposite sides, will differ greatly in their respective inclinations to the line of progress, and the mean direction of those on the reverse side will be found more backward than on the opposite side, where the rotative course coincides with the progressive action.

In the case before us, the mean direction of all the prostrations on the right side of the track is found to incline fifty-seven degrees inward from the line of progress. The course of the tornado is here taken to be east; although for the last half mile its course had been a little north of east. On the left side, the mean direction is found to be S. 6° W., or ninety-six degrees inward and backward; a difference in the mean inclination from the course on the two sides of forty-seven degrees*.

If we now take the indications afforded by the two exterior portions of the track, to the width of five chains on each side, where the effects are more distinctive in their character, we find on the right side a mean inward inclination of forty-nine degrees; the mean direction being N. 41° E.: while on the left side of the track the mean inclination is not only inward, but forty-seven degrees backward; the mean direction on this side being S. 47° West. We have thus a mean difference in the inclination of the fallen trees on the two exterior portions of the track, of no less than eighty-eight degrees. These indications seem conclusive also in favour of the whirling action in the direction from right to left.

---

* The inclinations of the fallen trees from the course, on both sides the axis, are reckoned inward and backward.

10. Although of less importance, it should be mentioned that the diminished action of the tornado which is commonly observed on the hill sides and summits over which it passes, and the greatly increased action in the bottoms of the valleys and even in deep ravines, afford a strong argument against ascribing the effects to the ascent of a non-whirling rarefied column; as the latter, it would seem, must act with greater force on the hill sides and summits than in the bottoms of valleys. The general correctness of the observation here stated cannot justly be questioned.

11. The sudden and extraordinary diminution of the atmospheric pressure which is said to take place at the points successively passed over by the tornado, causing the doors and windows of buildings to burst outwards, seems to afford strong confirmation of a violent whirling motion, for an effect of this kind is necessarily due to the centrifugal and upward force of the vorticular action. There are no other means known by which such an abstraction of pressure can be effected in the open air. An increase of calorific elasticity, if such were produced, either generally or locally, would not greatly disturb the equilibrium of pressure, being resisted by the surrounding and incumbent weight of the entire atmosphere. Besides, the immediate effect of such increased elasticity might rather be to burst *inward* the windows and doors of buildings exposed to its action.

Some of the more important indications mentioned above, appear also from Prof. Bache's observations; although the latter are not definitely located by him, as regards the extreme borders of the track. Thus, in figure 7 of Prof. Bache's paper, assuming the course of the tornado to be east, and rejecting a few observations near the centre, to avoid error, we find in twenty observations on the right side of the track, a mean inward inclination of sixty-four degrees, and for nine observations on the left side, a mean inclination, reckoned inward and backward from the course, of one hundred and four degrees, being fourteen degrees backward.

It is stated by Prof. Bache, "that the trees lying perpendicular to the track of the storm, are not those furthest from the centre of that track." This generalization accords with my own observations; but can hardly be reconciled with an inward non-whirling motion in the tornado.

It may appear to some that in the case of a whirlwind the greater portion of the prostrations on the reverse side of the axis should be found in a backward direction: and so they would undoubtedly be found, were it not for the inward and the progressive action. But the force is here so far lessened by the reverse action above noticed, that in most cases only a small portion of the trees exposed will be thus prostrated: while the greatest force of the whirlwind, on this side, is felt near its last or closing portion and towards the apparent axis, where the inward, together with the rotative and progressive forces, seem to combine their influence in the closing rush towards the heart of the receding vortex. This appears to account for the nearly opposite directions of prostration found on this side, and it is apparently by this more violent closing action, that many trees which were first overthrown in a direction nearly across the centre of the path, were again overturned inversely, and often carried forward nearly in the course of the tornado. It is proper to remark here, that an attentive examination of these effects has served to convince me that on the right and more central portions of the track the prostrations for the most part take place either at the outset or under the middle portions of the whirlwind, while on the left or reverse side, up to the line of the apparent axis, and sometimes across the latter, they occur chiefly under the closing action of the whirl, as above described. The violent effects of the latter are more clearly seen as we advance from the left-hand margin towards the centre or apparent axis of the path.

From the causes to which I have just alluded, aided by the elevating forces about the axis of the tornado, the effects are usually more violent on and near the line passed over by the axis, than in other portions of the track. This line of greatest violence is found to coincide nearly with the line which separates the inwardly inclined prostrations of the two opposite sides of the track*. The latter line or apparent axis of the track is sometimes called the line of convergence, and is indicated on the figure by the line and arrow *c c*. Along this line, from the causes just mentioned, aided also by the elevating forces about the axis, many of the trees are carried forward and left with their tops in a direction nearly parallel to the course of the tornado; forming an apparent, but not a just exception, to the more lateral direction which pertains to most of the trees prostrated by the onset of the whirlwind, near the central portions of the track. Indeed, the central or closing violence of the advancing whirl is here so great, that the trees are not unfrequently torn out of the ground and carried onward to considerable distances.

---

* The line of greatest violence, for the most part, is found somewhat to the right of the line of convergence.

It is proper to state here, that in the tracks of all the tornadoes which I have had opportunity to examine, and in some, at least, of those examined by others, the course of rotation has been found the same as in the case before us*.

In order to make a just and satisfactory examination of the effects of a tornado, it appears necessary to select portions of the track where the extension of wood or single trees, on each side, is found sufficient to mark clearly the exterior limits of the prostrating power, and where the effects on both sides of the axis are also clearly developed. Our next care should be to ascertain, as near as may be practicable, the line which separates the opposite convergence of the two sides, noticed above as the axis or line of convergence. We should then determine the general direction of this line and of the track at the place examined; which being done, we may proceed to measure the distance to which the prostrations are extended on each side, and then carefully to take the position and direction of prostration of each and all of the fallen bodies, noting with care, also, any other phænomena which may serve to aid our inquiries. We may thus obtain valuable materials for future analysis; and this course of investigation, if faithfully pursued, will, it is believed, remove all reasonable doubt of the rotative action of these tornadoes. An examination of their probable origin and the causes of their enduring activity and violence, belongs not to the present ocasion.

New York, July 20, 1840.

Schedule of Observations on the Directions of the Trees prostrated by the Tornado of June 19, 1835, *near New Brunswick, N. J. Course of the Tornado, East.*

No.	Direction of Prostration.	Inclination inward and backward.
	Table I. *Left division of the track to the line* b b: *width five chains, or* 110 *yards.*	
a.	S. 20° W.	110
b.	S. 80 W.	170
1.	N. 67 W.	157
2.	South.	90
3.	West.	180
4.	S. 80°,W.	170
5.	S. 40 W.	130
6.	S. 40 W.	130
7.	S. 80 W.	170
8.	S. 10 E.	100
9.	S. 50 W.	140
10.	S. 50 W.	140

---

* As in the tornado which passed through Allegany county, New York, July 25, 1838, described by Mr. Gaylord in the American Journal of Science, vol. xxxvii. p. 92.

No.	Direction of Prostration.	Inclination inward and backward.
11.	S. 26° W.	116°
12.	S. 50 W.	140
13.	S. 65 W.	155
14.	South.	90
	16 cases: mean direction S. 47° West, being 47 degrees backward, or 137 degrees inward and backward.	
	Table II. *Left of the centre or axis, from* b b *to* c c: *width one and a half chain, including also* 32 *and* 33, *and excluding* 34 *and* 35.	
15.	S. 2° E.	88
16.	S. 12 W.	102
17.	S. 35 E.	55
18.	S. 62 E.	28
19.	S. 25 E.	65
20.	N. 80 W.	190
23.	S. 20 E.	70
24.	S. 80 E.	10
25.	S. 45 E.	45
27.	S. 45 E.	45
28.	S. 20 E.	70
29.	S. 60 E.	30
30.	S. 60 E.	30
31.	East.	00
32.	S. 75 E.	15
33.	S. 56 E.	34
	16 cases: mean direction S. 35° E., being 55 degrees inward.	
	Mean of two divisions on the left side, 32 cases, S. 6° W., being 6 degrees backward or 96 degrees inward and backward.	
	Table III. *Doubtful or neutral cases; belonging to either side.*	
35.	East.	00
36.	N. 85° E.	05
37.	East, two cases	00
	4 cases: mean direction E. 1° N.	
	Table IV. *On the right of the axis from* c c *to* w e: *width three chains, including* 34, *and excluding* 32, 33, 36, *and* 37.	
21.	N. 56° E.	34°
22.	N. 60 E.	30
26.	N. 10 E.	80
34.	N. 80 E.	10
38.	East	00
39.	N. 30 E.	60
40.	N. 70 E.	20
41.	N. 55 E.	35
42.	N. 50 E.	40
43.	N. 78 E.	12
44.	N. 45 E.	45
45.	N. 45 E.	45
46.	N. 25 E.	65
47.	N. 35 E.	55
48	N. 40 E.	50
	15 cases: mean direction N. 51° E., being 39 degrees inward from course.	
	Table V. *On the right of axis from* w e *to* a a: *width four and a half chains.*	
49.	N. 67° E.	23
50.	N. 45 E.	45
51.	N. 22 E.	68
52.	N. 3 E.	87
53.	N. 30 E.	60
54.	N. 10 E.	80
55.	N. 35 E.	55
56.	North	90
57.	N. 10° E.	80
58.	N. 3 E.	87
59.	N. 45 E.	45
60.	N. 10 E.	80
61.	N. 35 E.	55
62.	N. 60 E.	30
64.	N. 40 E.	50
65.	N. 20 E.	70
66.	N. 10 E.	80
67.	N. 20 E.	70
68.	N. 40 E.	50
69.	N. 70 E.	20

No.	Direction of Prostration.	Inclination inward and backward.	No.	Direction of Prostration.	Inclination inward and backward.
70.	N. 50° E.	40°	84.	N. 40° E.	50
71.	N. 35 E.	55	85.	N. 70 E.	20
72.	N. 30 E.	60	86.	N. 23 E.	67
73.	N. 50 E.	40	87.	N. 31 E.	59
74.	North (two)	90	88.	N. 20 E.	70
75.	North	90	89.	N. 22 E.	68
76.	North	90	90.	N. 10 E.	80
77.	N. 20° W. (clump)	110	91.	N. 55 E.	35
78.	N. 35 W.	125	92.	N. 70 E.	20
79.	N. 30 W.	120	93.	N. 55 E.	35
80.	N. 10 W.	100	94.	N. 68 E.	22
81.	N. 65 E.	25	95.	N. 25 E.	65
82.	N. 70 E.	20		14 cases: mean direction, N. 41° E., being 49 degrees inward.	
	33 cases: mean direction, N. 24° E., being 66 degrees inward.				
	Table VI. *Right division of the track from* a a *to the south margin.*			Mean direction of three divisions on the right of axis; 62 cases: No. 33½° East, being an inward inclination of 57 degrees, nearly.	
63.	N. 35° E.	55			
83.	N. 45 E.	45			

VII. *On a new Compound of Chlorine and Cyanogen.*

By James Stenhouse, *Esq.*[*]

THIS somewhat remarkable compound may be obtained by two processes, both of which I shall now minutely describe. The first way in which I obtained it was by decomposing an alcoholic solution of bicyanuret of mercury by dry chlorine gas. The method of proceeding is the following. Four or five ounces of bicyanuret of mercury should be finely powdered, and introduced into a tubulated retort, and the same weight of strong alcohol poured over them. It is proper to agitate the mixture for some time, and even to heat it a little, in order to saturate the alcohol completely with the salt. The retort is then to be kept as cool as possible by being placed in a vessel of cold water, a copious stream of which is to be kept falling upon it during the whole process. The chlorine to be introduced into the solution of the bicyanuret of mercury must be dried, by being passed through an intermediate vessel containing sulphuric acid: it may then be conveniently introduced into the solution by a tube passed through the tubulure of the retort, and reaching nearly to its bottom. The current of chlorine is to be sent through the liquid very slowly: if this is not attended to, the temperature rises very high, and the gaseous chloride of cyanogen passes off as fast as it is formed, instead of being absorbed by the alcohol, and the quantity of the compound obtained is exceedingly diminished, if indeed its formation be not wholly prevented. When this is the case heavy muriatic æther is almost the only product; but by careful cooling and cautious evolution of the chlorine this result may be easily prevented.

---

[*] Communicated by the Author; having appeared in Liebig's *Annalen* for January, 1840.

When the current of chlorine has been sent through for some time, abundance of crystals begin to appear in the retort, accompanied by a violent effervescence. These crystals will be found to be sal-ammoniac, the quantity of which, when there was not much alcohol in the retort, is so great as to convert the whole into a solid mass. If the chlorine is continued to be sent through the liquid after the crystals of sal-ammoniac have appeared, it forms abundance of the heavy muriatic æther, which adheres tenaciously to the chlorocyanous compound, and from which it can be separated only by repeatedly dissolving it in hot alcohol, and precipitating it by water. If any trace of this æther remains adhering to the crystals, it communicates to them its peculiar smell and greasy feel, and lowers their melting point very considerably.

If the alcoholic solution is then treated with water, the sal-ammoniac is dissolved, and the cyanogen compound falls in great abundance in long silver-white needles. If this is done by hot water the crystals form more slowly, and become therefore larger and more beautiful. When the liquid has stood some time the crystals are to be collected and washed upon a filter with cold distilled water, till every trace of acid is removed; they are then quite pure. The salt which remains in solution after the crystallization of the cyanogen compound is not corrosive sublimate, as might be expected; it is the combination of chloride of mercury and sal-ammoniac usually known as the sal-d'Alembroth; it is much more soluble than corrosive sublimate.

I shall now describe the second method, which is much more œconomical.

Strong hydrocyanic acid is first made in the usual way by sulphuric acid and prussiate of potash. It is then to be redistilled, and to be condensed in the alcohol intended to be used, until the latter is saturated with the acid. The other arrangements are precisely the same as in the first process; and the too rapid evolution of chlorine, or the heating of the solution, must be carefully guarded against. The chlorine is continued to be sent through the liquid till the crystals of sal-ammoniac begin to form, which is accompanied by the violent effervescence I have before mentioned, owing to the escape of carbonic acid gas.

The cause of these phænomena is owing to the decomposition of chloride of cyanogen, by its constituents uniting with the elements of four equivalents of water, and giving rise to sal-ammoniac and carbonic acid, as will be readily seen from the following symbols.

$$\begin{array}{c|c} Cl_2\ N_2 & C_2 \\ H_8 & O_4 \end{array}$$

or $C_2\ N_2\ Cl_2 + H_8\ O_4 = C_2\ O_4 + N_2\ H_8\ Cl_2$.

Properties of the Compound.

The compound crystallizes in long, soft, perfectly white needles, of a silvery lustre; they very much resemble sulphate of quinine: it is neutral to test paper; it is tasteless and inodorous; it melts at 140° C, by which heat it is partly sublimed. When heated to 160° C. it is decomposed, and emits a smell resembling that of benzoic æther. It burns easily with a large yellow flame, resembling that of alcohol, and emits no smoke. It is little soluble in cold water, but so much so in boiling as to be deposited in crystals on the cooling of the liquid. In alcohol and æther it is very soluble, but it may be precipitated from either solvent by water: it is deposited in crystals on their evaporation.

When heated with an aqueous solution of potash it is decomposed with the evolution of ammonia, and the solution becomes of a deep brown colour. With a solution of liquid ammonia in the cold there is no action, but it is dissolved on the application of heat, and is deposited unaltered on the cooling of the liquid. Sulphuric acid dissolves it very readily when assisted by a gentle heat; it is not in the least blackened; it does not precipitate on the cooling of the liquid, but water causes the precipitation of it in an apparently unaltered state.

The analyses conducted in the usual way gave the following results.

1st. 0·7902 of the combination gave 1·022 carbonic acid and 0·360 water.

2nd. 0·505 gave 0·658 carbonic acid and 0·227 water.

1st. 0·7925 gave by decomposition at red heat by quick lime 0·833 chloride of silver, equal to 25·930 per cent. chlorine.

2nd. 0·5245 gave 0·555 chloride of silver equal to 26·10 per cent. chlorine.

The nitrogen was determined qualitatively. The carbonic acid was to the nitrogen in the proportion of 8 :: 1 by volume, accordingly 8 :: 2 in atomic proportion.

This gives the following result.

	1.	2.
Carbon	35·760	36·02
Hydrogen	5·038	4·99
Nitrogen	10·350	10·43
Chlorine	25·930	26·10
Oxygen	22·922	22·46
	100·000	100·00

Theoretical composition.

		In 100 parts.
16 at. Carbon	1222·96	35·60
28 at. Hydrogen	174·71	5·08
4 at. Nitrogen	354·08	10·30
4 at. Chlorine	885·30	25·75
8 at. Oxygen	800·00	23·27
	3437·05	100·00

From this formula Prof. Liebig has shown that it may be considered as a combination of 3 atoms aldehyd, 2 atoms chlorcyan, and 5 atoms of water, as follows.

3 at. Aldehyde = $C_{12} H_{18} O_3$

2 at. Chloride of cyanogen = $C_4 N_4 Cl_4$

5 at. Water = $H_{10} O_5$

1 at. of the combination $C_{16} H_{28} O_8 N_4 Cl_4$

VIII. *On the Composition of Chelidonin and Jervin.* By Dr. Heinrich Will[*].

THE organic bases employed in the following analyses were prepared and sent to me to be analysed by their discoverers; the chelidonin by Dr. Probst of Heidelberg, and the jervin by E. Simon, apothecary in Berlin.

Chelidonin[†].—The chelidonin, whose properties and mode of preparation have been described by Probst, forms a pure white, soft powder, and is also obtained in a crystalline form. When heated it melts into a colourless oily fluid, which becomes brown at a stronger heat; finally it takes fire and burns away with a bright smoky flame, without leaving the least residue. The base dried in the air contains water of crystallization which disappears completely at a temperature of 100 centigrade; if heated then till it melts, which it does at 130 cent., it loses no more weight. It is soluble in alcohol and æther, but not in water; a good deal of ammonia is developed when it is melted with caustic potash. A solution of chelidonin in diluted muriatic acid, or the watery solution of the neutral muriate itself, gives with chloride of platinum a beautifully yellow precipitate, at first flaky, but afterwards granular, which may be washed out without decomposition. This precipitate, which is a double salt of muriate of chelidonin with chloride of platinum, was used for determining the atomic weight of chelidonin. This double salt was not decomposed by boiling with nitric acid.

---

[*] From Liebig's *Annalen der Pharmacie* for July, 1840: vol. xxxv. part 1.

[†] See the Supplementary Number for the present month, vol. xvii. p. 543.

First. 0·448 of chelidonin dried in the air lost at the temperature of 100° centigrade, 0·023 of water = 5·13 per cent.

Second. At 130°, when the chelidonin was melted, 0·172 lost 0·008 of water = 4·65 per cent.; the mean of both experiments is 4·89 per cent.

1. Chelidonin weighing 0·425 dried at 100°, gave 1·062 of carbonic acid, and 0·215 of water.

2. 0·2585 gave 0·643 of carbonic acid, and 0·1315 of water.

3. 0·321 gave 0·793 of carbonic acid, and 0·162 of water.

In a direct nitrogen determination 0·2995 yielded 30 cubic centimeters of nitrogen gas at 11° cent., and height of the barometer 27″ 10‴, which after reduction to standard temperature and pressure gave 2·88 cub. cent. = 12·19 per cent. Hence the following per cent. composition is calculated:

	1.	2.	
Carbon ……	69·07	68·76	68·30
Hydrogen …	5·62	5·65	5·60
Nitrogen……	12·19	…	…
Oxygen ……	13·12		
	100·00		

1. 0·307 of chelidonin-chloride of platinum dried at 100° left when burnt 0·0535 of metallic platinum = 17·42 per cent.

2. 0·3125 gave 0·055 of metallic platinum = 17·60 per cent.

From these two determinations the numbers 7076·8 and 7008·5, come out as the atomic weight of the double salt, and therefore for the chelidonin 4502·89 and 4434·9, which determination, particularly the last, agrees as well with the following formula as can be expected from such weighing, where a difference of half a millegramme increases or diminishes the atomic weight 70 or 80 parts.

The foregoing determination of the water of crystallization gives exactly 2 atoms of water to 1 atom of chelidonin.

The theoretical composition of the chelidonin, in the calculation of which the hydrogen was taken as agreeing very near, perhaps too near with what was found by experiment, is the following:

		In 100 parts.
40 at Carbon ………	3057·40	68·90
40 at Hydrogen ……	249·59	5·62
6 at Nitrogen ………	531·12	11·97
6 at Oxygen ………	600·00	13·51
1 atom of anhydrous chelidonin =	4438·11	100·00
2 atoms of water … … … …	224·96	4·82

1 atom of crystallized chelidonin =4663·07.

The chelidonin-chloride of platinum consists of 1 equivalent of muriate of chelidonin.

	Calculated.		Found.	
			1.	2.
	4893·24	69·78	70·08	69·70
1 of chloride of platinum …	2118·80	30·22	29·92	30·21
	7012·04	100·00	100·00	100·00

or

		In 100 parts.	Obtained.	
			1.	2.
1 equivalent of chelidonin	4438·11	63·29		
1 do. platinum	1233·50	17·59	17·42	17·60
3 do. chlorine	1327·95	18·94	…	…
1 do. hydrogen	12·48	0·18	…	…
	7012·04	10·000		

The composition of the chelidonin stands in remarkably close relation to that of narcotin, if we retain for the latter as the truer the older formula given by Liebig:

Chelidonin is … … $C_{40}\,H_{40}\,N_6\,O_6$
Narcotin … … … $C_{40}\,H_{40}\,N_2\,O_{12}$

It is evident that in chelidonin with an equal number of atoms of carbon and hydrogen, the oxygen of the narcotin is wholly or in part replaced by nitrogen. Experiments to change one into the other remained without result. If we dissolve narcotin in a solution of ammonia in alcohol, and keep the mixture some time near boiling, it crystallizes again, on cooling, with its properties unchanged. If chelidonin is melted with caustic potash and a little water so that it is not perfectly decomposed, it may be obtained unchanged when neutralized with an acid and precipitated by ammonia. The small quantity of the bases did not permit me to make further experiments.

For comparison I place here the per cent. composition of narcotin and chelidonin near one another.

Carbon	68·90	65·27
Hydrogen ...	5·62	5·32
Nitrogen......	11·97	3·78
Oxygen	13·51	25·63

The plants which yield these two bases belong to the same family, the Papaveraceæ.

Jervin.—This vegetable base, the general properties of which and mode of preparation have already been described in vol. xxiv. p. 214 of the *Annalen der Pharmacie*, is obtained along with veratrin and sabadillen from the root of *Veratrum album*.

The analysed jervin was white and crystalline; when heated on a slip of platinum it melted to an oily fluid, almost as clear as water, which at a greater heat became brown, took fire, and was consumed with a smoky flame, but without leaving any residue. When melted with caustic potash ammonia is developed. It is almost insoluble in water, but dissolves in spirits of wine; with acetic acid it forms a salt soluble in water. The sulphate muriate and nitrate of jervin are scarcely soluble in water, and in mineral acids. The acetic solution of jervin is precipitated in copious flakes by the last-named acids and by ammonia, which redissolve only with difficulty, even in a great excess of the precipitant. Jervin bears in an oil-bath a temperature of 190° centigrade, without being decomposed, but above 200° it becomes brown and is decomposed. With chloride of platinum, jervin, like all organic bases, forms a double salt, which is best obtained by precipitating the acetic solution of jervin with an acid solution of chloride of platinum, or by replacing the alcohol solution of the acetate of jervin by chloride of platinum, and then evaporating it and washing the salt with water; the double salt precipitated from the acetate of jervin forms a beautifully bright yellow substance, easily washed without being decomposed, which was employed in the analysis given below.

1. 0·218 of jervin dried in the air lost at a temperature of 100°, 0·015 of water = 6·88 per cent.

2. 0·4575 heated to 130° lost 0·0315 of water = 6·88 per cent.

1. 0·370 jervin dried at 100° gave 0·319 water, and 1·0165 carbonic acid.

2. 0·203 gave 0·178 water and 0·555 carbonic acid; 0·398 at 17°·8 centigrade, and 27″ 6‴ of barometer gave 17·4 cubic centimeters of nitrogen gas = 16·11 centigrade, and at 0° centigrade, and 28 Rhenish inches of barometer.

This gives for the composition of jervin in 100 parts,

	I.	
Carbon	75·96	75·60
Hydrogen ...	9·57	9·74
Nitrogen......	5·38	5·38
Oxygen	9·09	9·28
	100·00	100·00

To find the atomic weight the platinum in the double salt was determined by burning. It melted, became black, and burned away with flame.

I. 0·1237 gave 0·018 of metallic platinum = 14·55 per cent. As the atomic weight we deduce for the double salt the number 8476·83; for the muriate of jervin 6358·03, and for the anhydrous base 5902·90.

II. 0·094 of the double salts gave 0·0135 of metallic platinum = 14·33 per cent. Hence the atomic weight of the double salts 8588·81; of the muriate of jervin = 6470·01, and of the jervin = 6014·88.

If we calculate after these data the formula of the jervin, we get the following theoretical composition:—

			In 100 parts.
60 at	Carbon	4586·10	76·41
90 —	Hydrogen	561·57	9·36
4 —	Nitrogen	354·08	5·89
5 —	Oxygen	500·00	8·35
1 at	Anhydrous jervin	= 6001·75	100·00
4 —	Water	= 449·92	6·97

1 at Crystallized jervin = 6451·67.

For the platinum double salt we get the following composition:—

	Calculated.		Obtained.	
			1.	2.
1 equ. muriate of jervin	6456·88	75·29	75·34	75·01
1 — chloride of platinum	2118·80	24·71	24·66	24·99
1 equ. muriate of platinum and jervin .... =	8575·68	100·00	100·00	100·00

or also

		Calculated.	Found.	
			1.	2.
1 equ. jervin . .	6001·75	69·98	——	——
3 — chlorine	1327·95	15·49	——	——
1 — platinum	1233·50	14·39	14·55	14·33
1 — hydrogen	12·48	0·14	——	——
1 equ. of the double salt	8575·63	100·00.		

Among all the organic bases hitherto analysed, there is none to which the jervin stands related as to its composition.

IX. *On some new and curious numerical Relations of the Solar System.* *By* S. M. Drach, *Esq.**

In every system of bodies circulating round a comparatively much greater one possessing a rotary motion, the stability of the orbital revolutions cannot be insured unless the secondaries be placed at distances from the primary greater than the limit of equilibrium between the gravitating and centrifugal forces of the latter; otherwise the secondaries would ultimately inevitably fall on the primary, causing great devastation to both. This limiting distance, so important an element in the solar and planetary systems, seems hitherto to have been overlooked, although the distances of the planets from the sun, and of the satellites from their primary, are connected with it in a remarkable manner, as the following investigation will show.

Denoting by m, r, p, q, the relative mass, equatorial radius, period of rotation, and surface gravity of a planet with respect to the earth; and putting $Q = \frac{\text{centrifugal force}}{\text{gravity}}$ at the surface of our planet, and q = the similar quantity at the surface of another, we have

$$q = \frac{r^3}{m p^2} . Q. \quad \ldots\ldots\ldots \quad (1.)$$

Supposing the planets spherical and homogeneous, and putting δ = relative density, (1.) becomes

$$q = \frac{1}{\delta p^2} . Q \quad \ldots\ldots\ldots\ldots \quad (2.)$$

The *maximum possible* equatorial radius of the limiting surface $= \sqrt[3]{\frac{1}{q}}$, and the corresponding polar one was found by Laplace $= \frac{2}{3}$ of the former; the planetary ellipticities are confined between $\frac{q}{2}$ and $\frac{5}{4}q + \frac{75}{14}q^2$, &c. According to M. Poisson (*Méc.* i. p. 367.), $Q = \frac{1}{288 \cdot 908}$.

---

* Communicated by the Author.

With data furnished by the Ladies' Diary for 1837, the following table was constructed:

St.	q^{-1}.	$q^{-\frac{1}{3}}$.	Eq. rad. in miles.	Rel. Pol. Rad.	Limits Ell^y.
☉	48205·39	36·3942	16153281	24·2628	$\frac{1}{38564}$ & $\frac{1}{96410}$
☿	758·935	9·121	13609	6·081	$\frac{1}{606}$ & $\frac{1}{1518}$
♀	282·604	6·562	25006	4·375	$\frac{1}{224}$ & $\frac{1}{565}$
♁	289·908	6·619	26225	4·413	$\frac{1}{231}$ & $\frac{1}{580}$
♂	201·684	5·864	12380	3·909	$\frac{1}{161}$ & $\frac{1}{403}$
♃	12·476	2·319	100027	1·546	$\frac{1}{11}$ & $\frac{1}{25}$
♄	5·497	1·765	72319	1·177	$\frac{1}{6}$ & $\frac{1}{11}$
♅	$63 \cdot 22p^2$	$3 \cdot 98p^{\frac{2}{3}}$		$2 \cdot 65p^{\frac{2}{3}}$	

This table suggests the following remarks:

1. The first four planets rotating nearly in equal times, q is for them nearly as the density inversely, and $\sqrt[3]{\frac{1}{q}} \propto \delta^{\frac{1}{3}}$.

2. With the single exception of Venus, the value of the relative radius appears to decrease as we recede from the sun according to some function of the distance.

3. The oft-named limiting distance of a planet (16 millions of miles): distance of the first planet Mercury (37 millions) : : 1 : 2 . 27 : : 1 : $2\frac{1}{3}$: : 3 : 4 + 3 nearly, which is probably the true reason of 3 and 4 being the *fundamental* numbers of Bode's law, viz.

$$4 \quad , \quad 4 + 2^i . 3 \quad \text{from } i = 0 \text{ to } i = 6.$$

4. These researches have evolved to me a simpler and more universal law, embracing the comets, and secondary systems

of our planetary world. The four periodic comets have for their semi-axes major (Bowdich, *Méc. Cél.*, vol. iii.).

Encke's	2·224346	Olbers's	17·7
Biela's	3·53683	Halley's	17·98705.

Putting the distance of Jupiter from the sun = 121, I find

Limit	3·946	= 2^2	Juno	62·09	= 8^2
Mer.	9·005	= 3^2	Cer.	64·38	
Venus	16·33	= 4^2	Pal.	64·51	
Earth	23·26	= 5^2	Bie.C.	82·28	= 9^2
Mars	35·45	= 6^2	Jup.	121·00	= 11^2
En. C.	51·74	= 7^2	Sat.	221·9	= 15^2
Vesta	54·94		Ol.C.	411·8	= 20^2
			Hal.C.	418·8	

Hers. 446·2 = 21^2.

The greatest deviation from the exact *square* = $\frac{1}{11}$. Hence the periodic times are nearly as the *cubes* of the natural numbers; and the squares of the times, as also the cubes of the distances, are expressible by the *sixth* powers of the series 1, 2, 3, 4, &c. very nearly.

For Jupiter's system (Baily, Ast. Tab.),

Limit	2·319	= 25·0	or 5^2
1st Sat.	6·049	65·2	8^2
2nd	9·623	103·6	10^2
3rd	15·350	165·4	13^2
4th	26·998	291·0	17^2.

For the Saturnian system (Baily), diam. ring = $\frac{38''\cdot42}{16''\cdot20}$ = 2·3716 diam. Sat.

Limit	1·765	= 26·5	or 5^2
Ring	2·372	35·4	6^2
1st Sat.	3·351	50·0	7^2
2nd	4·300	67·2	8^2
3rd	5·284	78·9	9^2
4th	6·819	101·7	10^2
5th	9·524	142·2	12^2
6th	22·081	329·6	18^2
7th	04·359	960·5	31^2.

For the Herschelian system (Baily).

Limit (unknown).

1st Sat.	13·120	= 96·0	or 10^2
2nd	17·022	124·3	11^2
3rd	19·845	145·2	12^2
4th	22·752	166·5	13^2
5th	45·507	333·0	18^2
6th	91·008	665·9	26^2.

The period of this planet's rotation being probably between 6^h and 12^h, $\sqrt[3]{\frac{1}{q}}$ is between 1·5 and 2·5, and the corresponding square is 4^2.

5. As every body *within* the limit *must* fall on the planet, we may suppose the planetary sphere to extend to the limit, and by Kepler's law, the rotation of a body at the limit of the

Sun	= $25^d\cdot78$	Saturn's limit	$8^h\cdot6472$
Earth	= $1^d\cdot0086$	Do. ring	$13^h\cdot4588$
Jupiter	= $10^h\cdot0824$	Mean of do.	$11^h\cdot052$

All of which are nearly coincident with the *actual* times of rotation, and from these last the true value of q and r may be found for every moon-attended planet.

6. The greater the number of satellites, the more is the danger that from some internal explosion or external shock a moon might be precipitated upon the surface of its primary; hence the number of satellites must have influenced the relative or actual limit. The relative distance from the limit to the surface of the star, is for the

Sun	=35·3942	Jupiter	=1·319
Earth	= 5·619	Saturn	=0·765.

And our planet having one moon, Jupiter 4, and Saturn 7,

$$1\cdot319 \times 4 = 5\cdot276,\ \frac{7}{4}(0\cdot765) = 1\cdot338,\ 0\cdot765 \times 7 = 5\cdot355,$$

the results being nearly = the terrestrial and joval distances. If this be correct, the sun must have $35\cdot394 \div 5\cdot619 = 6$ to 7 satellites (primary planets) capable of causing any *serious* damage, which is the exact number of the large or older planets.

7. If t denote the time, z the height above a planet's surface (radius = 1), the equations of motion give

$$\frac{d^2 z}{d t^2} = -\frac{g}{(1+z)^2} + g q (1+z) = 0 \text{ at limit.}$$

$$\text{Whence } \frac{d z^2}{d t^2} = \frac{2g}{1+z} + g q (\overline{1+z}^2 - 1) = g (3 q^{\frac{1}{3}} - q). \quad (3.)$$

At the limit where $q(1+z)^3 = 1$; g being the gravity in parts of the radius, eq. (3.) expresses the velocity to send a body up from the surface of the planet, or the velocity with which a body let fall from the limit will impinge on the surface; this is per second for the Sun 1,316,500 feet, Mercury 38,000 feet, Venus 16960, Earth 17397, Mars 7858, Jupiter 144,080, and Saturn 103,800 feet.

8. The axis of the planet Venus being inclined at the singularly low angle of 15° to its orbit, its torrid zone extends

to 75° latitude from its equator, and its frigid zone from the pole to 15° latitude, so that Venus's arctic circles are nearer the equator than its tropic, and it must, therefore, have very hot summers and very cold winters, mitigated, however, by the sun's rapid change of declination, and by its year being only two-thirds of ours. Mercury's torrid zone extends only to 7° latitude, its frigid zone commences at 83° latitude, so that 76° of latitude are in the temperate zone, which must diminish the burning heat at that planet very considerably*. The tropics of the other planets being at less than 30° latitude, cause no great difference between them and the earth in this respect.

9. The sun is the principal cause of the difference of mean annual temperature between our pole and equator; therefore as this action diminishes with the planet's distance from the sun, the further the planet, the more equable is its surface-temperature, the less is the elasticity of its atmosphere disturbed, and consequently the calmer is the atmosphere, so that the diminished heat and light is compensated by the greater purity and stillness of the atmosphere, allowing those agencies to be more effective.

London, Aug, 10, 1840. S. M. DRACH.

P.S. The sun's relative limit less unity = 35·3942, which divided by 7 (the number of planets capable of causing *serious* injury) gives 5·0563. Dividing this last number by 1, 4, and 7, we nearly get the limits (less 1) of the Earth, Jupiter, and Saturn. Applying this principle to Uranus, we have

♅ and ☉, 6 : 1 :: 5·056 : 0·843
⊕, 6 : 1 :: 5·619 : 0·936
♃, 6 : 4 :: 1·319 : 0·879
♄, 6 : 7 :: 0·765 : 0·892

whereof the mean — 0·887. Hence 1·887 is nearly the relative limit of ♅ = 3·984 $p^{\frac{2}{3}}$ $p = 0·32597$ sidereal day = 8 hours nearly, so that ♅ rotates on his axis in 7½ to 8 hours. Also $q = \frac{1}{6·7}$; his ellipticity is therefore between $\frac{1}{3}$ and $\frac{1}{13}$. But the Earth's and Jupiter's actual oblateness is nearly a mean between the limits; therefore from analogy the oblateness of ♅ = ⅓ nearly.

Sept. 4, 1840. S. M. D.

---

* This diminution would be materially assisted by an ocean, whose great evaporation would reduce and nearly equalize the temperature.

X. *On the surprising intensity of current of the Zinc-Iron-circuit, its causes, and some allied subjects.* *By Prof.* J. C. Poggendorff *.

THE author read before the Berlin Academy † of the recent discovery of Mr. Martyn J. Roberts,‡ that iron, combined with zinc and dilute sulphuric acid, produces a considerably more energetic current than, under like circumstances, the far more negative copper. He in the first place communicated some observations in confirmation and extension of this fact, both interesting in a scientific point of view and important for the praxis. He showed that the superiority of the current of the *zinc-iron circuit* does not merely occur on employing dilute sulphuric acid, but also with dilute nitric acid, solutions of caustic potash and common salt, and similar fluids; and indeed not solely in reference to the *zinc-copper circuit* (even one with double copper surface), but likewise in reference to circuits of *zinc* and *silver*, or *zinc* and *platina*. It was, in fact, necessary to give to the zinc-platina circuit plates three times as large as those of the zinc-iron circuit, if its current was to be of like intensity with that of the latter. On the other hand, it was found that a Daniell circuit, in which, as is well known, the copper is placed in a solution of the sulphate of copper, and the zinc separated by membrane, developes, in acid, the size and distance of the plates being the same, a greater intensity of current than the zinc-iron circuit; while, on the other hand, that a combination constructed after Daniell's circuit, namely, a circuit in which the iron is placed in a solution of the sulphate of the protoxide of iron, produces a current of very inconsiderable energy.

The author then passes on to the explanation of these phænomena, respecting which Mr. Roberts has offered no opinion, and which, indeed, according to the views at present entertained in England on galvanism, could hardly be given in a satisfactory manner.

And yet the explanation has not far to be sought for. We have long known, says the author, that the intensity of a current of a voltaic circuit depends on two things,—the electromotive force and the resistance. We know, moreover, that it is the quotient from the division of the former by the latter.

---

* Translated and communicated by Mr. William Francis, from Poggendorff's *Annalen*, vol. l. p. 255.

† This notice is taken from the Reports of the Royal Berlin Academy, and must be considered merely as a preliminary notice with reference to the main subject; a more accurate numerical determination of the elements treated of will follow in a short time.

‡ Announced by Mr. Roberts in L. and E. Phil. Mag., vol. xvi., p. 142.

Now the electromotive force between zinc and iron is, it is true, smaller than that between zinc and copper, silver or platina, which is evident from this, that when it is combined in the opposite sense with one of the last-mentioned circuits into a system, it is immediately overpowered by it; but that resistance in two circuits which are alike in all except in the nature of the plates, constitutes the sole or essentially different element; viz. the resistance of transition is, as Fechner has shown, in general likewise small in metals which are attacked by the liquid of the circuit. If now, as might hence be supposed, this resistance is smaller in iron with acids and solutions of salts than in copper, and indeed in still greater proportion smaller than the electromotive force between zinc and iron, in comparison to that of zinc and copper, it is evident that, all other circumstances being equal, the current of the zinc-iron circuit must be stronger than that of the zinc-copper circuit.

If, however, the iron circuit owes its greater intensity of current to the smallness of its resistance of transition, then its current must possess a slighter tension than that of the copper circuit, or in other words, it must be weakened by the insertion of a foreign resistance of some consideration in a greater proportion than that of the latter circuit. The testing of this circumstance must decide as to the correctness of this explanation.

The author has undertaken such an examination, it is true, in want of an apparatus of measurement, only with the help of a common galvanometer, which for quantitative determinations is an ill-suited instrument, but will however afford for the present case a sufficient approximation. The presupposition was made that the intensity of the current must be proportional to the tangent of the deviation. This presupposition is evidently false: the force increases in a greater proportion than the tangent of the angle of deviation, since the needle on rotation goes further from the convolutions of the wire. But exactly because the force increases in greater proportion than the tangent of the angle of deflection,—the stronger forces, therefore, were estimated in a greater proportion too small than the weaker,—the conclusions drawn from the measurements under that presupposition must deserve the more confidence.

The plates employed were all of equal size, 1 inch broad, dipped 2·5 inches into dilute sulphuric acid, and stood 5 lines from each other. The current of the two circuits, that of zinc-iron and that of zinc-copper, were successively measured in the above-mentioned manner under two circumstances; once when the circuit was merely closed by the 11-foot long and $\frac{1}{3}$-line thick multiplier wire; and then, when to this a wire of German silver, about 50 feet in length and of the same thickness, the resistance of which is nearly equal to that of a 50-feet long copper wire of the same thickness, was added. To render the galvanometer suited for the measurement of forces of some intensity, a similarly directed position was given to its needles.

In three experiments made at different times, the intensity of the current corresponding to the smaller resistance, which may be expressed for each by 100, sunk, by the insertion of the greater resistance, in the following proportion:

Copper-circuit.	Iron-circuit.
100 : 17·6	100 : 12·3
100 : 19·6	100 : 14·4
100 : 17·2	100 : 13·6

The greater weakening current of the iron circuit is here so evidently expressed, that no doubt can remain as to its reality, especially if we consider that it is the intensity of the current of this circuit which, as the greater, in both cases, and especially in the smaller resistance, has been estimated too low more than that of the copper circuit. It may, therefore, be regarded as good as proved by these measurements, although they are but approximations, that the ascendency of the intensity of the current of the iron-circuit is founded on the smallness of its resistance of transition.

In the just-mentioned measurements, the current of the iron circuit was more weakened than that of the copper circuit by the interposition of the 50-feet long German silver wire. It remained, nevertheless, always considerably stronger than the latter, as will be evident from the following comparison of the intensities of the current of both circuits:

Iron : Copper with smaller resistance.	Iron : Copper with greater resistance.
267 : 100	187 : 100
237 : 100	175 : 100
225 : 100	178 : 100

But it is evident that we can, by continued increase of the inserted resistance,—presupposing naturally that it be both as to size, immersion, and distance of the plates, equal for both circuits,—at last arrive so far as to make the current of the iron circuit not only as weak, but even weaker than that of the copper circuit.

The author has attempted to test the theory even on this interesting point, but he did not succeed, with the means at his disposal, with *equal* and indeed with the *stated* size of the plates, even in attaining an equality between the intensity of current of both circuits. The currents, it is true, can be weakened very easily, and in any degree, by the interposition of a long column of liquid, but they then become unmeasurable if the multiplying (multiplicatorischen) means be not increased in the same proportion. A multiplier, with 16 to 20,000-feet long copper wire, such as employed by Fechner, is an essential requisite for such inquiries.

On the other hand, it will be perceived that the ascendency of the current of the iron-circuit will sink in that measure more, when we, under otherwise like circumstances, take the plates of both circuits larger, but still remaining equal; but evidently a considerable size of the plates will be requisite to render the current of the copper circuit more powerful, or even as powerful as that of the iron circuit.

And therefore it is that the discovery of Mr. Roberts is of importance in the practical application. With moderate size of plates and moderate resistance, which may already at present be foreseen, we may in all the usual circuits and piles substitute iron with considerable advantage for copper, both with respect to œconomy as well as to action; and the more so in piles or many-membered circuits, as the ascendency of the zinc-iron combination over that of zinc-copper must increase with the number of plates.

The author, intending subsequently to communicate more accurate measurements on this as well as on allied subjects, is induced, however, to observe, that the iron circuit, although not affording a constant current, still possesses, in so far, a not inconsiderable advantage over the copper circuit, as the intensity of the current decreases with it slower than with the latter.

Lastly, the iron-circuit is therefore of much interest, for the theory, in as far as it perhaps exhibits more distinctly and decidedly than any other known phænomenon the presence and influence of the resistance to transition still doubted by so many *. There is a whole series of similar phænomena, espe-

---

* Even the author was formerly inclined to suspect in the resistance to transition, at least partially, an effect of the discharge, which, indeed, actually occurs in many observations not entirely foreign to the above. But experiments made by him on the passage of the currents of Saxton's machine through fluids, in which a discharge in a perceptible degree cannot have place, convinced him subsequently, most decidedly, of the existence of such a resistance. The first interposition of a thin layer of fluid in the circuit produced a resistance at least four to five times as great as the doubling of this layer, as could be observed by means of an air-thermometer, and a peculiar, well-adapted arrangement for the accurate measurement of resistances. That, moreover, the resistance of transition with iron, even in a solution of caustic potash, a liquid which does not dissolve this metal, is smaller than with copper, evidently proves that this resistance is not always in inverse ratio to the chemical attack, which, indeed, has long

cially in the contrary sense, *i. e.* cases where weakness of current is connected with considerable magnitude of the electromotive force; but one which points to the cause so clearly as the iron circuit, might perhaps not easily be found a second time.

Hitherto the author has only succeeded in discovering one pair, and that moreover less prominent, analogous to the zinc-iron circuit. This is a circuit of amalgamated zinc and common zinc. This produces (the size and distance of the plates being as above, charged with diluted sulphuric acid, and merely closed by a multiplier-wire 11 feet long) a more powerful current, than under similar circumstances, one of amalgamated zinc and cadmium or tin. Since cadmium and tin are more negative than zinc, consequently the electromotive force of the latter circuits greater than that of the former, it is evident that the greater intensity of current of this can likewise solely be founded on its slighter resistance to transition. It is likewise conceivable that, from the smallness of the electromotive force of a circuit of amalgamated and common zinc, there can be but few combinations with the first metal as positive member which are inferior to it in intensity of current. And this is really the case. Already a circuit of amalgamated zinc and brass or copper produces, notwithstanding its greater resistance to transition, a more powerful current.

Another fact belonging here, is that the amalgamated zinc, which, from its being considerably more positive than the non-amalgamated, is usually regarded as far more effective than the latter (which has even given rise to special explanations), does in fact only produce a current of less great intensity than the non-amalgamated, when it, as this, is connected with the circuit by a negative metal, a dilute acid and a connecting wire of moderate resistance. The current from amalgamated zinc and copper is in so slight a degree stronger than that from non-amalgamated zinc and copper (all other circumstances in both circuits being alike), that a momentary removal of the plates from the acid, or any other mode of opening the latter circuit, suffices to give to this in intensity of current the ascendency over the former*. This equilibrium even follows of itself after no very long time. The advantage of employing amalgamated zinc in the construction of voltaic piles, consists, therefore, almost solely in no metal being uselessly expended: little is gained in effect by it.

---

since been known from its decrease in platina, by moistening this metal with nitric acid.

* This, as well as the previously-mentioned comparisons, were made with the so-called *differential-galvanometer*.

The author remarks, in conclusion, that amalgamated iron (which is obtained without difficulty by immersing iron in a solution of the chloride of mercury, or in metallic mercury to which a dilute acid has been added), combined with zinc and acid, produces a considerably weaker current than, under similar circumstances, the non-amalgamated iron: it is, however, still much more powerful than that from a zinc-copper circuit. Amalgamated iron is less attacked than the non-amalgamated by dilute sulphuric acid, and is somewhat more *negative.* The latter circumstance, connected with the silvery lustre of the amalgamated iron, shows distinctly the insufficiency of the position lately advanced by Vorsselman de Heer, that amalgamated zinc is more positive than the non-amalgamated, from its surpassing this in lustre; an assertion which, if not already disproved in general by the well-known action of alloys, of which just as many stand *below* and *above* as *between* their constituents in the electromotive scale, would be contradicted by the fact that non-amalgamated zinc still always remains considerably negative towards the amalgamated, even when it has acquired the greatest possible degree of lustre by filing. An inquiry respecting the order of succession of various easily oxidizable metals, in the amalgamated and non-amalgamated state, gave the following result, enumerated from the positive to the negative: *amalgamated zinc, zinc, cadmium, amalgamated cadmium, amalgamated tin, amalgamated lead, lead, tin, iron, amalgamated iron.* Of these five metals, therefore, three, viz. *zinc, tin, lead* (the latter, however, but very slightly), are in the amalgamated state *more positive,* two, on the contrary, *cadmium* and *iron, more negative* in this state than in the non-amalgamated.

XI. *Additional Observations on the Voltaic Decomposition of Alcohol. By* Arthur Connel, *Esq., F.R.S.Ed., Professor of Chemistry in the United College of St. Salvator's and St. Leonard's, St. Andrew's. Read at the Meeting of the British Association in Glasgow.*[*]

In the course of the experiments by which I endeavoured to establish, some years ago[†], that under powerful voltaic agency, the water entering into the constitution of absolute alcohol is resolved into its elements, the hydrogen being evolved at the negative pole, and the oxygen going to the positive, where it is usually employed in producing a secondary action, although under particular arrangements it separates and becomes visible;—I showed that the galvanic agency was greatly increased by dissolving minute quantities of potash in the alcohol, so small a quantity as $\frac{1}{10000}$th part having a decided influence; this effect being, as I conceived, due to an increased conducting power of the liquid, and to the disposing affinity of the potash for the products of the secondary action of the oxygen. Notwithstanding the very insignificant quantity of the dissolved alkali which was capable of producing this result, and the circumstance that the voltaic decomposition took place without its aid, as well as by adding saline substances which do not contain water, and that the quantity of hydrogen collected greatly exceeded that contained in the water of the dissolved potash, it was objected in more than one quarter, that the increase of effect was due to the water of the hydrate of potash employed. It afterwards occurred to me that every cavil might be obviated by employing small quantities of potassium instead of potash. It is known that when potassium is dissolved in alcohol, it is oxidated with evolution of hydrogen; and when the solution is carried to saturation, a solid compound is obtained of anhydrous potash and æther. Hence, if a minute portion of potassium is dissolved in absolute alcohol, a corresponding quantity of anhydrous potash will remain in solution; and thus we can increase the proportion of the dissolved body as we think proper without the fear of adding water. Accordingly, it was found that minute quantities of potassium had precisely the same effect as minute quantities of potash in promoting the voltaic agency. The quantities of elastic fluid collected in such experiments are necessarily small, because a considerable electric action is required; and when we carry the power beyond a certain point, the action is so violent that the liquid is thrown into ebullition, and the foils are soon left bare.

---

[*] Communicated by Sir David Brewster.

[†] Transactions of the Royal Society of Edinburgh, vol. xiii. and Lond. & Edinb. Phil. Mag. 1835.

About a drachm of alcohol (sp. gr. ·7918, at 66° F.), with a quantity of potassium dissolved, equivalent to $\frac{1}{266}$ of anhydrous potash, yielded, under the action of seventy-two pairs of four-inch plates, 0·8 cubic inch of hydrogen from the negative pole in 1^{h} 5^{m}; and when the dissolved anhydrous potash was $\frac{1}{133}$, 0·99 cubic inch were obtained in the same time. In these experiments the greatest energy of the battery was not brought into action, as is done by placing the platinum foils parallel to one another. Their extremities were simply approached to one another in the alcohol. After an hour's action the evolution of gas slackened considerably; so that after two and a half hours' action, the quantity collected was about 1¼ cubic inch from the beginning. But by re-charging the battery, and adding a little more potassium to the same alcohol, we can renew the effect. Thus in an experiment where, with a quantity of potassium dissolved equivalent to $\frac{1}{366}$ of anhydrous potash, 1·15 cubic inch of hydrogen was obtained, and the discharge had much slackened, as much more potassium was added to the same alcohol, and the charge of the battery was renewed. The flow of gas then became as rigorous as ever, and in two hours from the renewal 1·04 cubic inch of additional hydrogen was obtained, or 2·19 cubic inches from the commencement of the experiment; and after some hours' longer action, 2½ cubic inches in all were collected. It is thus evident that, by successive additions of small quantities of potassium and renewals of the charge, we might decompose any proportion of the water, entering into the constitution of the alcohol, we think proper. In all these experiments similar secondary products of oxidation were formed at the positive pole as formerly detailed.

In repeating, on a larger scale, the experiments in which a comparison was instituted between the quantity of hydrogen evolved from alcohol and that evolved by the same current from water, I found that several circumstances require to be attended to. The platinum foil poles ought not to be placed parallel to one another in the alcohol containing dissolved potash or potassium, because a portion of the hydrogen, from being produced in near proximity to the other pole, enters into the constitution of the secondary products of oxidation, and the quantity evolved is thus diminished. And even when the extremities only of the foils are approximated to one another, and the negative is kept uppermost, there still is a certain diminution of the hydrogen liberated, unless where other circumstances of powerful action are employed. Thus where $\frac{1}{366}$ of anhydrous potash was in solution, and seventy-two pair of four-inch plates were employed, the quantity of hydrogen liberated from water, acidulated with $\frac{1}{12}$ of sulphuric acid within the first hour, was usually about one-quarter greater than that from the alcohol; and if the action was longer continued, the difference continued to increase. The cause of this I conceive to be twofold; first, that a part of the hydrogen, unless when very rapidly liberated, is absorbed by the liquid and enters into the constitution of the substances formed at the positive pole; and second, that when the solution diminishes considerably in conducting power the electric action diminishes also; a result which may be imitated by passing the same current through distilled water, and water acidulated with sulphuric acid, when the hydrogen liberated from the pure water will be found to be notably less than that from the well-conducting fluid.

To obtain a satisfactory result, we ought to dissolve a somewhat larger quantity of potassium in the alcohol, and employ a still more energetic electric current, and foils approached to one another in the same plane, when the quantities of hydrogen liberated from the two fluids will be found to be very nearly equal in amount; and the comparison is best instituted during the early stages of the action, because the potash gets saturated with the secondary products of oxidation, and the conducting power diminishes, whilst the electric energy is also on the decline.

I still consider the experiments, now and formerly detailed, to afford the only direct experimental proof which we yet have of the existence of water, as such, in absolute alcohol. The oxidation of potassium in alcohol, with evolution of hydrogen, is perhaps the nearest approach of other experiments to such evidence; but still the tendency to form æther, and the affinity of potash for that substance, might give rise to the evolution of hydrogen. But it appears to be quite impossible, on electro-chemical principles, to suppose, that under electric agency a part of the hydrogen of the alcohol should go to one pole, and the remainder, with its other constituents, to the other, there being no evidence of any *one* substance at the positive pole, which could have been in combination with hydrogen as an electrolytic compound; and, besides, we have direct proof that it is oxygen which passes to the positive pole, because the effects which take place there are effects of *oxidation*, and under particular arrangements that oxygen becomes even *visible*. We have thus oxygen going to the one pole and hydrogen to the other, and the latter in the proportion in which it exists in water; and it is only a truism to say that the direct voltaic decomposition of any substance cannot take place, unless that substance previously has a distinct and substantive existence.

XII. *On the Electricity of Effluent Steam.* No. II. *By* W. G. ARMSTRONG, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN concluding my last communication on the above subject*, I alluded to experiments which had then been commenced to try the effect of insulating the boiler and entirely condensing the issuing steam. These experiments have since been completed, and many others have also been tried which subsequently suggested themselves, all of which I shall now proceed to describe.

---

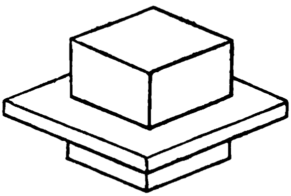
* See our last Number, vol. xvii., p. 452.

The insulation of the boiler was effected by lifting the engine with screw-jacks, until the wheels were raised about six inches above the rails, and then supporting it upon four insulators which rested upon logs of timber. Each insulator consisted of three separate pieces of baked wood, coated with pitch, and having layers of pitch and brown paper placed between them. The middle piece was made larger than the other two, so as to project beyond them, and thereby increase the surface without adding to the height of the insulator, which would have been dangerous; and the three pieces when put together formed a block, of which a representation is annexed (fig. 1.).

As soon as the engine was placed on the insulators, the boiler was filled with water, and the fire lighted, and as the pressure gradually rose in the boiler, the steam was occasionally suffered to escape.

The engine indicated no electricity whatever so long as the steam was confined in the boiler, but became negatively electrified as soon as any escape was permitted. A very trifling escape proved sufficient to render the negative electricity of the boiler sensible, and when the steam was very freely discharged the negative development became exceedingly powerful. The sparks never much exceeded an inch in length, but were very large and brilliant, and, owing no doubt to the magnitude of the body from which they were drawn, they produced effects fully equal to those obtained by the use of an average-sized Leyden jar. Some idea of their potency may be formed from the fact, that when not more than half an inch in length, they easily ignited a piece of cotton wool filled with powdered resin.

Fig. 1.

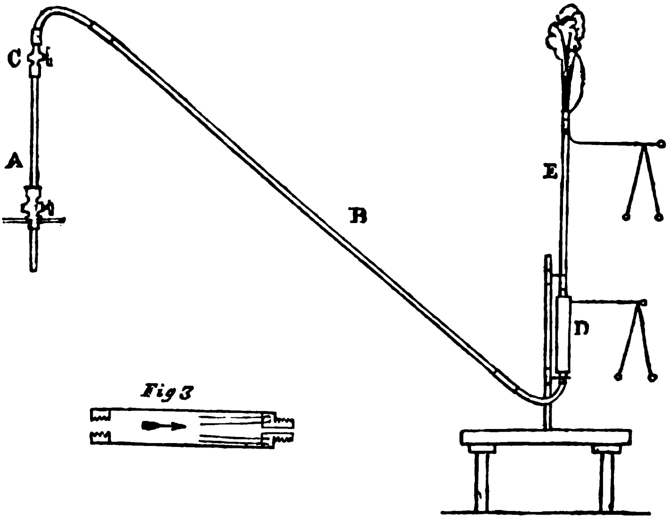


The greatest care was taken to ascertain whether the extent of the electrical development was at all dependent upon the *density* of the steam in the boiler; and it was found that when the discharge from the valve was so regulated in the different trials as to render the actual quantity or *weight* of steam ejected in a given time as uniform as possible, the negative electricity of the boiler increased *a little* with the pressure, but the positive electricity, drawn by the pointed conductor from the issuing steam, increased *enormously* as the density of the steam was augmented. After the fire had been extinguished at the close of the experiments, and the pressure had subsided to six or eight pounds on the square inch, sparks could no longer be obtained from the conductor held in the steam, but the negative electricity of the boiler continued to produce sparks until the steam was entirely exhausted, or as nearly so as possible. These results appear to indicate that a jet of high-pressure steam is not in reality much more electrical than one of low-pressure steam, but merely that the electricity of the high-pressure jet is more easily collected.

The insulation of the boiler undoubtedly had the effect of diminishing the positive electricity obtained from the steam, but not to such an extent as might have been anticipated.

A glass tube A (fig. 2), with a cock C affixed to it, having been inserted in the boiler in the manner described in my last letter, another glass tube B, about four feet long, was attached to the cock, and supported at the end furthest from the boiler by a glass rod fixed in the insulating stool. A small brass cylinder D having a number of pointed wires projecting from one end into the inside, as shown in the section (fig. 3),

Fig. 2.



was then joined to the glass tube B, and to this cylinder was added a third glass tube E, so as to extend the channel through which the steam was to be conveyed. A wire with forked points was affixed to the top of the tube E for the purpose of collecting the electricity of the jet, and from this wire a pair of pith-balls was suspended. Another pair of pith-balls was in like manner suspended from a wire screwed into the brass cylinder, all which arrangements will be clearly understood by reference to the figure. The cylinder, and a great part of each of the tubes, were enveloped in flannel, so as to prevent condensation as much as possible. The chief object of this apparatus was to test the electrical condition of the steam before it issued into the air, both when the boiler was insulated and when it was connected by a conductor with the earth. An iron bar was then placed against the engine to establish a communication between it and the ground, and the cock being opened, a current of steam mixed with water issued from the tube E. Under these circumstances both pairs of balls remained stationary, the electricity being probably carried off by the water which escaped with the steam. As the tubes became heated the quantity of water discharged with the steam was reduced to a mere spray, and both pairs of balls then slightly diverged with positive electricity, the upper pair expanding rather more than the lower pair; but upon partially closing the cock both pairs of balls expanded fully *three times as much* as they had done when the cock was fully open; and then gradually converged, arriving at their original position after the lapse of about a minute.

I was at first exceedingly puzzled how to account for this singular effect of attenuating the steam, but I now think it may be explained as follows. It is probable that the moisture of the steam when the cock is fully open is sufficient to enable it to conduct great part of the electricity of the jet back again to the boiler; but that as soon as the temperature of the steam is reduced by the attenuation below the temperature which the tubes have previously acquired from the high-pressure steam, its dryness is so far increased as to prevent the transmission of electricity from the jet to the boiler, but not from the jet to the cylinder, which is a much nearer object. As the tubes cool down to an equality with the steam the dampness returns, and the electricity is again in a great measure carried back to the boiler.

The iron bar was then removed from its contact with the boiler so as to restore the insulation, and the engine was rendered intensely negative by a copious emission of steam from the safety-valve. The cock being again fully opened, both pairs of balls diverged strongly with *negative* electricity; and by diminishing the escape from the valve to a certain point, and then partly closing the cock, the upper balls indicated *positive*, and the lower ones *negative* electricity. When the escape from the valve was entirely prevented and the cock fully opened, both pairs of balls remained in a collapsed state, but on partly opening the cock they both diverged for a short time with positive electricity to much the same extent as they had previously done while the boiler communicated with the earth by means of the iron bar.

It is important to state that when the balls were repelled with *negative* electricity, they collapsed very considerably when the cock was touched by the hand, but when they diverged with *positive* electricity no effect was produced by touching the cock, which strongly favours the supposition that the *positive* electricity manifested by the lower pith-balls, was conducted to the cylinder from the jet.

A coil of lead pipe, immersed in a glas jar filled with wet snow, was then placed upon the insulating stool and connected with the tube B, after the brass cylinder had been detached; and the iron bar was again placed against the boiler to suspend the insulation. Upon opening the cock to the full extent, little or no electricity could be discovered in the coil, but when the cock was partly closed, positive electricity appeared for a short time and then faded away exactly as in the experiments with the brass cylinder. Upon removing the connexion between the boiler and the earth and raising the valve the coil became highly negative, but upon closing the valve the negative electricity vanished.

I have little doubt that the predominance of the *negative* over the *positive* electricity in the above experiment is attributable to the conducting power of the steam causing more negative electricity to be conveyed to the coil from the boiler, than the coil would acquire if the steam were a non-conductor. When negative electricity was in like manner so strongly manifested by the pith-balls in the experiments with the brass cylinder, it was noticed, that after closing the cock, while the interior of the tubes were bedewed with moisture, scarcely any negative electricity was transmitted to the balls, so that the conducting power must have been in the steam, and not in the mere dampness on the glass.

A vertical glass tube, about an inch in diameter, and between two and three feet long, was then screwed on to the cock in substitution of the tube B. The lower part of this tube contained a number of pointed wires for the purpose of abstracting and imparting to the cock any electricity which the steam might possess on entering the tube. When the cock was now fully opened, flashes of light began to dart through the whole length of the tube, from the cloud above it to the cock, and continued to do so as long as the cock remained open, both when the boiler was insulated, and when it was connected with the earth. The steam in the tube was perfectly transparent, and no moisture could be seen on the inner surface of the glass.

The visible transmission of electricity from the jet to the cock, in this experiment, furnishes convincing proof that the positive electricity which we find in the jet is not developed until the steam assumes the form of a visible vapour; and it shows also that the steam, even in its transparent state, is, as I have already surmised, a tolerable conductor of electricity.

I will now venture to offer such an explanation of the electrical phænomena of effluent steam, as I conceive to be most consonant with the experiments I have described in this and my preceding communication.

Independently of the experimental proofs which have been adduced of the neutral state of the steam up to the instant of its transformation into an opake vapour, the water and the steam must be so thoroughly intermixed in the boiler of a locomotive engine, as to render it impossible for the steam to become electrified with positive electricity without immediately imparting it to the water.

I assume, then, as a fact, established both by reason and experiment, that the steam is in a neutral state in the boiler; and I think I am equally supported in saying, that it does not exhibit positive electricity, after leaving the boiler, so long as it retains its aeriform nature.

We learn from experiment that a development of negative electricity in the boiler accompanies the emission of the steam; and since the negative development in the boiler is obviously independent of the subsequent condensation of the steam, it follows, that if the effluxion of the steam could be effected without permitting any condensation to take place, we should then have a development of *negative* electricity in the boiler without any simultaneous development of *positive* electricity; and in like manner if the ejected steam were subsequently condensed into water, we should then have a liberation of *positive* electricity without a concomitant liberation of *negative* electricity.

These conclusions are entirely incompatible with the theory of two electric fluids, but are quite reconcileable with the hypothesis of a single fluid. It seems perfectly rational and consistent with analogy to suppose, that the immense augmentation of volume which takes place when water expands into steam of any ordinary density, should occasion a greatly increased capacity for electricity; and consequently, that the quantity of electricity which suffices to produce a neutral or saturated state in water, should be quite inadequate to sustain that condition when the water is converted into steam. Upon this supposition the steam as it forms in the boiler will absorb electricity from the adjacent conductors in order to acquire a *neutral* or *saturated* state, and when by condensation it again becomes water, the electricity thus absorbed will necessarily be set free; and hence the positive electricity which we find in the jet.

Upon the same principle, if the boiler be insulated, the water, the boiler, and the uncondensed steam, will all be rendered negative, provided the steam be permitted to escape, but not otherwise; for if the steam be confined within the limits of the boiler, the evaporation will not be attended with any *increase of volume*, and the absorption of electricity will in consequence be prevented. In all these respects the theory exactly accords with observation.

I am bound, however, to admit that the explanation which I have here advanced, involves certain conditions which appear somewhat at variance with experiment. In the first place the condensation of a given weight of *low-pressure* steam ought, if the absorption of electricity depends upon increase of volume, to liberate more positive electricity than the condensation of an equal weight of *high-pressure steam*; and in the second place, the expansion of steam from one degree of density to another should, on the same principle, be accompanied by a development of negative electricity. Notwithstanding, however, what has been advanced in favour of high-pressure steam containing more electricity than steam of low density, I think it quite possible that the reverse of this may be the fact; for it is not at all improbable that a low-pressure jet may conduct so large a portion of its electricity back again to the boiler as to make it appear to liberate less electricity than a high-pressure jet, while in reality it may develope a great deal more. It is quite reasonable to suppose that the conducting power of steam should be increased by rarefaction; and besides, in order to make a fair comparison between the quantities of electricity liberated by two jets of steam, it is indispensable that they should discharge equal *weights* in equal times, and to effect this condition it is necessary to discharge a much larger *volume* when the steam is of low pressure, than when it is of great density, and by so doing we unavoidably increase the quantity of unvaporized water swept out of the boiler in conjunction with the steam, and thus improve the communication between the jet and the boiler, and at the same time cause a considerable dispersion of the electricity.

With regard to expansion producing negative electricity, I have certainly never detected such a result; but I much question whether in any of the above experiments expansion was effected without an aqueous condensation sufficient to produce a countervailing effect; or the insulation was ever sufficiently perfect to prevent feeble negative electricity being carried off, or overwhelmed by the positive electricity developed in the jet.

Mr. Nicholson has zealously cooperated with me in the experiments I have described, and it is our intention to make a further attempt to clear up the difficulties which still embarrass the subject.

The limits of this letter will not permit me at present to enter upon any discussion on Dr. Schafhaeutl's interesting paper which appeared in your last publicatlon.

Newcastle-upon-Tyne, WM. GEO. ARMSTRONG.
Dec. 19, 1840,

XIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Anniversary Address of the Marquis of Northampton, *President, November* 30, 1840.

GENTLEMEN,

IN addressing you at the termination of this, the second year that I have had the honour of presiding over your Society, my first duty is to return my thanks to those gentlemen whom you have nominated to be my Council. They have rendered an onerous duty comparatively light and easy by their unremitting attendance and zeal; and have, as I trust, prevented your affairs from suffering from any incompetency on my part. In making my report of the transactions of the last year, I know of nothing to regret, except the loss of some of our valued Associates, who between the end of last November and the present time have paid the debt of nature.

Among the new members enrolled in our body, it will perhaps be right to mention the name of the Bishop of Norwich, as being President of the Linnean Society, one of the oldest branches that may be considered as thrown off from our parent stem. Also that of the Duke of Richmond, as President of the two newest Associations founded for the promotion of science, the Royal Agricultural and Botanical Societies. We have one new member of still higher rank, who has honoured us by becoming one of our Fellows, H. R. H. Prince Albert, the consort of our beloved Queen and Patroness. As your organ, Gentlemen, I will venture to say that you duly appreciate the honour conferred on yourselves; at the same time while His Royal Highness gratifies us by joining our body, we entertain no doubt that he does so from the just conviction that the patronage and advancement of science are national objects of the deepest importance.

The Antarctic Expedition, to whose departure I adverted in my last year's Address, is, I trust, now successfully pursuing its career of scientific research. Already a portion of the fruits of its labours has reached us, and promises an abundant and valuable harvest. The fixed Magnetic Observatories on the territories of Her Majesty are now also in full operation; while foreign powers have given us the assistance of observations made at the same instant of time, and on the same system as our own. I have the satisfaction to state, that these observatories exceed forty in number *.

While these researches are being made by our own and other states, private enterprise is not idle. Mr. Enderby, to whom geography is already indebted, has sent out a vessel for the purposes of discovery in the Antarctic seas, with the object of approaching as near as may be to the Southern pole. His ship is navigated by Mr. Mapleton, an officer who had been selected by Her Majesty's Government to take a part in Captain Ross's expedition; but he had not returned to England before that Expedition had sailed. We may well hope that he will merit this double confidence, and that he will, if his life be spared, add another wreath to the laurels of England, won by a Parry and a Ross, a Cooke and a Vancouver.

You are aware, Gentlemen, that previously to the departure of the late expedition, the Council of the Royal Society was requested by the government to draw up a statement of the most desirable objects in science to which the attention of the officers employed might be directed. With this request, as you might also know, the Council immediately complied; and in the execution of the duty thus devolved on us, the assistance of the Scientific Committees was our principal means of success. Since that time it has occurred to us, that the same recommendations, in rather a different shape, might be of great use to other scientific travellers. We have accordingly taken considerable pains in perfecting these suggestions, which we have caused to be printed for the public in general †. We have already furnished copies to the Commander of the Expedition to the Niger, and I hope that, in addition to his higher objects, he may be enabled to promote our acquaintance with the details of the geography and natural history of those imperfectly known parts of the globe.

I have the satisfaction to inform you, that by desire of the Council Mr. Shuckard has completed a Catalogue of the valuable manuscript letters in our library, among which are many from Ray, Willoughby, Newton, Boyle, Hook, and other eminent men, which we trust may serve as a useful aid to those Fellows who may wish to consult documents of so much interest and value.

You will also be glad to know that Mr. Halliwell, one of our Fellows, has undertaken and executed the task of making a catalogue of the miscellaneous manuscripts in our library; a labour for which I am sure you will feel much indebted to the author. In this collection the Society possesses one most valuable manuscript, the *Principia* in the hand-writing of the immortal Newton.

The Royal Society, Gentlemen, was founded for the advancement of natural knowledge, not for any purposes of private advantage or vain glory. It must, therefore, always hail the foundation and

---

[* See L. & E. Phil. Mag. vol. xiv. p. 137; also vol. xv. p. 224; for an account of the instrument and mode of observation to be employed in the magnetic observatories founded by the British government: also vol. xvi. p. 598, xvii. pp. 144, 418.—EDIT.]

[† A portion of these recommendations, relating to physics and meteorology, will be found in L. & E. Phil. Mag. vol. xv. p. 177.—EDIT.]

prosperity of new bodies of scientific men, brought together by the same object in particular branches of science, either in the abstract, or connected with important arts and manufactures. It cannot but rejoice, therefore, at the continued prosperity of the British Association, and in the formation of new societies for Microscopical Research*, and for the improvement of Botany and Agriculture.

With respect to the last of these, we must look with satisfaction to every effort to carry the torch of science, to light up the intricacies and doubts of an art, the first probably in age, and certainly the first in importance to civilized man. As to the Botanical Society, we cannot but be glad that the want of a public and national Garden should be in some measure supplied, and we may indulge a hope that the example may, at a future time, lead our Government to provide an establishment commensurate with the wealth of Great Britain, with the magnitude of the metropolis, and with the extensive colonial empire from which it might be supplied with the productions of the most varied climates.

With the continued and increasing prosperity of the British Association, I may the more boldly call on you to sympathize, as the greater part of its most active and talented members are also among the most valued of our own Fellows. It had this year a great additional merit in the very numerous attendance of the scientific natives of other countries, and it gave to those among us who were present at Glasgow the opportunity of becoming personally acquainted with some of our Foreign Members; with an Encke, and an Agassiz. The latter gentleman, having fortunately extended his visit to England, we have now the pleasure of seeing him at our Anniversary; a pleasure that we should have probably lost, had he not been attracted by the meeting of the British Association.

During the last year we have had more than one occasion of testifying our grateful and loyal attachment to our Royal Patroness. Another now presents itself, which I am sure you will gladly seize, to express our joy at the birth of a Princess, our gratitude to Heaven for our Sovereign's safety, and our fervent prayers for her long and prosperous reign over the hearts as well as persons of her subjects.

Since we last assembled our room of meeting has been ornamented, as you will see, Gentlemen, by the portrait of our late Royal President, from the pencil of Mr. Phillips. I am sure that you will be gratified by this addition to the likenesses of the far greater number of my predecessors. Future successors of the present Fellows of the Royal Society will look with satisfaction at the striking representation of a Prince, who has added to the many favours received by us from his illustrious house, that of deigning to preside over our Meetings and our Councils.

Of the papers that have been read at our Meetings, it is not necessary for me to speak. Of their merits you will be enabled to judge in a considerable degree by the Abstracts contained in our printed Proceedings; while those that have appeared to the Council,

---

[* The foundation of the Microscopical Society is noticed in L. & E. Phil. Mag. vol. xv. p. 549.]

assisted by the Scientific Committees, to be of the most importance, will be found in the Philosophical Transactions. As a new proof of which I cannot, however, refrain from adverting for a moment to one topic,—the discovery of Photography, for which we are indebted to a Neipce, a Daguerre, and a Talbot[*]; and which not only promises important results to art, but valuable assistance also, by the application of an Ibbotson and others, to those branches of knowledge which are connected with organic matter. This is, however, not all. Through the interesting observations of Sir J. Herschel and Mr. Hunt, followed up, as they will doubtless be, by other philosophers, a vista seems to open into hitherto unexplored regions of science[†].

I have the honour, Gentlemen, to inform you that the Council have awarded the Royal Medals for the present year to Sir John F. W. Herschel and Professor Wheatstone; one Copley Medal to Professor Liebig, and another to M. Sturm; and the Rumford Medal to M. Biot.

SIR JOHN LUBBOCK.

It is to me a cause of the highest satisfaction that I have to present through you this Royal Medal to Sir John F. W. Herschel, being the fourth Medal that has been awarded to him by the decision of different Councils.

It is to myself and to the other Members of the Council a most gratifying circumstance, that in an invention of great interest to art, he has found an instrument capable of making us acquainted with most curious laws regulating the chemical action of the different rays of the spectrum on the same substances. They have an additional interest to us as well as to you, in reminding us of researches made by the honoured parent whose example of scientific zeal he so well emulates. The observations that Sir J. Herschel has made in his recent communication, appear likely to lead to new discoveries in optical as well as chemical science; and we trust that the papers already before us may be the forerunners of still more striking and important results.

PROFESSOR WHEATSTONE.

It is with great pleasure that I address you, and present to you one of those Medals that our Royal Patroness has placed at the disposal of the Council of our Society. Your valuable experiments, and the ingenious methods by which you have solved the difficult question of Double Vision[‡], form the immediate, and certainly sufficient motive, for their adjudication. It is, however, still more satisfactory, as it gives me, as their organ, the opportunity to mark their sense of the value of your other discoveries, of the science and ingenuity by which you have measured Electrical Velocity, and by which you have also turned your acquaintance with Galvanism to the most important practical purposes.

---

[*] [See L. & E. Phil. Mag. vol. xiv. p. 196.—EDIT.]

[†] [An Abstract of Sir John Herschel's paper appeared in L. & E. Phil. Mag. vol. xvi. p. 331; and of a paper by Mr. Hunt in vol. xvii. p. 381; in which volume also, p. 202, appears Mr. Hunt's last paper, referring to others before inserted.]

[‡] [Noticed in vol. xiii. p. 416; see also vol. xvi. p. 599.]

PROFESSOR DANIELL.

In confiding to your charge, as our Foreign Secretary, the Rumford Medal which the Council has awarded to M. Biot, I am sure that every cultivator of the higher and more abstruse branches of science will feel that it is bestowed on a philosopher, whose optical researches on the curious and interesting subject of Polarized Light are of the highest value. You may, at the same time that you forward the Medal, assure M. Biot of the anxious wish of the Royal Society that he may be long enabled to carry on inquiries so honourable to himself, and so important to more than one branch of science.

Professor Daniell, I hold in my hand, and deliver to you one of the Copley Medals, which has been awarded by us to Professor Liebig. My principal difficulty, in the present exercise of this the most agreeable part of my official duty, is to know whether to consider M. Liebig's inquiries as most important in a chemical or in a physiological light. However that may be, he has a double claim on the scientific world, enhanced by the practical and useful ends to which he has turned his discoveries. I hope that he may long be able to follow at the same time the paths of scientific research and practical utility.

Professor Daniell, I have again to call on you, in your official capacity, to transmit a Medal to the Continent. The gentleman to whom we have adjudged it is M. Sturm, for his valuable mathematical labours, the fruits of which must be important, not only to mathematics, but also to those other high and abstruse sciences to whose advancement algebraical analysis is a necessary instrument. In his solution, therefore, of a problem which has baffled some of the greatest mathematicians that the world has produced, he has well earned the gratitude of every lover of natural knowledge.

You will, Gentlemen, hear read an account of the eminent men connected with our Society whom we have had the misfortune to lose since last November. Having confided the task of enumerating them to one of your Members, more able than myself to do justice to their merits, I shall not further touch upon the subject than to express my deep regret at the decease of one who had been my predecessor in this Chair, and to whose counsel I might have looked for aid in any conjuncture of difficulty, with full reliance on his good sense and ability, and also on his zeal in any matter in which the interests of the Royal Society were at stake. I may also be permitted to express the condolence of all the Members of the Royal Society with the domestic affliction of our valued Treasurer by the decease of his father, who was also one of our Fellows. I will now desire Dr. Roget to read the account of those whom we now miss from our ranks.

The first name in the list of our deceased Fellows, which it is my melancholy duty to notice, is one which cannot be mentioned in this room, without feelings of deep regret for the loss of his services and of affectionate respect for his many virtues: it is hardly necessary for me to add that I refer to our late associate and former President, Mr. DAVIES GILBERT*.

He was the only surviving son of the Rev. Edward Giddy, of St. Erth in Cornwall, and his mother, whose maiden name was Davies, was the representative of several ancient and distinguished families in that county, and the heiress likewise of very considerable property. His early education, which was almost entirely domestic, was chiefly superintended by his father, who was an accomplished classical scholar; and in 1785 he became a gentleman commoner of Pembroke College, Oxford, where he attended the lectures of Dr. Beddoes on chemistry, Dr. Sibthorp on botany, and Mr. Hornby on geometry and astronomy, and devoted himself with very unusual diligence to the study of mathematics and the natural sciences. He used to boast in after life, with very becoming pride, that he was the first student of his class in the University of Oxford, who had ever read the Principia of Newton.

In 1791, he was elected a Fellow of this Society, and became associated from that time forward with the most eminent men of science in the metropolis. He had, in very early life, appreciated the extraordinary combination of poetical and philosophical genius in his friend and fellow-countryman Humphry Davy, at that time in a very humble capacity; and by recommending him, first, as an assistant to Dr. Beddoes in his experiments on the medical effects of gaseous inspirations, and, secondly, to the Royal Institution, he had the merit and good fortune of contributing to rescue from obscurity one of the greatest discoverers in modern chemistry†. In the year 1804, he became Member of Parliament for Helston, and in 1806 for Bodmin, a borough in his own immediate neighbourhood, which he continued to represent until the era of Parliamentary Reform in 1832. He was emphatically the representative of scientific interests in the House of Commons, and contributed by his exertions to carry many very important projects, including amongst them the great breakwater at Plymouth and the bill for the revision of weights and measures; a bill founded upon the report of a commission of which he was a member, in conjunction with Captain Kater, Dr. Young and Dr. Wollaston.

Mr. Davies Gilbert was the author of Papers in our Transactions "On the Mathematical Theory of Suspension Bridges‡," with particular reference to the Menai bridge, which was at that time in progress, and the curvature of which was considerably modified in conformity with the results of his calculations:—"On the Progressive Improvements made in the Efficiency of Steam-engines in Cornwall, with investigations of the methods best adapted for imparting great

---

[* Another obituary notice of Mr. Davies Gilbert appears in the Supplementary Number for the present month, vol. xvii. p. 531. A third will be given shortly.—EDIT.]

[† See L. & E. Phil. Mag. N. S. iii. p. 57, for Mr. Gilbert's own statement of these circumstances.]

‡ For 1826, Part III. p. 202.

Angular Velocities*," in which he first distinctly defined and made known to men of science what is termed the *duty* of steam-engines, from the correct observation of which so many important practical improvements have followed:—"On the Nature of Negative and Imaginary Quantities†," which contains many ingenious views, although they have been in a great measure superseded by later speculations on this subject‡. Mr. Gilbert was a mathematician of the old school; but the papers to which we have just referred are very creditable specimens of the clearness with which he apprehended the bearing of some simple theoretical truths upon very important practical questions.

He became President of this Society in 1828, on the resignation of Sir Humphry Davy; a situation from which he retired in 1831. He continued, however, for the remainder of his life, to take a prominent part in the concerns of the Society; and there are few of my brother-Fellows, whom I have now the honour of addressing, who have not had opportunities of observing and appreciating his constant zeal for the interests of science, the variety and philosophical character of his conversation, the simple and unaffected eloquence of his public addresses§, and, above all, that sweetness of temper and kindness of heart which beamed forth in the expression of those truly classical and benevolent features, which one of the most accomplished of our artists (himself a brother-Fellow) has so happily perpetuated in the portrait which adorns these walls. The very absence of that inflexibility of purpose and of opinion which some might consider essential to the perfection of the character of a philosopher, seemed, in his case, the proper developement of that natural benevolence and humanity which made him so justly beloved in every relation of life, whether as a husband, a father, and a brother, —as a master, a landlord, and a friend.

Mr. Gilbert was the author and editor of several antiquarian and other works relating to his native county, whose interests he always laboured to promote with more than common zeal and patriotism. He was President of the Cornish Geological Society from the period of its first establishment in 1814, and he never omitted attending its meetings, though on the last occasion he was so weak as to be compelled to resign the chair to his friend and countryman Sir Charles Lemon. In 1808, he married Miss Gilbert, and assumed her name in 1817, on succeeding to a very large property in Sussex. The same simple and unaffected character which distinguished him in public life was still more conspicuous in his domestic relations. He died on the 24th of December last, and his body was borne to the grave by his own labourers, and followed by his widow and family in that primitive and unostentatious form which best suited the simplicity and natural humility of his own character.

---

* For 1830, Part I. p. 121. † For 1831, Part II. p. 341.

[‡ Abstracts of the two papers here referred to will be found in Phil. Mag. and Annals, N. S., vol. vii. p. 449, vol. ix. p. 37. Mr. Gilbert communicated a paper on the Regular or Platonic Solids, in Phil. Mag. and Annals, N. S. vol. iii. p. 161; and in vol. ix. p. 200, appears his original statement respecting the legacy of the late Earl of Bridgewater.]

[§ Addresses delivered to the Royal Society by Mr. Gilbert, will be found in Phil. Mag. and Annals, N. S. vol. iii. p. 50, vol. vii. p. 33, and vol. ix. p. 39.—Edit.]

Dr. SAMUEL BUTLER, Bishop of Lichfield, was born in 1774, at Kenilworth, which was likewise the birth-place of two other contemporary prelates of our church. He was educated at Rugby, and became afterwards a member of St. John's College, Cambridge, where he gained the highest classical honours which the University could confer. In 1798 he was made Head Master of Shrewsbury School, over which he continued to preside during a period of thirty-eight years. His great acquirements as a scholar, his eminent skill as a teacher, his active interest in the welfare of his pupils, and the tact and knowledge of character which he showed in their management, all contributed to raise the school to the highest reputation, and to give it, during many years, the pre-eminence over every other school in the kingdom in the number and rank of the academical honours which were gained by his scholars. The date of his elevation to the Bench was nearly contemporaneous with the appearance of that fatal disease which, after three years of the most depressing sufferings, borne with most exemplary patience and resignation, brought him to the grave. He was a man of great cheerfulness of temper and disposition, kind, affectionate and generous in every relation of life, and justly the object of the grateful attachment and love of his numerous pupils.

Dr. Butler was the author of an elaborate edition of Æschylus, with the notes and text of Stanly, and of several educational and other works. He formed a very extensive library; and his collection of Aldines, which is unhappily now dispersed, was perhaps the most complete in Europe. One of his last works was an interesting memoir of Dr. John Johnstone, of Birmingham, with whom he had long been connected by the bonds of the most affectionate friendship.

Mr. JAMES PRINSEP, whose brilliant career of research and discovery has been closed by a premature death in the flower of his age, was Principal Assay Master, first of the Mint at Benares, and secondly of that of Calcutta, where he succeeded Professor Wilson in 1833; he was a young man of great energy of character, of the most indefatigable industry, and of very extraordinary accomplishments; he was an excellent assayist and analytical chemist, and well acquainted with almost every department of physical science; a draughtsman, an engraver, an architect, and an engineer; a good Oriental scholar, and one of the most profound and learned Oriental medallists of his age.

In 1828 he communicated to our Society a paper "On the Measurement of High Temperatures," in which he described, amongst other ingenious contrivances for ascertaining the order, though not the degree, of high temperatures, an air-thermometer applicable for this purpose, and determined by means of it, probably much more accurately than heretofore, the temperature at which silver enters into fusion[*].

[[*] See Phil. Mag. and Annals, N. S. vol. iii. p. 129, and vol. x. p. 356, note.—EDIT.]

His activity whilst resident at Benares has more the air of romance than reality. He designed and built a mint and other edifices; he repaired the minarets of the great mosque of Aurengzebe, which threatened destruction to the neighbouring houses; he drained the city and made a statistical survey of it, and illustrated by his own beautiful drawings and lithographs the most remarkable objects which the city and its neighbourhood contains; he made a series of experimental researches on the depression of the wet-bulb hygrometer; he determined from his own experiments the values of the principal coins of the East, and formed tables of Indian metrology and numismatics, and of the chronology of the Indian systems and of the genealogies of Indian dynasties, which possess the highest authority and value.

When transferred to Calcutta, he became the projector and editor of the "Journal of the Asiatic Society of Bengal," a very voluminous publication, to which he contributed more than one hundred articles on a vast variety of subjects, but more particularly on Indian coins and Indian Palæography. He first succeeded in deciphering the legends which appear on the reverses of the Greek Bactrian coins, on the ancient coins of Surat, and on those of the Hindoo princes of Lahore and their Mahomedan successors, and formed alphabets of them, by which they can now be readily perused. He traced the varieties of the Devanagari alphabet of Sanscrit on the temples and columns of Upper India to a date anterior to the third century before Christ, and was enabled to read on the rocks of Cuttock and Gujarat the names of Antiochus and Ptolemy, and the record of the intercourse of an Indian monarch with the neighbouring princes of Persia and Egypt; he ascertained that, at the period of Alexander's conquests, India was under the sway of Boudhist sovereigns and Boudhist institutions, and that the earliest monarchs of India are not associated with a Brahminical creed or dynasty. These discoveries, which throw a perfectly new and unexpected light upon Indian history and chronology, and which furnish, in fact, a satisfactory outline of the history of India, from the invasion of Alexander to that of Mohammed Ghizni, a period of fifteen centuries, are only second in interest and importance, and we may add likewise in difficulty, to those of Champollion with respect to the succession of dynasties in ancient Egypt.

These severe and incessant labours, in the enervating climate of India, though borne for many years with little apparent inconvenience or effect, finally undermined his constitution; and he was at last compelled to relinquish all his occupations, and to seek for the restoration of his health in rest and a change of scene. He arrived in England on the 9th of January last; but the powers both of his body and his mind seemed to have been altogether worn out and exhausted; and after lingering for a few months, he died on the 22nd of April last, in the forty-first year of his age. The cause of literature and archæology in the East could not have sustained a severer loss.

Sir Anthony Carlisle was born at Stillington, in the county of Durham, in the year 1768. After commencing his professional education at York, under the care of his uncle, he became a student at the Hunterian School of Anatomy, in Windmill Street, under Dr. Baillie and Mr. Cruikshank, where he attracted the particular notice of John Hunter. He subsequently became a resident pupil of Mr. Henry Watson, one of the most eminent surgeons in the metropolis, whom he succeeded at the Westminster Hospital in 1793. In 1800 he communicated to our Transactions a paper "On a Peculiarity in the Distribution of the Arteries sent to the Limbs of Slow-moving Animals." This was followed by many others on various points of comparative and human anatomy, including his papers "On Muscular Motion," and "On the Arrangement and Mechanical Action of the Muscles of Fish," which formed the Croonian Lectures for 1804 and 1806. He was the author likewise of many communications in the Transactions of the Linnean and Horticultural Societies and in other contemporary journals, on different branches of natural history and physical science*.

"An Essay on the Connexion between Anatomy and the Fine Arts," led to his appointment, in 1808, to the Professorship of Anatomy to the Royal Academy, a situation which he filled with great advantage to the students during a period of sixteen years.

Sir Anthony Carlisle was not less distinguished for his knowledge of anatomy, physiology, and natural history, than for his professional merits, and for his patience and skill as an instructor of medical students. As a practitioner, he was invariably kind and attentive to those who were entrusted to his care, and eminently liberal in devoting his professional services to those who had no adequate means of repaying them.

Mr. Nicholas Aylward Vigors was born in 1787, at Old Leighlin, in the county of Carlow, where his family had long resided. After the usual preparatory education, he proceeded to the University of Oxford, where he became a very diligent and successful student. On quitting the University, he purchased a commission in the Guards, and distinguished himself highly at the battle of Barossa, by continuing to bear the colours of his regiment after he was severely wounded. On his return from the Peninsula, he was prevailed upon, by the earnest entreaties of his family, to quit the army; and he devoted himself afterwards, with characteristic ardour, to scientific and literary pursuits.

Mr. Vigors was one of the founders and the first Secretary of the Zoological Society, to whose museum he gave his very valuable collections of ornithology and entomology, which were the two branches of natural history he had most carefully studied. He was the author of a very elaborate paper in the Linnean Transactions†, "On the Natural Affinities which connect the Orders and Families of Birds," in which he attempted to apply in detail the same principles of arrangement that Mr. MacLeay had previously

---

[* In 1832, Sir A. Carlisle communicated to us some official documents respecting the health of workmen employed in the sewers of Westminster and the existence of cholera among them, which were inserted in L. and E. Phil. Mag. vol. i. p. 354.—Edit.]

† Linnean Transactions, vol. xiv.

sketched out in his *Horæ Entomologicæ*, in a more general way, as applicable to the whole animal kingdom. He afterwards published, in conjunction with Dr. Horsfield, another very valuable memoir* on the Birds of Australia, grounded upon a rich collection from that country, in the possession of the Linnean Society, which they described and arranged according to their natural affinities. He was likewise the principal editor, during several years, of the "Zoological Journal," in which he wrote many memoirs, chiefly devoted to the further exposition of his views with respect to the affinities of birds, but some of them descriptive of new or rare Mammalia, or new forms of exotic insects or birds †.

Mr. Vigors was a man of very considerable attainments as a scholar as well as a naturalist ‡, and made a liberal use of an ample private fortune in the promotion of those sciences which he cultivated: he was the representative in Parliament, for some years before his death, first of the city, and lastly of the county of Carlow.

Mr. Rickman was born in 1771, and educated at Westminster School, from whence he proceeded as a student to Christ Church, Oxford. Early in life he was recommended by Dean Jackson as Secretary to Mr. Abbott, the Speaker of the House of Commons, and was chosen to examine and digest the Parliamentary returns under the first Population Act in 1800, a duty which he continued to perform at the three succeeding decennial periods, and was preparing to discharge it for the fifth time during the present year, when he was attacked by the disease which terminated in his death. He was appointed second Clerk Assistant to the House of Commons in 1815, and subsequently Clerk Assistant, an office which he continued to hold for the remainder of his life.

The introductions to the "Population Returns," of which he was the author, are remarkable for the very able analysis which they contain of the general condition, changes and prospects of all classes of the population §.

Mr. Rickman was an excellent classical scholar, and was, in addition to many other attainments, extremely well acquainted with many branches of engineering and practical mechanics. He was the intimate friend, and after his death the executor, of the late Mr. Telford, whose autobiography he published, with a preface and an atlas of engravings descriptive of his principal works, which is in every way worthy of the fame of that great engineer.

---

* Linnean Transactions, vol. xv.

[† Mr. Vigors's name has very frequently appeared in our pages, from the time when he first became known as a naturalist to a very recent period. Abstracts of many papers by him occur in the Proceedings of the Zoological Society, beginning in Phil. Mag. and Annals, N. S., vol. ix. p. 54, and continued through that and the present series.—Edit.]

[‡ Mr. Vigors was the author of "An Inquiry into the Nature and Extent of the Poetic Licence," of which a second edition appeared in 1813, Lond. 8vo.—Edit.]

[§ A review of the Introductions to the Population Returns (from the pen, we may now mention, of the late George Harvey, F.R.S.), was inserted in L. & E. Phil. Mag. vol. i. p. 213.—Edit.]

Sir **Jeffrey Wyattville**, member of the Royal Academy and a distinguished architect, was a member of a family which has long been honourably connected with the arts. He was born in 1766, and acquired a knowledge of his profession under the instructions of his father and uncle, and was subsequently employed, during many years, in the somewhat ambiguous capacity of architect and builder in the execution of many considerable works. In 1824 he was selected by George IV., to whom he had been formerly known, to design and superintend the magnificent alterations and additions to Windsor Castle, a truly royal and national work, in which he succeeded in combining uncommon external grandeur and strict architectural propriety with great convenience and splendour of internal arrangements. Sir Jeffrey Wyattville, besides many important original works, made very extensive additions to the principal mansions of our nobility, including Chatsworth, Longleat, Woburn, Badminton, and Ashridge. He was a man of sound judgment and great integrity, and was very generally beloved for the remarkable simplicity and frankness of his manners, his great kindness of heart, and cheerful and unaffected good humour. He died in February last, and was buried in St. George's Chapel, at Windsor, in a vault which he had himself prepared for the reception of the remains of a beloved daughter, who died in the flower of her age.

Captain **Charles Phillipps**, of the Royal Navy, was the author of several inventions of great value in navigation, and in the equipment and management of ships: such are his methods of suspending compasses so as to avoid concussions in time of action; his improvement of the pump-dale of ships, and more particularly the capstan, which bears his name, and which is in general use in the Navy. He was an active and enterprising officer, who had seen much service during the last war, had been eminently successful in rescuing slaves off the coast of Africa, and had nearly fallen a victim, in common with the greatest part of his crew, to that pestilential climate.

Sir **Robert Seppings** received his education as a shipwright under the late Sir John Henslow, Surveyor of the Navy, and continued in connexion with the important service of our dock-yards during a period of fifty years. He was the author of many important improvements in our naval architecture, including his system of diagonal bracing and trussing, which formed the subject of two memorable Papers in our Transactions in the years 1814[*] and 1818[†], and which attracted an unusual amount of public attention. The great principle of this method was such an arrangement of the principal timbers as would oppose a powerful mechanical action to every change of position of the ribs and other timbers in every part of the ship; thus firmly compacting together the entire fabric, and preventing that perpetual racking of beams and working of joints, which, in the ancient system of ship-building, produced hogging, creaking, leakage, and rapid decay; and filling up likewise every vacuity between the timbers, which were occasionally the unavoidable receptacles for foul air, filth, vermin, and various other sources of rottenness and disease.

---

[*] On a New Principle of Constructing His Majesty's Ships of War.—Phil. Trans. 1814, p. 28. [See Phil. Mag. First Series, vol. xliii. pp. 228. 305; vol. xlv. p. 374.

[†] On the great strength given to Ships of War by the application of Diagonal Braces.—Phil. Trans. 1818, p. 1.

These important improvements, though opposed to the inveterate prejudices of the older shipwrights, a body of men who have not sufficiently valued and understood, in this country at least, the just principles of mechanical action, in the practical operation of ship-building, were universally adopted in the Navy under the enlightened administration of Mr. Charles York, and the powerful advocacy of Sir John Barrow*; and the merit of their author was acknowledged by his appointment as Surveyor of the Navy, and by the award of the Copley Medal of this Society.

This was not the only important improvement which Sir Robert Seppings introduced into our system of naval architecture. The Admiralty presented him with £1000 as a reward for his simple yet most useful invention of an improved block for supporting vessels, by which their keels and lower timbers were much more easily and promptly examined and repaired. His plan for lifting masts out of the steps, which superseded the employment of sheer hulks for that purpose, has been the means of saving much expense and labour. His new mode of framing ships has led to a much more extensive use of short and small timbers, which were formerly of little value; but the most valuable of all the reforms of construction for which the Navy of England is indebted to him, was the substitution of round for flat sterns, which afford increased strength to the framework of the ship, greater protection against *pooping* in heavy seas, an almost equal power of anchoring by the stern and by the bow, a more secure and effective position for the rudder, and a stout platform for a powerful battery, embracing a sweep of more than 180°. This capital improvement was strenuously opposed by many distinguished naval officers, who regretted the loss of those magnificent cabins, which were better suited for purposes of state than of service; but the good sense of less prejudiced judges happily prevailed, and secured for our ships of war an additional claim upon the respect of our enemies.

Foreign nations have not been tardy to acknowledge the value of these important improvements, and their author received many substantial proofs of their sense of his merits; and we may safely affirm, that in the national record of the great benefactors of their country, there are few names which will deserve, and, we trust, continue to receive, a more grateful commemoration than that of Sir Robert Seppings.

It has long been the practice of the Royal Society to associate with its body those persons in our country who are most eminent for their high rank or their commanding talents, for their distinguished public services, for their accomplishments in the arts, for their attainments in literature, for the important influence which their virtues or labours may have exercised upon the character and prospects of society, or upon the general interests of humanity; wisely judging that science will gain both in the enlargement of its objects and in the dignity and estimation of its cultivators, by being thus united with whatever is best entitled to command and to receive the admiration and respect of mankind: it is amongst this class of our Members that I have to notice several losses of more than ordinary importance.

---

* In very able articles in the 24th and 43rd Numbers of the Quarterly Review.

The EARL OF MANSFIELD was a nobleman of illustrious family, who, in addition to many other accomplishments, was one of the most elegant and effective parliamentary orators of his day.

LORD HOLLAND was Chancellor of the Duchy of Lancaster, and a nobleman who was remarkable for his profound knowledge of the constitutional history of his country, and for the extent and variety of his literary attainments*. It was the remark of a well-known philosophical author and writer, "that there was something so sweet in the blood of the Foxes, that no one could approach them without feeling the fascination of their social powers:" and there was probably no man of his age who was the object of more enthusiastic love and admiration of his friends, private and political, than Lord Holland.

Sir WILLIAM SYDNEY SMITH was a hero in the most chivalrous period of our naval history, the scenes of whose early triumphs have so recently been rendered illustrious by others of an equally memorable character.

Sir JOHN LUBBOCK was one of those persons engaged in trade whose extensive transactions and liberal views give dignity to the operations of commerce: it is not one of the least distinctions of such a father, that his name and honours have been inherited by one whose profound acquirements in the most difficult branches of science have merited and received the highest honours which this Society is able to confer.

In our foreign list we have to lament the loss of three of our most illustrious members, Blumenbach, Olbers, and Poisson.

JOHN FRIEDRICH BLUMENBACH was born on the 11th of May, 1752, at Gotha, where his father was Prorector of the Gymnasium. He was accustomed to attribute the formation of his taste for literary history and the study of the natural sciences to the instructions and encouragement of Menz and Christ, two professors of Leipsig, who were friends and fellow-townsmen of his father. After studying for some time at Jena, he removed to Göttingen, for the purpose of completing his medical course, where he was very favourably noticed by Heyne and Michaelis, and more particularly by Büttner, Professor of Natural History, a great linguist, and a man of very extraordinary acquirements, whose museum of medals and natural history, when afterwards purchased by the University, he was employed to arrange. The skill and diligence which he showed in this employment, and the reputation of his professional and other attainments, secured him the appointment of Extraordinary Professor of Medicine in 1776, and of Ordinary Professor in 1778, a situation which he continued to hold for nearly sixty years.

---

* He was the author of a most elegant account of the life and writings of Lope de Vega, accompanied by some beautiful translations of his more remarkable poems.

His lectures comprehended Natural History, Comparative Anatomy, Physiology and Pathology, on all which subjects he published many valuable memoirs and other works, more particularly his admirable *Manuals*, which have long enjoyed an extraordinary popularity, and which have been translated into nearly every great European language.

The first of this series of publications was the "Handbuch der Naturgeschichte," which appeared in 1779. In his "Institutiones Physiologicæ," a work equally remarkable for the originality, precision and clearness of its statements, which was published in 1787, he made known his views on the "bildungs trieb," or "Nisus formativus," which he had before announced in the Göttingen Transactions for 1785, and which he made the subject of a special work in 1789*. His "Specimens of the Physiology of Warm- and Cold-blooded Animals," appeared in 1789. In 1794 he published in our Transactions, "Observations on some Egyptian Mummies opened in London in 1792," with especial reference to the three distinct varieties of national physiognomy which appear amongst them. His "Handbuch der vergleichenden Anatomie" appeared in 1805, and showed how fully he already appreciated the important views of Cuvier, which elevated Comparative Anatomy from a merely descriptive science to one which was capable of the most instructive generalizations, and affording the means of distinguishing types and laws of formation, as well for different organs as for different classes of animals.

The term *nisus formativus* was employed by Blumenbach to denote that *vital power* which is innate in all living organized bodies, and in active operation during the whole period of their vital existence, by which they are controlled and modified with reference to a specified end; it is that power by which the organizable matter of every individual being assumes, at its conception, its allotted form; which form is also capable of successive modifications by nutrition, according to the purpose for which it is destined by the Author of Nature, as well as of the reparation (within prescribed limits) of the injuries which it may have received. The announcement of this principle was received with extraordinary favour by physiologists, though it differed in little more than in name from the *vis essentialis* of the celebrated Wolff. It will be found to have formed the basis of some of his important speculations.

Blumenbach's well-known collection of the crania of the different races of mankind was made with a view to their more accurate classification, and gave rise to some of his more celebrated publications†. According to his ultimate views, he would make the

---

* Ueber den Bildungs trieb.

† Collectio Decad. vi. craniorum diversarum gentium tabulis 60 æneis illustrata: 1790—1820. De generis humani varietate nativâ: 1795.

Caucasian race the primary stem, from which all the others have degenerated to the Mongol at one extremity, and the Æthiopic at the other, interposing the American variety between the Caucasian and the Mongol, and the Malay between the Caucasian and the Æthiopic: it is difficult, however, to arrive at very correct general conclusions on this very interesting subject, without reference to those which are founded on the analogies of language, as has been done by Cuvier and Prichard.

It is quite impossible, within the short compass to which this notice is necessarily confined, to convey more than a very general impression of the vast variety of the labours of this distinguished philosopher. We find him applying his knowledge of natural history in illustration of the arts and poetry of antiquity*; he was also one of the first naturalists who appreciated the importance of a knowledge of fossils in determining the relative ages of the strata of the earth†. He had cultivated archæology and literary history‡ from his earliest years with more than common interest and zeal. There were, in fact, few departments of knowledge and literature, however remotely connected with the natural sciences, which he has not illustrated by his writings: it was when thus travelling into provinces of knowledge which were somewhat foreign to his own, that he was accustomed to quote the adage of Seneca: "*Soleo et in aliena castra transire, non tanquam transfuga, sed tanquam explorator.*"

Blumenbach had long been considered as the patriarch of the University of Göttingen, and was allowed the full privileges attached to his distinguished reputation, to the memory of his long services, and to the respect due to his venerable old age; he retained his usual cheerfulness, his memory, and much of his ancient activity, until nearly the close of his life. He died on the 22nd of January last, in the 88th year of his age, a memorable proof that the tranquil pursuits of science and the gentle stimulus of constant though not laborious employments are equally favourable to contentment of mind and length of days.

The name of the venerable Dr. OLBERS, of Bremen, must be for ever memorable in the annals of astronomy, as the discoverer of two planets in our system. He was a member of that remarkable association of twenty-four astronomers which the indefatigable Baron de Zach of Gotha had formed towards the close of the last century, who undertook the vigilant observation of as many zones of the heavens, with a general view of discovering new comets and planets, and of recording any remarkable phænomena that might occur. Their zeal in the prosecution of these researches had been stimulated by the recent discovery of Herschel, as well as by the revival of a suggestion made by Kepler of the probable existence of a planet between Mars and Jupiter, in conformity with one of those mystical analogies, which might have been treated as the visionary dreams of an enthusiast, if they had not been so intimately connected with the discovery of the great laws forming the true basis of all correct knowledge of the system of the universe. The absence likewise of a planet at the distance from the sun, represented by 28, that of the earth being 10, interfered with the completeness of an empirical law which Bode of Berlin had suggested, and was not without its influence in confirming their faith in these extraordinary anticipations. The labours of this Association had been hardly organized, when the remarkable discovery of Ceres by Piazzi on the first day of the present century, in almost the precise position which Bode's singular law had assigned to it, seemed at once to convert their dreams into realities. Dr. Olbers calculated a circular, and Gauss an elliptic orbit for the same planet; and so wonderful was the accuracy of the first approximation to the elements which the latter had made, that they enabled Olbers to re-discover it on the 1st of January 1802, exactly one year after it had been first observed. It was in consequence of having formed a configuration of stars in the geocentric route of this planet, with a view to its being more readily found, that he discovered Pallas on the 25th of March of the same year[*], at nearly the same distance from the sun[†], though moving in an orbit more than three times as much inclined to the plane of the ecliptic. The discovery of two planets, in the position where one of them had been so anxiously sought for[‡], induced Dr. Olbers to conjecture that they were fragments of a larger planet, which had been scattered by some great catastrophe, and that many others probably existed at nearly the same distance from the sun, and possessing common nodes: he therefore earnestly recommended astronomers to observe most carefully those spaces of the heavens in which the nodes of these planets are placed; a practice which he himself observed for many years. His exemplary diligence was rewarded by the discovery of Vesta on the 29th of March, 1807, nearly in the precise position in which he had conjectured that it was most likely to be found[§]. This was the last of those remarkable discoveries whose history illustrates in so striking a manner that union of profound, yet somewhat visionary speculation, with unconquerable perseverance, which is so characteristic of the German nation.

---

* Specimen historiæ naturalis, antiquæ artis operibus illustratæ eaque vicissim illustrantis: 1803. Com. Acad. Gott., tom. xvi.

Specimen historiæ naturalis ex auctoribus classicis, præsertim poetis, illustratæ eosque vicissim illustrantis: 1815. Com. recent. Acad. Gott., tom. cxi.

† Beiträge zur Naturgeschichte der Vorwelt: 1790. Specimen archæologice telluris terrarumque imprimis Hannoveranarum: 1801. Also Comment. Acad. Gott., tom. xv. p. 132—156. Com. recent. Acad. Gott., tom. cxi. pp. 3—24.

‡ His "Introductio in Historiam Medicinæ Literariam," published in 1786, is a most instructive specimen of scientific bibliography.

---

[*] "Ueber einen neuen von Dr. Olbers in Bremen endeckten höchst sonderbaren cometen." Zach's Monatliche Correspondenz for May, 1802.

[†] If the distance of the earth from the sun be 1, that of Ceres is 2·7674, and that of Pallas 2·7676: the difference is less therefore than 19,000 miles.

[‡] Their essays on this subject were generally headed, "On the long-expected Planet between Jupiter and Mars."

[§] The longitude of the ascending node of Pallas is 172° 32′ 35″; that of Vesta is 171° 6′ 37″.

His well-known method of calculating the orbits of comets, which has been so generally used by German astronomers, was published at Weimar in 1797*, with a commendatory preface by his zealous friend the Baron de Zach. This memoir, independently of its other merits, is sufficient to show that its author was a mathematician of very considerable powers, and perfectly acquainted with the works of contemporary astronomers.

Dr. Olbers was a diligent observer of comets; and there are few astronomers who have contributed so much to our knowledge of these singular bodies. He was the discoverer of several comets, including the celebrated comet of long period of 1815; and we are indebted to him, not merely for very important suggestions and observations respecting the celebrated comet of Encke, but still more for having developed the taste for astronomical calculations and observations of that great astronomer, who for many years served him in the capacity of assistant in his observatory.

The Baron de Zach visited this observatory in September, 1800 †, and has described the simple apparatus which enabled him to make so many important discoveries. It was placed in the upper part of his house in the midst of the town of Bremen, and afforded openings or platforms sufficient to afford a command of nearly every point of the heavens. His instruments were an excellent five-foot Dollond of 3¾ inches aperture, with a circular micrometer (which he used in the observation of the small planets), a five-foot reflecting telescope by Schröter, a quadrant by Bird, an admirable sextant by Troughton, and a clock by Castens of Bremen. He possessed no transit instrument or fixed instruments of any kind; yet he speedily availed himself of the circumstances of his locality to determine his time with great accuracy, as well as nearly every element which the peculiar character of his observations rendered necessary; so fertile are the resources of genius and enterprize to overcome difficulties, which by ordinary men would be abandoned as altogether insuperable.

Simeon Denis Poisson, one of the most illustrious men of science that Europe has produced, was born at Pithiviers on the 21st of June, 1781, of very humble parentage, and was placed, at the age of fourteen, under the care of his uncle, M. L'Enfant, surgeon, at Fontainebleau, with a view to the study of his profession. It was at the central school of this place that he was introduced to the notice of M. Billy, a mathematician of some eminence, who speedily discovered and fostered his extraordinary capacity for mathematical studies. In 1798 he was elected a pupil of the École Polytechnique, which was then at the summit of its reputation, counting amongst its professors Laplace, Lagrange, Fourier, Monge, Prony, Berthollet, Fourcroy, Vauquelin, Guyton Morveau, and Chaptal. The progress which he made at this celebrated school surpassed the most sanguine expectations of his kind patron, M. Billy, and secured him the steady friendship and support of the most distinguished of his teachers.

---

* Abhandlung ueber die lechteste und bequemste methode die Bahn eines cometen aus einigen beobachtungen zu berechnen.

† Monatliche Correspondenz for Feb. 1801.

In the year 1800, he presented to the Institute a memoir "Sur le nombre d'intégrales complètes dont les équations aux différences finies sont susceptibles," which cleared up a very difficult and obscure point of analysis. It was printed on the recommendation of Laplace and Lagrange in the *Mémoires des Savans Etrangers*, an unexampled honour to be conferred on so young a man.

Stimulated by this first success, we find him presenting a succession of memoirs to the Institute on the most important points of analysis, and rapidly assuming the rank of one of the first geometers of his age. He was successively made Répétiteur and then Professor of the Polytechnic School, Professor at the Collège de France and the Faculté des Sciences, Member of the Bureau des Longitudes, and finally, in 1812, Member of the Institute.

His celebrated memoir on the *invariability* of the major axes of the planetary orbits, which received the emphatic approbation of Laplace, and secured him throughout his life the zealous patronage of that great philosopher, was presented to the Institute in the year 1808. Laplace had shown that the periodicity of the changes of the other elements, such as the eccentricity and inclination, depends on the periodicity of the changes of the major axis; a condition, therefore, which constitutes the true basis of the proof of the stability and permanence of the system of the universe. Lagrange had considered this great problem in the Berlin Memoirs for 1776, and had shown that, by neglecting certain quantities which might possibly modify the result, the expression for the major axis involved periodical inequalities only, and that they were consequently incapable of indefinite increase or diminution. It was reserved to Poisson to demonstrate *à priori* that the non-periodic terms of the order which he considered would mutually destroy each other; a most important conclusion, which removed the principal objection that existed to the validity of the demonstration of Lagrange*.

This brilliant success of Poisson in one of the most difficult problems of physical astronomy, would appear to have influenced him in devoting himself thenceforward almost exclusively to the application of mathematics to physical science; and the vast number of memoirs and works (amounting to more than 300 in number) which he published during the last thirty years of his life, made this department of mathematical science, and more particularly whatever related to the action of molecular forces, pre-eminently his own. They comprehend the theory of waves and of the vibrations of elastic substances, the laws of the distribution of electricity and magnetism, the propagation of heat, the theory of capillary attraction, the attraction of spheroids, the local magnetic attraction of ships, important problems on chances, and a multitude of other subjects, which the time allowed for this notice will not permit me to mention. His well-known treatise on Mechanics is incomparably superior to every similar publication in the clear and decided exposition of principles and methods, and in the happy and luminous combination of the most general theories with their particular and most instructive applications.

---

* The publication of this memoir recalled the attention of this illustrious mathematician to a subject which he had long neglected, and gave rise to three of his noblest memoirs. Poisson, in his "Mémoire sur le Mouvement de la Lune autour de la Terre," has not satisfactorily shown that the major axis of the moon's orbit contains no argument of long period amongst the terms which involve lower powers of a certain quantity *m*, which denotes the ratio of the sun's mean motion to that of the moon, than the fourth; a demonstration of this most important proposition has been given by Sir John Lubbock in the Philosophical Magazine for the present year. [L. E. & D. Phil. Mag. vol. xvii. p. 338.]

Poisson was not a philosopher who courted the credit of propounding original views which did not arise naturally out of the immediate subjects of his researches; and he was more disposed to extend and perfect the application of known methods of analysis to important physical problems, than to indulge in speculations on the invention or transformation of formulæ, which, however new and elegant, appeared to give him no obvious increase of mathematical power in the prosecution of his inquiries. His delight was to grapple with difficulties which had embarrassed the greatest of his predecessors, and to bring to bear upon them those vast resources of analysis, and those clear views of mechanical and physical principles in their most refined and difficult applications, which have secured him the most brilliant triumphs in nearly every department of physical science.

The confidence which he was accustomed to feel in the results of his analysis—the natural result of his own clear perception of the necessary dependence of the several steps by which they were deduced—led him sometimes to accept conclusions of a somewhat startling character: such were his views of the constitution and finite extent of the earth's atmosphere, which some distinguished philosophers have ventured to defend*. It is not in mathematical reasonings only that we are sometimes disposed to forget that the conclusions which we make general are not dependent upon our assumed premises alone, but are modified by concurrent or collateral causes, which neither our analysis nor our reasonings are competent to comprehend.

The habits of life of this great mathematician were of the most simple and laborious kind; though he never missed a meeting of the Institute, or a lecture, or an examination, or any other public engagement, yet on all other occasions, at least in his later years, he denied access to all visitors, and remained in his study from an early hour in the morning until six o'clock at night, when he joined his family at dinner, and spent the evening in social converse, or in amusements of the lightest and least absorbing character, carefully avoiding every topic which might recall the severity of

---

[* Noticed by Mr. (now Sir John W.) Lubbock, in L. E. & D. Phil. Mag. vol. xvii. p. 469.—EDIT.]

his morning occupations. The wear and tear, however, of a life devoted to such constant study, and the total neglect of exercise and healthy recreations, finally undermined his naturally vigorous constitution, and in the autumn of 1838 the alarming discovery was made that he was labouring under the fatal disease of water in the chest. The efforts of his physicians contributed for a time to mitigate the more serious symptoms of his malady; but every relaxation of his sufferings led to the resumption of his labours; and to the earnest remonstrances of his friends, and the entreaties of his family, he was accustomed to reply, that to him *la vie c'était le travail*; nay, he even undertook to conduct the usual examinations of the École Polytechnique, which occupied him for nearly ten hours a day for the greatest part of a month. This last imprudent effort ended in an attack of paralysis, attended by loss of memory and the rapid obscuration of all his faculties; he continued to struggle, amidst alternations of hope and despondency, for a considerable period, and died on the 25th of April last, in the fifty-ninth year of his age.

Poisson was eminently a deductive philosopher, and one of the most illustrious of his class; his profound knowledge of the labours of his predecessors, his perfect command of analysis, and his extraordinary sagacity and tact in applying it, his clearness and precision in the enunciation of his problems, and the general elegance of form which pervaded his investigations, must long continue to give to his works that classical character, which has hitherto been almost exclusively appropriated to the productions of Lagrange, Laplace, and Euler. If he was inferior to Fourier or to Fresnel in the largeness and pregnancy of his philosophical views, he was incomparably superior to them in mathematical power: if some of his contemporaries rivalled or surpassed him in particular departments of his own favourite studies, he has left no one to equal him, either in France or in Europe at large, in the extent, variety, and intrinsic value of his labours.

The last work on which he was engaged was a treatise on the theory of light, with particular reference to the recent researches of Cauchy: nearly two hundred pages of this work are printed, which are altogether confined to generalities, whose applications were destined to form the subject of a second and concluding section: those who are acquainted with the other works of Poisson will be best able to appreciate the irreparable loss which optical science has sustained in the non-completion of such a work from the hands of such a master.

XIV. *Intelligence and Miscellaneous Articles.*

LEPIDOMELANE—A NEW MINERAL.

THIS mineral comes from Pirsberg in Wermeland, Sweden, and appears to be a species of mica differing from others. It has been analysed by M. Soltman in M. Wohler's laboratory.

This mineral is a granular and schistose aggregate of small crystals in scales, which seldom exceed half a line in diameter; their form is irregular, but in some cases it has the appearance of six-sided tables, which approach a regular form; these scales are raven-black, and reflect a very bright green, and yield a green powder. Separately examined they are flat and shining, their lustre is vitreous like the diamond, with very brilliant aggregated faces. They are not translucent, but when very small they are transparent. It is difficult to state their cleavage and flexibility; their specific gravity is 3·; hardness on Mohs's scale = 3. It is therefore harder than the mica of two axes, but less so than the pearly mica; it is rather brittle; it is rather hard to the touch, but less than pearly mica, Heated to redness by the blowpipe, it becomes of a tombac brown colour, with the metallic lustre of magnetic iron ore. It gives a bottle-green colour to borax, dissolves readily in nitric and hydrochloric acid, and the silica remains in soft pearly scales and crystalline form, like the mica of one axis. It yielded by analysis

Silica	37·40
Alumina	11·60
Oxide of iron	27·66
Protoxide of iron	12·43
Magnesia and lime	0·60
Potash	9·20
Water	0·60
	99·49

In some of its characters this mineral resembles a species of mica which M. Breithaupt has described under the name of Siderischer Felsglimmer, or Raben Glimmer; but as the analysis of this has not been given, it is difficult to ascertain whether these two minerals belong to the same mineral species.—*L'Institut*, No. 352.

HYDROTELLURIC ÆTHER. BY M. F. WOHLER.

The existence of this compound is interesting, as affording a fresh proof of the remarkable analogy which exists between sulphur and tellurium, and as showing that the latter substance may, like sulphur, enter into the composition of organic compounds, and replace oxygen in them.

This æther is readily prepared by the double decomposition of sulphovinate of barytes and telluret of sodium; it is sufficient for this purpose to submit solutions of these two substances in water to distillation. The telluret is prepared by the calcination of tellurium, or the native telluret of bismuth with carbonate of soda, mixed with charcoal; and, in order to avoid oxidation, the rough product is immediately conveyed to the retort which contains the boiling solution of sulphovinate. The hydrotelluric æther distils with the water, yielding, at the beginning, much froth in the retort.

This substance is a liquid of a reddish-yellow odour, approaching the colour of bromine, but lighter; it is heavier than water, and this dissolves only a trace of it; its odour is disagreeable, extremely penetrating, and remains for a long time; it partakes of the smell both of hydrosulphuric æther and hydrotelluric acid, and it appears to be very poisonous. Its boiling point is below 212°; it readily takes fire and burns with a brilliant white flame, terminated by a blue which is quite peculiar, and it yields a very thick white vapour of telluric acid. This æther, when exposed to the air, is soon covered with a pellicle, which is at first yellowish, becomes afterwards white, and is eventually converted into tellurous acid. If it be exposed in a vessel to the direct rays of the sun, it immediately exhales vapour, and its oxidation goes on much more rapidly than under the action of diffused light; the action even of pure oxygen gas is, however, never sufficiently energetic to inflame the æther. Nitric acid attacks it rapidly, and dissolves it with the disengagement of nitric oxide gas; if hydrochloric acid be added to the solution, a heavy colourless liquid separates in oily drops; this has not been examined.

According to calculation, hydrotelluric æther should contain 68·53 of tellurium; analysis gave 68·75; in fact, 0·560 of this æther were dissolved in nitric acid, and hydrochloric acid was added; it was then boiled for a long time, after which the addition of sulphurous acid separated 0·385 of metallic tellurium dried *in vacuo*. This product is therefore simple hydrotelluric æther $C^4 H^5 Te$, and is composed of

Hydrogen	5·33
Carbon............	26·14
Tellurium	68·53
	100·

The product, corresponding to mercaptan, would contain 81 per cent. of tellurium.—*An. de Ch. et de Phys.*, 75, 215.

METEOROLOGICAL OBSERVATIONS FOR NOV. 1840.

Chiswick.—November 1. Foggy: rain. 2. Showery: fine. 3. Foggy. 4. Dense fog: clear and fine. 5. Rain: fine: rain at night. 6, 7. Rain. 8. Clear and fine. 9. Boisterous. 10. Cloudy and fine. 11. Hazy: rain. 12. Clear. 13. Boisterous with rain; barometer very low. 14. Clear. 15. Fine. 16. Cloudy and windy. 17. Stormy with heavy rain. 18. Heavy rain. 19. Rain: fine. 20. Very fine. 21. Stormy with rain. 22—24. Fine. 25. Dense fog. 26. Sharp frost. 27, 28. Very dense fog. 29. Foggy: fine. 30. Overcast.

Boston.—Nov. 1. Fine: lightning P.M. 2. Stormy. 3, 4. Cloudy. 5. Rain: rain early A.M. 6. Cloudy: rain A.M. 7. Cloudy. 8. Fine. 9. Cloudy: rain early A.M.: rain A.M. 10, 11. Fine. 12. Cloudy. 13. Rain and stormy: rain early A.M. 14. Cloudy. 15. Fine. 16. Cloudy: rain early A.M. 17. Cloudy: rain A.M. and P.M. 18. Fine. 19. Cloudy: rain A.M. and P.M. 20. Fine. 21. Rain and stormy. 22. Cloudy: rain early A.M. 23. Rain: rain early A.M. 24—30. Fine.

Applegarth Manse, Dumfries-shire.—Nov. 1. Dull, cloudy, and moist. 2. Showery all day. 3. Very fine. 4. Wet nearly all day. 5, 6. Occasional showers. 7. Wet A.M.: cleared up. 8. Slight showers. 9. Rain A.M.: cloudy all day. 10. Rain A.M.: moist P.M. 11. Thick fog all day. 12. Cloudy and watery. 13. Wet and stormy. 14. Fair after rain during night. 15. Fair and fine: frosty morning. 16. Rain and high wind. 17. Fair but dull and threatening. 18, 19. Frost: clear. 20. Thaw: cloudy. 21. Heavy rain A.M.: cleared. 22. Fair all day. 23. Rain early A.M. 24. Fair throughout and warm. 25. Drizzling day. 26. Frost. 27. Frost: very hard. 28. Frost; threatened change. 29. Cloudy: drizzling. 30. Moist without rain.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1840. Nov.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8¾ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·644	29·592	29·337	29·25	29·52	29·40	47·2	53·7	44·9	56	46	46	52½	42½	E.	NE.	calm	E by S.	...	·30	...	...	47
☽ 2.	29·538	29·515	29·455	28·95	29·16	29·41	51·2	53·3	46·5	57	45	47	50	45	SE.	S.	S.	E.	·050	...	...	...	46
3.	29·432	29·404	29·343	29·04	29·44	29·30	51·5	56·4	47·6	56	40	49	53½	43	NE.	E.	calm	E.	...	·02	...	...	48
4.	29·288	29·339	29·200	28·80	29·09	29·04	50·3	55·6	49·0	56	46	49	48	43	S.	W.	calm	E by S.	·052	·16	...	...	49
5.	29·202	29·232	29·152	28·69	29·01	29·04	50·3	55·8	48·8	55	41	50	46½	42	S.	S.	E.	E by S.	·302	·23	·15	...	49
6.	29·056	29·073	28·921	28·72	29·00	28·60	47·3	54·4	45·8	51	41	45	45	40	SE.	SW.	NW.	E by S.	·133	·48	·11	...	45
7.	29·138	29·154	29·083	28·63	28·74	28·98	48·8	51·7	48·0	58	41	49·5	49½	43	SSW.	SW.	NW.	S.	·286	·10	·09	1·00	47
8.	29·360	29·336	29·299	28·85	29·05	29·06	47·0	55·3	44·7	55	41	42	47½	35	S.	SW.	calm	S.	·083	·20	...	...	45
○ 9.	29·106	29·406	29·077	28·64	28·90	28·91	48·8	53·8	46·0	53	41	47	45½	42	S.	SW.	W.	NW.	·091	·10	·13	...	46
10.	29·074	29·263	29·046	28·63	28·92	29·05	44·5	53·5	44·2	51	37	39	45	39½	SSW.	NW.	calm	WNW.	·105	·01	·07	...	45
11.	29·332	29·308	29·207	28·88	29·10	29·20	44·2	50·3	42·2	49	36	38	44	31½	S.	SW.	calm	E by S.	...	·30	...	..	43
12.	29·528	29·516	29·316	29·08	29·50	29·32	41·3	49·8	40·6	54	39	36	44	34	S.	S.	calm	E.	·114	·35	...	..	40
13.	28·828	28·818	28·669	28·55	28·90	28·46	48·7	51·0	41·7	55	44	44	43	37	S.	S.	S.	E.	·272	·22	·44	...	45
14.	29·044	29·287	28·992	28·41	28·69	29·09	46·2	53·4	45·0	50	28	44	44½	41	S.	W.	W.	WNW.	·230	...	·24	0·77	44
15.	29·512	29·509	29·463	29·05	29·28	29·30	39·3	50·6	38·6	56	38	39	43½	31	S.	SW.	calm	WSW.	...	·41	...	...	39
☾ 16.	29·084	29·234	29·059	28·47	28·74	28·71	55·5	57·9	38·5	62	44	55	49	32	SW. var.	SW.	S.	SW.	·083	...	·27	...	49
17.	29·600	29·547	29·138	29·04	29·33	29·38	48·2	60·6	46·7	56	38	47·5	48	40	S. var.	S.	W.	ENE.	...	·40	·04	...	47
18.	29·814	29·789	29·605	29·39	29·69	29·75	41·2	54·6	41·4	44	34	35	38	28½	NW.	NE.	calm	NENW	·447	·41	·23	...	43
19.	29·800	29·939	29·771	29·46	29·89	30·00	39·8	42·4	37·2	44	30	40	38½	30½	N.	NE.	NW.	N.	·450	·01	·04	...	41
20.	30·112	30·078	30·061	29·70	29·85	29·88	35·7	44·7	35·0	43	37	33	44	25½	NW.	W.	calm	WNW.	...	·02	·02	...	33
21.	29·526	29·604	29·261	29·04	29·02	29·46	43·3	44·3	35·0	46	39	42	45	40	S.	SW.	W.	NNW.	·033	·16	·02	1·05	41
22.	29·794	30·033	29·778	29·42	29·89	29·92	40·3	47·3	40·0	43	30	41	43½	34	NNW.	N.	N.	NW.	·211	·01	·11	...	38
23.	30·070	30·056	29·980	29·61	29·74	29·79	38·4	43·5	35·6	44	39	36·5	52½	34½	S.	SW.	S.	W.	...	...	·07	...	33
● 24.	30·140	30·242	30·088	29·68	30·00	30·19	50·0	51·2	38·2	56	28	45	51	45	W.	NE.	calm	SW.	...	...	...	...	43
25.	30·424	30·400	30·372	30·00	30·24	30·28	41·0	54·3	38·3	46	24	35	46	38½	N.	SE.	calm	SW.	...	...	...	...	40
26.	30·460	30·433	30·382	30·05	30·30	30·31	37·4	47·0	36·0	44	22	32	41	33	E.	E.	calm	E.	...	...	...	...	35
27.	30·430	30·400	30·368	30·02	30·30	30·28	34·5	44·0	34·3	34	23	30	35½	24	N.	NE.	calm	SE.	...	...	...	...	35
28.	30·406	30·395	30·328	30·04	30·20	30·17	32·8	44·5	32·8	35	22	27·5	41	23½	NE.	NE.	calm	SE.	...	...	...	0·44	35
29.	30·336	30·326	30·185	29·94	30·02	29·79	30·8	39·3	30·3	36	30	31·5	48½	39	NE.	E.	calm	SW.	...	...	...	...	32
30.	30·084	30·066	29·932	29·58	29·72	29·78	46·7	47·7	30·3	54	50	45	51	47	S.	SW.	calm	SW.	...	...	...	...	35
Mean.	29·639	29·676	29·528	29·18	29·434	29·459	44·1	50·7	40·8	49·96	36·46	41·3	45·8	37·4					Sum. 2·942	3·59	2·03	3·26	Mean. 42

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

FEBRUARY 1841.

XV. *On the Combination of Hydrated (the Hydrate of) Sulphuric Acid with Nitric Oxide.* By M. ADOLPH ROSE*.

THERE has frequently occurred of late in commerce, a sulphuric acid which is more or less contaminated by one of the oxides of nitrogen, and which, treated with a solution of copperas, (*eisenvitriol*) gives at times a dark red, at times a blackish-brown colouring. It is usually considered as contaminated by nitric acid, which is said to originate, partially by the decomposition of the white crystals, which are frequently, or almost always formed in larger or smaller quantities in the leaden chambers†, partially by the oxidation of the nitrous acid in the chambers. Those crystals consist, according to Gaultier de Claubry and Henry, of sulphuric acid, nitrous acid and water, and are stated to be decomposed on solution in water, into sulphuric acid, nitric acid, and evolving nitric oxide.

This, even though slight, amount of nitric acid, in English sulphuric acid of commerce, is very injurious in the preparation of hydrochloric acid, as on the employment of such an acid a chloriferous hydrochloric acid is constantly obtained.

To purify the sulphuric acid from the nitric acid and sulphate of lead, it is usually subjected to distillation. On the distillation of a sulphuric acid which gave a strong colouring with copperas, I changed the receiver, as in this, according to almost all works on chemistry‡, the water and nitric acid are contained. To my astonishment, however, I found both this, as well as the next portion distilled, free from every trace of nitric acid, or any other oxide of nitrogen; on the contrary, the residuum in the retort was considerably more contaminated by one of these oxides, a fact which I subsequently found Barruel* had already drawn attention to, and which had been confirmed by Wackenroder†. On a second distillation, for which I employed 4 pounds of the same acid, I obtained the same result, somewhat above 1½ pound of perfectly pure acid having passed over.

---

* From Poggendorff's *Annalen*, vol. l. p. 161. Translated and communicated by Mr. W. Francis.

† [A paper on the composition of these crystals and on the relations of their formation in the manufacture of sulphuric acid, by Dr. S. L. Dana, appeared in Lond. and Edinb. Phil. Mag., vol. iii. p. 116.—EDIT.]

‡ Geiger's *Pharmacie*, latest edition, p. 273.—Mitscherlich's *Chemie*, p. 480.

Upon this I mixed 4 pounds of pure sulphuric acid with 4 ounces of nitric acid of 1·4 spec. gr., and subjected the mixture to distillation, taking the precaution to change the receivers frequently. The first three ounces distilled were very watery, and contained much nitric acid and little sulphuric acid; the succeeding two and three ounces consisted almost solely of sulphuric acid, which contained but little nitric acid, the next two ounces still contained a trace of nitric acid, after which one pound and some ounces of perfectly pure sulphuric acid distilled over. An acid then passed over, containing traces of one of the oxides of nitrogen, upon which the distillation was discontinued.

The residuum in the retort was coloured yellow, and disengaged, when mixed with water, nitric oxide, which changed, on coming into contact with atmospheric air, into nitrous acid. To observe this more distinctly, some of the acid was conveyed into a cylinder filled with distilled water, which then became filled with a colourless gas that on the addition of air became red, and was therefore nitric oxide. The residuum behaved towards reagents exactly in the same manner as the solution of the anhydrous sulphate of nitric oxide of H. Rose‡, in sulphuric acid, which led me to suppose that it might perhaps be a similar combination, a view which seems to be fully confirmed by subsequent experiments.

To be certain that nitric oxide, and not nitrous acid, and some nitric acid, which might likewise have been the case, is contained in the residuum from the distillation, I diluted half an ounce of it with water until no evolution of nitric oxide any longer occurred, then divided the liquid into two equal portions, added to the one half one drop of nitric acid, and boiled both portions the same length of time. The portion to which nitric acid had been added, treated with a solution of copperas and pure sulphuric acid, exhibited a very strong dark colouring; the other portion, treated in the same manner, did not exhibit this at all. Had the residue contained nitrous acid, nitric acid must have formed on dilution with water, and I must have obtained in the boiled liquid a reaction of nitric acid. I have subsequently always tested in this way, and have found it to be the best, when I wished to learn whether nitric oxide or nitrous acid was combined with the sulphuric acid.

---

* Centralblatt, 1836, p. 314.

† *Annalen der Pharmacie*, vol. xviii. p. 153.

‡ Poggendorff's *Annalen*, vol. xlvii. p. 605.

The sulphate of the protoxide of iron is a most excellent reagent for the slightest traces of nitric oxide, nitrous and nitric acids; but it is absolutely essential always to add a considerable quantity of pure sulphuric acid, as otherwise small traces might easily be overlooked.

Nitric oxide and nitrous acid may be very well distinguished in concentrated sulphuric acid from nitric acid, by the addition of a dilute solution of the bichromate of potash, for both reduce the chromic acid to oxide, while they themselves are converted into nitric acid. The liquid is thereby coloured green; it is, however, requisite to add the dilute solution of the bichromate of potash in drops, otherwise the green colour might easily be concealed by the excess of the reagent. Nitric acid cannot naturally act in a reducing manner on the chromic acid of the bichromate of potash. A solution of the permanganate of potash cannot be employed, as this is altered by powerful acids; but it is a most excellent test, if the sulphuric acid has been previously diluted with about six parts of water. If sulphuric acid be mixed with pure distilled nitric acid, diluted with six parts of water, and, after it has perfectly cooled, some drops of a solution of the permanganate of potash be added to it, the colour is not in the least affected; but if the residue from the distillation, which, as I have shown to be probable, consists of a solution of the sulphate of nitric oxide in sulphuric acid, be diluted with six parts of water, the solution of the permanganate of potash is decolorated by it. If I heat the diluted liquid for a moment, so that the solution of the protosulphate of iron with sulphuric acid still indicates the presence of nitric oxide, the solution of the permanganate of potash is likewise decolorated. If I continue to heat the solution of the protosulphate of iron with sulphuric acid, it no longer occasions any colouring, and a drop of the solution of permanganate of potash is no longer decolorated.

Wackenroder* advised the addition to the concentrated or diluted sulphuric acid of a solution of the sulphate of the deutoxide of manganese, which, on the presence of nitric acid is not, but on that of nitric oxide and nitrous acid is, immediately decolorated; however, the solution of the hypermanganate of potash is far more sensitive.

---

* *Annalen der Pharmacie*, vol. xviii. p. 154.

A portion of the residuum from the distillation of the sulphuric acid with nitric acid, was distilled in a small retort over an alcohol lamp, with the precaution that the passing product of distillation was frequently removed and tested. At first a sulphuric acid passed over, which contained very little nitric oxide; however, the amount of this latter increased in the succeeding portions more and more; and the last was a concentrated solution of the sulphate of nitric oxide in sulphuric acid, which acted towards reagents exactly like the solution of the sulphate of nitric oxide in sulphuric acid presently to be mentioned. Only a trace of the sulphate of lead remained in the retort. The last product was white, became yellow on heating, and on diluting it with water, green, blue, and lastly, colourless, under the evolution of much nitric oxide.

I hereupon attempted to prepare direct a combination of nitric oxide with hydrated sulphuric acid. I passed nitric oxide dried over the chloride of calcium, into distilled English sulphuric acid which was placed in a large vessel, but so that the entrance of air was prevented. A rapid absorption took place when the disengagement was not effected too violently, and no gas could be observed to escape; only when the evolution was too violent did nitric oxide escape. At the same time, a crystalline crust was formed on the sides of the vessel, which, however, disappeared again when the glass was shaken, it being dissolved by the acid. After passing the nitric oxide through for some time the liquid became lilac, then slightly blue, and at last, beautiful dark blue, without any increase of temperature being perceptible. The liquid became thicker and thicker, and on shaking the vessel ran down its sides like a thick syrup; on shaking it was converted into a white frothy mass, which, however, on being left to stand, again changed into the blue syrup. By continually passing nitric oxide through it, it was at last converted into a solid white crystalline mass, which melted on being slightly heated without decomposition, and on cooling, again solidified.

If I passed nitric oxide not dried over the chloride of calcium, into English sulphuric acid, a slight increase of temperature occurred; nevertheless, the crystals then appeared to form more rapidly, so that it seems that a little aqueous vapour favours their formation.

If this crystalline saline mass was treated in a dry glass gradually with water, nitric oxide was evolved, and there was formed, according to the quantity of water, a green, blue, and lastly, a colourless liquid. The crystalline mass dissolves in concentrated sulphuric acid without decomposition; and, if the solution be subjected to distillation, the superfluous sulphuric acid, contaminated by some sulphate of nitric oxide, first passes over, and then the concentrated solution of the sulphate of nitric oxide in sulphuric acid, which can be distilled over several times without decomposition. This solution, as well as the crystalline mass, was tested for nitric and nitrous acids in the manner previously described, without, however, a trace of them being discovered. As a counter-experiment here likewise, as always subsequently, a drop of nitric acid was added to the one half of the diluted solution, and both portions were boiled for an equal length of time. In the portion to which nitric acid had been added, I naturally obtained with sulphuric acid, and a solution of copperas, a deep brownish-black colouring; in the other portion not the slightest trace of it.

The sulphate of nitric oxide is, it is true, decomposed as rapidly by water as its solution in sulphuric acid; yet the decomposition does not seem to be effected perfectly in the cold; if, however, a great quantity of water be employed for dilution, and it be boiled somewhat longer, every trace of nitric oxide at last escapes. But so long as one of the oxides of nitrogen is detected in the solution, by means of sulphuric acid and copperas, the solution of the permanganate of potash is decolorated by it.

The combination of nitric oxide with anhydrous sulphuric acid, prepared by Prof. H. Rose, acts, according to the experiments I have made with it, in exactly the same manner. On dilution with water the same play of colours occurs under disengagement of nitric oxide; and if the diluted solution be boiled for some minutes, all the nitric oxide escapes, and the solution of copperas with sulphuric acid does not indicate any nitric acid in the boiled liquid. Likewise, in this case, the sulphate of the nitric oxide is more completely decomposed, English sulphuric acid being present. If the crystalline mass was diluted with much water, and heated only for a moment without sulphuric acid, it was, it is true, coloured by copperas and sulphuric acid, but then the cooled liquid also still discoloured a dilute solution of the permanganate of potash; as soon as this, after longer boiling of the cooled liquid, was no longer discoloured, did copperas and sulphuric acid likewise cease to react.

Since these crystals acted exactly in the same manner as those which are produced in the preparation of English sulphuric acid, as also those which originate on passing nitrous acid into the hydrate of sulphuric acid, and which, as already mentioned, are stated to consist not of nitric oxide, sulphuric acid and water, but of nitrous acid, sulphuric acid and water, I was induced to prepare them.

For this purpose I conducted into a spacious bottle, containing one ounce of distilled oil of vitriol, and which was connected air-tight with a pneumatic trough, nitrous acid which I obtained by boiling 1 part of starch with 10 parts of nitric acid of 1·3 spec. gr. (according to Liebig's method). The air in the flask became immediately coloured dark red, and the escaping gas was caught; it consisted, however, for the greater part, of carbonic acid which had been produced by the action of the nitric acid on the starch contemporaneously with the nitrous acid. Since my expectation that oxygen would be evolved was disappointed, I terminated the operation after an hour. A yellowish green liquid had formed. If one part was diluted with much water, a great deal of nitric oxide was evolved; the diluted fluid, to one part of which sulphuric acid was added, to the other not, was boiled for some length of time, the evaporated water being from time to time replaced; but, nevertheless, I constantly obtained, on the addition of pure sulphuric acid and solution of copperas, a strong brown colouring, while the solution of the hypermanganate of potash was not in the least decolorated by it.

The well-closed liquid solidified after some hours on shaking, and separated into a white crystalline mass and into a supernatant yellowish liquid, which was separated from the crystalline mass by means of a funnel and glass rod; the crystalline mass was dried on clay-stone (*Thonstein*), over sulphuric acid. The filtered liquid contained much sulphate of nitric oxide, dissolved in sulphuric acid and nitric acid; when diluted with much water and boiled for any length of time, the evaporated water being from time to time restored, I obtained with a solution of copperas and sulphuric acid a strong deep brown colouring, while a dilute solution of the permanganate of potash was not discoloured. The crystalline dried mass gave on dilution with water under evolution of nitric oxide the frequently-mentioned changes of colour, behaved like the sulphate of nitric oxide, only that in the boiled diluted aqueous solution traces of nitric acid were constantly detected; this slight trace undoubtedly arose solely from the adherent mother-liquor, from which it cannot be separated. The nitrous acid seems to have been decomposed on its passing through sulphuric acid into nitric oxide and nitric acid, the former combining with the sulphuric acid, while the latter is present in great quantity in the mother-liquor. It is hence evident, that the crystals formed in this manner are contaminated either by nitric acid or by the nitrate of nitric oxide.

I now prepared these crystals by passing sulphurous acid and nitric oxide into a large vessel filled with atmospheric air, into which, by means of a glass tube, I could spurt water or blow air. With the presence of some water and an excess of nitric oxide, the crystals formed immediately, and covered partly the inner sides of the vessel, partly appeared in the centre in crystalline flakes, quite similar to snow. Decomposed by water, nitric oxide was abundantly evolved; but if this solution was boiled with sulphuric acid and water, and then solution of copperas and pure sulphuric acid added to it, it evinced no reaction of nitric acid, but this must necessarily have been the case if the crystalline mass consisted of nitrous acid, sulphuric acid and water. I likewise in this case divided the solution into two parts, and added to the one portion a trace of nitric acid, boiled this portion even somewhat longer, and yet obtained a distinct reaction of nitric acid in the portion to which nitric acid had been added, while the other portion showed not the least trace. Nor did I omit to add to the diluted boiled solution, after cooling, a solution of hypermanganate of potash, and by that means firmly convinced myself that no nitric acid had been formed by diluting with water, and that these crystals therefore did not consist of nitrous acid and sulphuric acid, but of a combination of sulphuric acid, nitric oxide and water.

But if, after the operation is terminated and the vessel remains filled with nitric oxide, atmospheric air be so long blown into the vessel that it again becomes colourless, then indeed a slight reaction of nitric acid is obtained after boiling, which however certainly arises from the water in the vessel dissolving the nitrous acid formed. The solution was tested as above.

These, then, are the crystals which are formed in the lead chambers in the preparation of English sulphuric acid, and always will be formed when an excess of nitric oxide in comparison to atmospheric air and sulphurous acid is present, a portion of the nitric oxide then changing only into nitrous acid, which oxidizes the sulphurous to sulphuric acid, and this then immediately enters into combination with the nitrous oxide; nay, they will be formed even with an excess of nitrous acid and atmospheric air, the sulphuric acid formed decomposing the nitrous acid into nitric acid and nitric oxide, with which it combines. It is therefore requisite, in order to avoid the production of these crystals in the preparation of sulphuric acid, to see that sulphurous acid is constantly present in sufficient quantity in the chamber.

The quantitative analysis of these crystals, as well as of those which originate on passing nitric oxide into sulphuric acid, and which is connected with some difficulties, I intend communicating in a subsequent paper.

The solution of the sulphate of nitric oxide in sulphuric acid is so analogous to the red fuming nitric acid, that it is not improbable that the latter may be with more reason regarded as a solution of the nitrate of nitric oxide in nitric acid, in favour of which, moreover, is the fact noticed by Gay-Lussac, that crystals are formed on mixing the red fuming nitric acid with fuming sulphuric acid, which are undoubtedly the sulphate of nitric oxide. If the solution of the sulphate of nitric oxide in sulphuric acid be distilled, the superfluous sulphuric acid as above-mentioned first passes over, and then this concentrated solution, which can be distilled several times without decomposition. When heated, the concentrated solution becomes yellow; diluted with water it is changed, under evolution of nitric oxide, according to the quantity of water, into a green, blue, and colourless liquid, just as is the case with fuming nitric acid. Both this, as well as the fuming nitric acid, cannot be completely decomposed by mere dilution with water, for if the diluted solutions of both be kept for any length of time, they still decolorate the solution of the permanganate of potash. But by heating, nitric oxide is expelled from both, only in fuming nitric acid quicker. Both can be prepared by merely passing nitric oxide into sulphuric acid, or into nitric acid.

Berzelius* considers it quite as probable that the fuming nitric acid is a solution of a combination of nitric oxide with nitric acid, as that it is a combination of nitrous acid with nitric acid in excess of nitric acid; and the first view undoubtedly acquires greater probability from the existence of an analogous combination with sulphuric acid.

It now appeared to me still of interest to learn how it is that at present the sulphuric acid is so frequently contaminated with the sulphate of nitric oxide, for contamination with nitric acid perhaps seldom occurs. I diluted pure sulphuric acid with water so long till it had a spec. gr. of 1·2 (of which concentration it is generally drawn off from the lead chambers), and treated a portion of it with the sulphate of nitric oxide, a portion with a solution of the same in sulphuric acid, a portion with pure nitric acid, and another portion with fuming nitric acid. Upon this I heated each portion separately in a retort un-

* Berzelius's *Lehrbuch der Chemie.*

til sulphuric acid passed over, always obtaining pure sulphuric acid as the residuum; I had, however, especially with the addition of nitric acid, to continue the heating so long, that the remaining sulphuric acid had a spec. gr. of 1·84. Even when concentrated sulphuric acid is mixed with nitric acid, and the mixture exposed to a very gentle heat, sulphuric acid, almost pure, is obtained as the residue. If the concentrated acids be mixed together without any change of temperature taking place, and the mixture be left to stand for several weeks, no decomposition appears to occur; if large quantities be quickly mixed together, a trace of the sulphate of nitric oxide is formed, which probably arises from a disengagement of heat. But if the mixture be quickly heated in a retort, a decomposition ensues, the neck of the retort is filled with red fumes, and at first a sulphuric acid, containing nitric acid, distils over, then pure sulphuric acid, and in the residue remains sulphuric acid, containing the sulphate of nitric oxide dissolved. When the sulphuric acid is coloured by organic substances, and it is decolorated by warming and adding drop by drop nitric acid to it, it becomes contaminated with sulphate of nitric oxide.

According to this the sulphuric acid ought to occur in commerce free from every oxide of nitrogen, when it has a specific gravity of 1·84, of which strength it however rarely occurs, and only rendered impure by the sulphate of nitric oxide when it has been discoloured by nitric acid. Of late the sulphuric acid is concentrated in vessels of platina, which are so constructed that dilute sulphuric acid continually flows into the more concentrated, and this is probably the cause that the sulphate of nitric oxide is formed which then does not distil over.

Barruel* has proposed to digest the impure sulphuric acid over sulphur at 200° cent. in order to destroy the acids of nitrogen, and then to distil. But if the acid be distilled, then this is superfluous, for even did the sulphuric acid contain nitric acid, a pure acid would be obtained by distillation, which is even, as I have stated, obtained when one ounce of nitric acid is added to a pound of sulphuric acid, a contamination which occurs seldom if ever in commerce; it is merely requisite to change the receiver frequently. If the sulphuric acid contains the sulphate of nitric oxide, then a pure acid passes over immediately.

To procure a pure sulphuric acid for the preparation of hydrochloric acid, it is only necessary to mix it, (it matters not whether it be contaminated by the sulphate of nitric oxide or nitric acid, with two parts of water) and then to heat it in a retort until vapours of sulphuric acid pass over, whereby there is at the same time the advantage of having an acid of 1·85 sp. gr.

---

* Centralblatt, 1836. p. 315.

In the distillation of sulphuric acid several precautions have been proposed to avoid the sudden jerks (*stossweise*) in the boiling of the acid, yet most of them are by cautious equal heating, even the platina wire, unnecessary, and I have at last even distilled without this over an open fire, without ever meeting with an accident. But it is very necessary that the neck of the retort is not too long and is as broad as possible, and that the receiver does not lie immediately on the neck of the retort, but is separated from it by a platina wire, and that an equal fire be kept up, which is best attained by using charcoal. The retort is filled two-thirds with sulphuric acid, placed with the usual precautions in the sand bath, and at first a good fire kept up until strong vapours ascend from the acid; the fire is then diminished, and the acid comes to boil gently. Care should be taken, by keeping up an equal fire, that the acid does not cease boiling; should this happen, it is not necessary to interrupt the distillation, but the fire must be cautiously increased, that the acid may not enter suddenly into a too violent ebullition.

XVI. *A new Method of analysing the Ores of Iron, and Crude Iron. By Professor* Fuchs[*].

THIS process is founded upon the fact, that hydrochloric acid is incapable of dissolving metallic copper, provided the air be excluded; but if a piece of copper be put into a solution of a persalt of iron in hydrochloric acid, the persalt is converted into a protosalt, and a quantity of copper is dissolved, forming a protosalt of copper exactly equivalent to the quantity of peroxide of iron contained in the solution.

Into a solution of peroxide of iron in hydrochloric acid put a piece of copper, whose weight is accurately known; boil it well until no more copper is dissolved; weigh the undissolved copper after having cleaned and dried it, and thus ascertain how much copper has been dissolved. The quantity of peroxide of iron contained in the solution is to that of the copper dissolved as their respective equivalents, viz. as 40 to 31·7. Therefore if 31·7 copper be dissolved, it indicates 40 as the quantity of peroxide of iron originally contained in solution.

When the peroxide and protoxide are both present, two experiments are necessary to determine their respective quantities. The quantity of peroxide is ascertained in the manner just indicated. To determine the quantity of protoxide the whole of the iron must be brought into the state of peroxide, and from the amount of the peroxide thus obtained must be deducted the quantity of peroxide in the first experiment, and the remainder reduced to the protoxide by calculation.

---

[*] Extracted from the Transactions of the Royal Bavarian Academy.

The following precautions must be observed in order to obtain accurate results. The copper must be in a state of purity. The hydrochloric acid must be also pure, tolerably concentrated, and employed in excess. Nitric acid should not be used for bringing the iron to the state of peroxide, but either a stream of chlorine, or what is preferable, chlorate of potash. Upon adding the piece of copper to the solution it must immediately be brought to the boiling point, and kept there in order to prevent access of air. Previously to removing the undissolved copper from the solution, hot water is to be added till the vessel is quite full; this is to be poured off and fresh hot water to be added: the copper, which is generally covered with a brownish coating, is then carefully washed in cold water, dried at a gentle heat, and then weighed. The foregoing process has this advantage; that the substances ordinarily met with in iron ores,—such as silica, alumina, magnesia, lime, oxide of titanium, oxides of manganese, the phosphoric and sulphuric acids, &c.—do not in any way interfere with the accuracy of the results. An iron ore containing arsenic, however, cannot be analysed on this principle, as blackish gray scales of arseniuret of copper are deposited on the metallic copper.

This process is applicable to the examination of cast iron, or iron of other kinds, and also for the comparison of one sort of iron with another.

Among many experiments which were made, M. Fuchs relates the following.

Exp. 1. 50 grains of very soft English iron were dissolved in hydrochloric acid, and brought to the state of peroxide by chlorate of potash; 85·8 grains of pure copper were boiled in this solution, of which 56·2 grains were dissolved. Therefore as 31·7 : 28 :: 56·2 : 49·46, which is equal to 98·92 per cent. of pure iron; a repetition of this experiment gave 99·19 per cent.

Exp. 2. 50 grains of pianoforte wires were examined, which indicated 98·75 per cent. of pure iron. The iron in this experiment was peroxidized by a current of chlorine; a considerable deposit of carbon was formed during the process of solution in hydrochloric acid, which, however, disappeared upon passing chlorine through the solution.

Exp. 3. 50 grains of gray and white cast iron from Bergen were examined, and gave 94·33 per cent. of pure iron: the impurities, on examination, were found to consist of

Carbon ...	3·43
Silica	1·75
Phosphorus	0·37
Sulphur ...	0·12.
	5·67

Exp. 4. 70 grains of crystallized carbonate of iron from Lobenstein, examined, gave equal to 56·9 per cent. of protoxide, equivalent to 91·68 per cent. of carbonate of the protoxide; the other constituents consisted of carbonate of the protoxide of manganese and carbonate of lime and magnesia.

Exp. 5. 70 grains of specular iron ore from Gleissing in the Fichtelgebirge, gave upon examination a quantity equal to 92·30 per cent. of peroxide of iron, and 7·40 per cent of silica, leaving only a loss of 0·3 per cent.

Exp. 6. Crystallized magnetic iron, containing both protoxide and peroxide, was next examined. 50 grains were dissolved in hydrochloric acid, to which chlorate of potash was added to bring the whole of the iron to the state of peroxide. 40·71 grains of copper were dissolved, which is equivalent to 51·36 of peroxide of iron, or 102·72 per cent. Another portion of 50 grains was dissolved without the addition of chlorate of potash; the copper taken up was only 27·1 grains, equivalent to 34·2 grains of peroxide of iron, or 68·4 per cent.; this deducted from 102·72 indicated by the first 50 grains, gives 34·32, corresponding to 30·88 protoxide of iron: consequently this mineral gave 68·40 peroxide of iron
30·88 protoxide of iron
0·72 loss.

100·00

This process is applicable in many instances to the determination of the quantity of copper contained in ores of that metal.

The ore of copper is to be dissolved in hydrochloric acid, care being taken that the whole of the copper is converted into oxide or chloride; the solution is then to be boiled with copper (the necessary precautions having been first duly observed), until it assumes a pale olive-green tint, and becomes colourless on being diluted with water. If no oxide of iron be present, precisely the same quantity of copper will be transferred to the solution as was originally contained therein; the quantity of copper remaining is to be subtracted from the quantity of reguline copper employed in the experiment, in order to ascertain the amount of copper contained in the copper ore examined. 100 grains of pure malachite were examined in this manner, and gave equal to 57·5 of pure copper, which is very nearly the quantity that should be obtained; had the quantity of copper dissolved proved considerably less, it would have shown that the malachite was not in a state of purity.

XVII. *On the Electricity of Steam. By* JOHN WILLIAMS, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

EARLY in November last, I read in your Journal a communication from Mr. Armstrong, of Newcastle-upon-Tyne, on the subject of the electricity observed in a jet of steam issuing from the boiler of a high-pressure steam engine*. I felt much interested on reading this account, as I doubt not that it will eventually lead to some valuable discoveries relative to meteorology and atmospherical electricity, subjects which have occupied much of my attention for more than forty years past.

Volta's experiment of dropping a red-hot coal into a vessel of water placed on the insulated cap of an electrometer, was repeated by me in various ways and on a large scale; and from the results, I came to a conclusion that the positive electricity observed in the steam-cloud on its *immediate formation* was simply the effect of evaporation, and not, as some electricians have supposed, of a decomposition of the water into its primitive elements. And I imagine, that when steam under great pressure, and, consequently, of a high temperature, is liberated, and in the act of expanding and mixing with atmospheric air, it has its capacity for electricity increased; and that a similar process is continually in action in the ordinary course of evaporation from the surfaces of water, leaves of vegetables, and moist soil, with this difference; that in the latter natural processes the resulting electricity of the rising vapour is of low intensity, and not discoverable by our ordinary electrometers.

These reflections led me to the consideration of the cause of volcanic lightning, and particularly of the shape generally assumed by the enormous cloud of smoke, as described by Sir William Hamilton on viewing an eruption of Vesuvius.

---

* [Papers on this subject have appeared in our Numbers for November, December, and January; vol. xvii. p. 370, 375, 449, 452, and 457; present volume, p. 14, 50, and 95.—EDIT.]

With a view to find out the cause of volcanic lightning, I made some experiments with great care, by insulating a small portable furnace; and by doubling the electrical signs, by means of condensers, I found the insulated furnace to be in a negative state when the fire burnt with a strong flame; these electrical signs, however, were strongest after the addition of fresh fuel, and when *a dense smoke arose a few seconds before it burnt into flame**.

After the locomotive high-pressure engines came into use as public conveyances for passengers, I visited Manchester and Liverpool purposely to inspect the machinery and observe the steam- and hot-air cloud when issuing from the funnel; the weather being cold, and consequently the steam visible, I noticed the rapid divergence of the particles of vapour, and thought this might be caused by its electrical state, which I resolved to prove by experiment, but which I was not enabled to do till November 1838, when, having provided myself with a small close-covered boiler, made with strong sheet-copper, and furnished with a safety valve loaded with a weight, it was placed on a portable furnace, and insulated on a stool with glass legs, placed under a large spreading willow-tree in my garden, where I never could observe any signs of atmospherical electricity. The day was nearly calm. The following is a copy of the marginal note I entered in my meteorological journal:

" November 13th, 1838.—To-day I repeated a former experiment†, which fully confirmed my pre-conceived ideas, that when steam, confined under pressure in a boiler, is suddenly allowed to escape into the open air, the liberated steam has its capacity for electricity increased, consequently it leaves an insulated boiler in a negative state, which proved to be the case; for on raising the safety valve by means of a silk string and a piece of sealing-wax, *the gold leaves of the electrometer* opened one quarter of an inch with *negative electricity*."

I am, Gentlemen,
Your obedient servant,

Pitmaston, Worcester, 15th December, 1840. JOHN WILLIAMS.

---

* [It must be remembered, that the cloud, apparently of smoke, which issues from the crater of a volcano in eruption, is not really smoke, but *steam*, (mingled with finely-divided solid matter—volcanic sand,—consisting almost entirely of di-electrics,) issuing from an orifice in non-conducting matter. Volcanic lightning, therefore, is precisely analogous to the electric sparks given by effluent steam.—E. W. B.]

† The former experiment here alluded to was made by me many years ago, by opening a steam-cock on a large Watt's low-pressure engine boiler, and I found by my electrometer that the expanding steam-cloud was in a state of *positive* electricity.

XVIII. *On the Circumstances under which Steam developes Positive Electricity. By Dr.* CHARLES SCHAFHAEUTL*.

IN a conversation with Mr. Bradley of the Adelaide Gallery, he informed me that steam issuing under a pressure of about 40 atmospheres, from the boilers of Mr. Perkins's steam-gun, was able only slightly to move the gold leaves of a voltaic condenser. On my expressing an opinion that the electricity of the issuing steam had some relation to the formation of the deposit or incrustation in the boiler, he invited me to make some experiments at the Adelaide Gallery, in order, if possible, to elucidate this point.

As Mr. Perkins's boiler is constructed so as to prevent any incrustation forming, I selected for my experiments a common Marcet boiler, consisting of a globular iron vessel about 5 inches diameter, in the vertical axis of which is inserted a mercurial gauge, and at an angle of 45 degrees from it, a thermometer on one side, and on the other side a jet, with a stop-cock. The lower part of the boiler was of course occupied by quicksilver, on the top of which distilled water was poured to the height of 1¼ inch. In the direction of the jet a glass bell was suspended of 9 inches diameter and 5 inches in depth, so that the distance of the top of the bell from the orifice of the jet was about 9 inches. A bundle of copper wire was then attached at one end to the disc of a voltaic condenser, and the other end was spread out and inserted in the glass bell. The water in the boiler was then brought to a state of ebullition, and when the mercurial column had risen to 31 inches, the stop-cock of the jet was opened, the issuing steam condensed in the glass bell in great quantities, and the gold leaves of the voltaic condenser after the first few seconds separated instantaneously to their full extent, even under a pressure of 23 inches. This was likewise the case when the condensing plate was removed from the electroscope, and the gold leaves were always found positive electrified.

The steam under the same pressure directed against the copper wires without the bell was found to exhibit no trace of electricity.

The distilled water, which had assumed a red colour from the hydrate of oxide of iron derived from the inside of the boiler, was now changed for a saturated solution of common salt, which occupied a space of about three quarters of an inch on the top of the quicksilver; the other part of the experiment was then repeated as before, but no trace of electricity could be discovered.

---

* Communicated by the Author, whose former communications on the subject will be found in vol. xvii. p. 449, and in our last Number, pres. vol. p. 14.

After the greater part of the water had been evaporated the boiler was opened, the deposited salt removed, and after being well washed filled again with distilled water. After the steam had reached a pressure of 31 inches, as in the first instance, the stop-cock was again opened as before, and the jet of steam was directed against the inside of the glass bell in all its varied positions, but the gold leaves of the condenser remained motionless, or when positively electrified had a tendency to collapse.

I was obliged to fill the boiler again, and now took care to have the same quantity of water as in the first experiment. On the second trial the gold leaves separated again. On the third trial the jet of steam touched the outside as well as the inside of the glass bell. The fourth succeeded as in the first experiment, and afterwards no effect could be produced.

The boiler was again filled, and as I thought I had observed that the gold leaves separated only when the steam issued from the jet with a peculiar rushing fluttering noise, quite different from the hissing sound which generally accompanies the escape of pure steam, I therefore directed my particular attention to it in this experiment.

The water was at the height of about 1½ inch, and as soon as the mercurial column had reached 32 inches I opened the stopcock with one hand, bringing with the other hand the axis of the glass bell in the direction of the jet of steam. The rushing sound was immediately observable, and the condensed water streaming from the edges of the bell, the gold leaves separating at the same moment to their extreme angle. After I had discharged the disc of the electroscope the leaves again opened several degrees, which happened likewise the third and fourth time, as often as the vibrating bell came into contact with the wires. The wetted interior of the bell had, therefore, not only obtained, but also retained, a small charge long after the steam had ceased to flow, and I was now able to produce the desired effect as often as I pleased, the success depending entirely on that state of the escaping steam which causes the rushing or fluttering noise which happened only when the boiler was filled at about 1½ inch, and ceased when the boiling sheet was reduced, the escape of steam without that peculiar sound, even under a pressure of 32 inches, causing the separated gold leaves to collapse.

The temperature of the glass bell seemed to be of little consequence, as the experiment succeeded on the first trial when the glass was quite cold, as well as when the bell had become warm after repeated trials; the only requisite being that peculiar condition of the steam which produces the above-mentioned rushing noise. Now this noise appears to be caused solely by the sudden boiling of the water, and the conversion of a portion of it into a fine spray, as the steam issuing under those circumstances and splashing against the interior of the bell deposits a large quantity of water running down in drops, and often in streams, from the edge of the bell.

The boiler was insulated and the stop-cock was opened with a dry folded silk handkerchief, but the omission of all these precautions did not in the slightest degree affect the results; a proof that the electricity of the steam developed in the glass bell could not be contained in the steam during its passage through the 3-inch long metallic jet of the boiler, as all electricity would have been deposited in this narrow metallic passage. But the condensation of steam even into the form of mist, seems not to be sufficient to produce electricity; the condensation into liquid water seems to be indispensable, at least in the present experiment, where the electricity appears to be due to the deposit of liquid water in the bell solely, or perhaps in relation to the separation of this liquid water from the steam.

The circumstance of steam in a state of mist not being capable of exhibiting traces of free electricity, seems to afford us some clue to the elucidation of a hitherto mysterious phænomenon, viz. that only certain clouds are capable of producing thunder storms. A common cloud consisting only of moisture seems to be analogous to a pure jet of steam in the glass bell, both consisting of minute hollow water globules or bubbles, leaving only a small deposit of moisture in the glass bell, or in the air, which finally collects into small drops of rain. But when the steam deposits rapidly a great quantity of liquid water which in a thunder-cloud produces those well-known heavy showers, electricity is set free in great quantities, so that a jet of issuing steam from Marcet's boiler in three seconds produced the same effects upon the gold leaves of the electroscope as a feeble spark from an electric machine with a glass plate of 9 inches, produced in damp weather. I have here only to observe, that the sudden separation of water in drops in thunder-clouds seems to be caused, as I expressed on a former occasion, by a sort of sudden compression and cooling caused by the vehement currents of air towards the centre of the thunder-cloud; because I found when I was immersed in a thunder-cloud, that the hygroscope during the increase of the wind rose to the highest degree of wet; the thermometer at the same time falling, which was followed immediately by a separation of water and a flash of lightning, either at the same moment, or following each other at short intervals. It therefore appears that the column of vapour rising from the crater of a volcano seems to be in a similar condition to the fluttering steam issuing from the jet of Marcet's boiler, and the flashes of lightning attributable to the separation of the liquid water from steam and smoke, as I hinted in my first communication on this subject, before I was enabled to make these experiments.

In continuing the above-mentioned experiments, I found that the bundle of wires connected with the electroscope for the absorption of the supposed electricity of the steam, might be dispensed with, and nothing more was necessary than to hold the glass bell against the jet of issuing steam; and as soon as the peculiar rushing noise commenced, the jet of steam at the same time changing from transparency to a milk-white opacity, the interior of the bell became immediately charged with electricity, whether the bell was 9 or 18 inches distant from the metallic jet. If the inside of the bell be brought whilst in this state into contact with the knob of the electroscope, the gold leaves immediately diverge very widely, and by repeating this experiment several times, a spark may easily be obtained.

A copper wire inserted through the jet into the boiler, forming the axis of the pencil of issuing steam, such wire approaching even within an inch of the glass bell, did not in the slightest degree affect the results described; a further proof that the free electricity which manifested itself was not contained in the issuing steam, but was developed during condensation in the glass bell. A tinfoil coating on the outside of the bell, reaching within an inch and a quarter of the edge, very considerably diminished the quantity of electricity. When an inch and a half of this coating was removed, the bell acted the same as when entirely uncovered, which may, perhaps, be simply ascribed to the steam on rushing out of the bell coming into contact with the foil when it is so near the edge.

If the water in the boiler was saturated with common salt or with sulphate of lime, and even a slight excess of sulphuric acid, the angle between the diverging gold leaves remained the same as if distilled water was used.

From these experiments we may safely infer that the observed free positive electricity in this case was solely attributable to the sudden condensation and separation of water from steam, caused in my experiment by its coming into contact

GENERAL LIBRARY University of MICHIGAN

with the internal surface of the glass bell, on the superficial contents of which the quantity of electricity developed greatly depends. On a larger scale, as in a steam-engine, the air in which the cloud originates has the same function as the glass surface. In thunder storms the currents of air rushing towards the centre of the nascent cloud, produce the peculiar sudden condensation and separation of water-gas similar to the pencil of steam issuing with the fluttering noise already mentioned from the jet of the boiler.

I must here particularly impress on the reader, that as the pencil of steam developes electricity only under certain circumstances, that is when steam is mixed with minutely-divided water before its expansion, so the thunder-cloud must likewise be in a similar state in respect to its water-gas and minutely-divided water, quite different to the state of a rainy atmosphere, where the condensation of the watery vapour into the shape of clouds commences only in the highest and coldest regions, uniformly and gradually, and where during their descent at certain intervals, from the highest to the lowest regions, the atmosphere begins gradually and uniformly to discharge moisture until the hygrometer indicates the point of saturation at which the formation of drops commences.

The electricity developed by evaporation, &c. could never manifest itself in a metallic boiler, and is under the most favourable circumstances so feeble, that its existence was long denied by the most eminent philosophers, until Dr. Harris succeeded unequivocally in demonstrating its presence. This electricity is likewise much influenced by chemical actions ensuing at the same time, as M. Pouillet has distinctly proved, which was not the case in my experiments.

With the positive electricity obtained from a jet of condensed steam, negative electricity is at the same time developed in the boiler and water, and as far as I was able to judge, of the same intensity. Negative electricity derived from a locomotive engine must necessarily be affected by the chemical process of combustion going on at the same time on a very extensive scale, as well as from various other causes, such as imperfect insulation, points, and sharp edges, and I was not able to detect in my small boiler any traces of negative electricity, if at the same time positive electricity did not appear in the glass bell.

XIX. *On the Phænomena of the Electricity of Steam, observed by* Mr. Pattinson *and* Mr. Armstrong. *By* M. PELTIER*.

MY experimental and meteorological pursuits† having led me to study the circumstances which impart electricity to vapours, I am enabled to add some facts calculated to afford the explanation of the phænomenon of Cramlington.

When a thick piece of platina slightly convexo-concave (embouti), is made red-hot, and water poured upon it, a part of the phænomena which take place is known; therefore I shall only mention them, although the true explanation of these movements has not, in my opinion, been yet given; I propose to revert to this subject at another time.

If the bottom upon which the drop of water rests is flat or very little convexo-concave, it takes the figure of an ellipsis, as has been very correctly observed by M. Auguste Laurent (*Annales de Chimie et de Physique*, vol. lxii. p. 327). This ellipsis returning upon itself, there is formed from it another which is perpendicular to it: this one being depressed in its turn, the first one re-appears, and so afterwards. After some moments the elongation of the ellipses diminishes; but then they are multiplied: others are formed from them at 45 degrees from the first, making four, marching two by two. The temperature diminishing, the sallies grow feeble; a greater number occur, but they are small, no longer possessing regular and successive movements. These tumultuous movements resolve themselves into a gyratory movement which has its axis vertical, round which is seen a crenelated crown, the vestiges of the ellipsoidal elongations which are disappearing. A little later this movement diminishes in velocity, then it stops, and the drop seems to be in a state of repose, and vividly reflects the light. The form of the collective movements of these elongations is in part produced by exterior resistances themselves depending on the curvature of the crucible, and on the size of the drop projected.

On examining the drop of water with a lens during its rotation, there are seen in the interior several kinds of movements. When the rotation stops, there is a moment of suspense in the resultant, which must arise from interior movements; another rotation is then produced, the axis of which is horizontal, and which leaves the superficies perfectly even. This collective movement lasts but a short time; several others established themselves in the interior; we see them jostle one another, press on each other in different directions, move in array again together once more, then again to divide themselves. At last the movement slackens: the drop, which until then had retained its globular form and had not moistened the platina, flattens, wets it, and evaporates all at once, without any electricity having been produced, if distilled water and a clean crucible have been used.

---

* *Ann. de Chim. et de Phys.*, Nov. 1840, p. 330. See the preceding articles.

[† M. Peltier has lately published an interesting meteorological treatise, "*Observations et Recherches sur la Formation des Trombes*; Paris, 1840;" in which he ascribes to water-spouts an electrical origin.—ED.]

If instead of pure water a weak solution of sea salt be used, the effect will be the same the first time; but the salt left by the water having formed a slight layer on the platina, is again taken up by the water used for the succeeding experiment: the strength of the solution thus increases, and when the drop is diminished two-thirds it is become almost opake; a multitude of minute bodies are seen swimming in the inside, and soon after some decrepitations are heard, accompanied with saline projections. At this moment the needle of the electrometer indicates a negative tension. If the platina has become cool enough to admit of moistening, the decrepitation ceases, the drop spreads, and it is immediately transformed into vapour; the electricity which had just been developed during the decrepitation, instead of being doubled by this sudden change into vapour, disappears with the vapour, which carries it off with it. The saline layer being increased, the effect at the third experiment is greater; the decrepitation is stronger, and the needle is projected to a distance. Thus, before and after the decrepitation there is no electricity produced; the instrument remains mute, whatever be the quantity of vapour produced.

From the preceding observation, it was natural to suppose that the salt, being substituted for the salt water, would produce the same effect; this is what the experiment has verified. The chloride of sodium decrepitating without aqueous fusion, the water interposed plays the part of saturated solution, and sets in motion the needle of the electrometer. If it be a salt containing water of crystallization, as the nitrate of ammonia so easy of decomposition, there is at first the aqueous fusion, a great evaporation without producing electricity, then at last comes the decrepitation, and the needle is strongly projected by negative electricity.

It is then at the moment of the separation of the combined molecules of water that electricity is produced; it is at the moment that a chemical decomposition takes place, and not during the separation of the excess of water. The application of these experiments to the phænomenon observed at Cramlington, is sufficiently obvious: it appears by the accounts given, that it was necessary for the water to be saturated to such a degree as to have a deposit formed on the side of the boiler; that a high temperature is requisite as the machine is a high-pressure one; that the electricity increases with the deposit, and that it varies with the temperature.

This phænomenon only appearing when there is a saline coat deposited, and its tension increasing with the thickness of the layer, it will serve to make known the degree of internal incrustation and the sudden variations of temperature.

XX. *On a new Fat Acid in the Butter of Nutmegs. By* LYON PLAYFAIR, *Ph. D.*[*]

THE butter of nutmegs has been the subject of several examinations; for the best account of its properties, however, we are indebted to Schrader. This chemist has shown that it is a compound of three oils, two of them being solid, the other volatile and liquid. He has also examined the proportional quantities of these, and described the methods by which they might be separated from one another.

Lecanu[†] observed that this butter possessed different properties from other vegetable fats, and approached more nearly in character to those of animals. He remarked likewise its partial solubility in æther, which had formerly been pointed out by Schrader as a distinguishing mark.

Pelouze and Boudet[‡] described a method by which *margarine* could be procured in a state of purity, and mentioned that the same margarine existed in the butter of nutmegs; but they have given no experiments in proof of this, nor did they state analyses of the products obtained.

None of the chemists now mentioned have published the numerical results of their examinations. Hence it was uncertain whether the acid existing in the butter of nutmegs was really margaric acid, or some other acid resembling it in properties. It was interesting to ascertain the exact composition of this *margarine*, and for this purpose the following examination was undertaken.

When the butter of nutmegs is digested with alcohol of the common strength, it is decomposed; the alcohol dissolves a coloured fat, acquires a wine-red colour; and yields by evaporation a red, soft, semi-fluid fat of an agreeable odour of the nutmeg. Part of the butter remains undissolved; a small portion of it does dissolve, but is again precipitated upon cooling. When the alcohol is very strong, the butter dissolves in four times its weight (Schrader).

---

[*] Communicated by the Author; having been read before the British Association at the late meeting at Glasgow.

[†] *Journal de Pharmacie*, t. xx. p. 339.

[‡] *Annales de Chimie et de Physique*, t. lxix. p. 47.

The fat, which remains undissolved, is very impure, and, even after several digestions, still retains the odour of the butter. It must therefore be subjected to strong pressure within folds of bibulous paper, first by treating it with alcohol, and afterwards with æther, and renewing the pressure after each treatment. The solution in æther must be filtered whilst hot, in order to get rid of the impurities. When the fat has attained a constant melting point of 31° C., it may be considered pure.

Care must be taken in the selection of the butter, for that sold in commerce under the name of "butter of nutmegs," sometimes consists of animal fat boiled with powdered nutmeg, and coloured with sassafras. The specimen may be relied on as pretty pure, if it dissolves in four times its weight of strong boiling alcohol, or half that quantity of æther.

The fat obtained as described above is oxide of glyceril in combination with a fat acid, which, as far as I am aware, has never hitherto been described. It has a beautiful white silky appearance. From this property (which is shared also by the acid derived from it) I propose to call it *Sericine* (from the Latin word *serica*), as I am desirous at present to give it no name exclusively applying to its origin; for researches now in progress of being made by another, appear to show that it is not confined to this butter, but also exists in others.

Sericic Acid.—The acid to which this name is applied, may be obtained by the saponification of *sericine*. The soap must be washed with cold water, in order to free it from the alkali employed in its saponification. It is now to be dissolved in boiling water, and muriatic acid added until the liquid possesses an acid reaction. The sericic acid now separates as a colourless oil, which solidifies to a crystalline fat on cooling. It must be washed with water, in order to free it from muriatic acid, and then repeatedly melted in fresh portions of pure distilled water.

Thus obtained, it possesses a snow-white colour and crystalline appearance. It is very soluble in hot alcohol, from which it is partly deposited in small crystals on cooling; the remainder may be obtained by further evaporation. In hot æther it dissolves in considerable quantity, but separates almost entirely on cooling. When allowed to crystallize slowly from alcohol by spontaneous evaporation, it is semi-transparent and highly crystalline. Its melting point is between 48½° and 49° C.

The atomic weight of the anhydrous acid obtained by the analysis of the salt of silver, is 2733·27; the mean of two analyses of the salt of barytes gave the atomic weight 2732·54. The formula of the anhydrous acid deduced from the analyses of its salts is $C_{28}\,H_{27}\,O_3$.

The formula for œnanthic acid is $C_{14}\,H_{13}\,O_2$; sericic acid may, therefore, be considered to have the same composition as œnanthic acid, in which one equivalent of oxygen is replaced by one of hydrogen.

2 atoms of œnanthic acid $= C_{28}\,H_{26}\,O_4$
1 atom of sericic acid $= C_{28}\,H_{27}\,O_3$

It may be compared, in this respect, to benzoic acid and the hydruret of benzule.

The anhydrous acid has not been procured in an isolated state; the acid procured by the decomposition of sericate of potash by muriatic acid, is the hydrate, and contains one atom of water. The following are the results of the analyses of this hydrate.

I. 0·351 gramme of substance, burned with oxide of copper, gave 0·389 gramme water, and 0·941 gramme carbonic acid.

II. 0·309 gramme of substance gave 0·342 water, and 0·829 carbonic acid.

III. 0·412 gramme of substance, burned with chromate of lead, gave 1·101 carbonic acid, and 0·454 water.

IV. 0·250 gramme of substance gave 0·670 carbonic acid, and 0·276 water.

V. 0·278 gramme of substance gave 0·744 carbonic acid, and 0·309 water.

	I.	II.	III.	IV.	V.*
Carbon ...	74·12	74·06	73·89	74·10	74·00
Hydrogen	12·31	12·29	12·24	12·26	12·02
Oxygen ...	13·57	13·65	13·87	13·64	13·98

These numbers approach closely to the formula $C_{28}\,H_{28}\,O_4$.

Atoms.			In 100 parts.
28	Carbon	2140·18	74·06
28	Hydrogen...	349·42	12·09
4	Oxygen......	400·00	13·85
		2889·60	100·00

---

* The analyses Nos. I. II. IV. were made upon sericic acid repeatedly crystallized from alcohol; IV. was kindly executed by Mr. Miller, assistant to Professor Daniell; III. V. were made upon the acid obtained by decomposing sericate of soda with muriatic acid, with precautions formerly described.

The formula for the anhydrous acid is $C_{28}\ H_{27}\ O_3$; hence the formula for the hydrate is $C_{28}\ H_{27}\ O_3 + HO$.

There are several points besides its composition, which distinguish this hydrate from that of margaric acid, for which it has hitherto been mistaken. Its melting point is very different from that of margaric acid, and it is soluble in almost every proportion in hot alcohol. The soaps which it forms with potash or soda dissolve with greater facility than the corresponding soaps of margaric acid; they are also more crystalline in their appearance. Sericic acid does not appear capable of forming two classes of salts with the alkalies, that is, its salts are always neutral, and the soaps may be treated with water without passing into supersalts; a character possessed neither by stearic nor margaric acid.

The solution of sericic acid in alcohol strongly reddens litmus paper. When it is boiled with nitric acid diluted with half its weight of water, a violent action ensues, and peroxide of nitrogen is evolved in considerable quantity. The products of the decomposition appear to be soluble in water, for the acid remaining after the action had ceased was found unchanged; at least the salts of barytes and silver made from it possessed a composition precisely similar to that of the original acid; the melting point also remained the same. The subject was not more nearly examined.

Sericine.—The process for obtaining this substance has been described at the commencement of the paper; it is the solid part of the butter of nutmegs, and has been described by MM. Pelouze and Boudet as *margarine* (margarate of the oxide of glyceril).

Sericine is a very beautiful fat (when crystallized from æther), having a snow-white colour, and silky lustre. It is soluble, in all proportions, in hot æther; but the greater part crystallizes on cooling: in water it is quite insoluble. It is not easily saponified by caustic alkaline ley of the common strength, a property which distinguishes it from margarine; but it is easily converted into a beautiful white soap, by melting it with hydrate of potash and a very small quantity of water.

For the purpose of determining with what base sericic acid is combined in sericine, the following process was adopted. A quantity of sericine was boiled for several days with basic acetate of lead. An insoluble salt of lead was thus formed with the acid: the base must, therefore, have been separated. A stream of hydrosulphuric acid was now passed through the liquid filtered from the salt, until all the lead was precipitated. Hence the liquid could now only contain the base with which the acid had been combined, together with acetic and hydrosulphuric acids. During the evaporation of the liquid, the two latter were expelled, and a thick straw-coloured liquor of a syrupy consistence and sweet taste remained, which possessed all the common characters of the oxide of glyceril.

The following are the results of the analyses of sericine.

I. 0·3045 gramme of substance yielded 0·344 gramme of water, and 0·832 gramme carbonic acid.

II. 0·406 gramme of substance yielded 0·452 gramme of water, and 1·104 gramme of carbonic acid.

III. 0·310 gramme of substance yielded 0·341 gramme of water, and 0·847 gramme of carbonic acid.

	I.	II.	III.
Carbon	75·55	75·19	75·55
Hydrogen ...	12·18	12·36	12·22
Oxygen	12·27	12·45	12·23
	100·00	100·00	100·00

Lecanu has endeavoured to show that stearin (the stearate of the oxide of glyceril) is composed of two atoms of stearic acid, and one atom of a peculiar oxide of glyceril represented by the formula $C_6\,H_6\,O_4$. But Pelouze has proved that the common oxide of glyceril should be represented by the formula $C_6\,H_7\,O_5$. Meyer, in his researches upon elaidic acid, has also shown that Lecanu's formula for glyceril is correct in some combinations of the fat acids; although it may reasonably be questioned whether it possesses this composition in stearin; for Liebig has shown it to possess the formula $2\,\underline{\mathrm{St}} + \mathrm{Gy}\,\mathrm{O} + 2\,\mathrm{HO}$. Liebig does not deny that other oxides of glyceril may exist besides that expressed by the formula $C_6\,H_7\,O_5$; on the contrary, he suggests that there may be several, which may unite with one, two, or three atoms of anhydrous acid, just as there are acids which unite with one, two, or three atoms of a base. But very little is known regarding the nature of glyceril.

Sericine may be an example of such a combination, containing an oxide of glyceril, $C_6\,H_5\,O_3$, capable of uniting with four atoms of an anhydrous fat acid. These different glycerins would be formed by the removal of one or two atoms of water from the common oxide of glyceril. The hydrogen shown by the analysis is a little too high to countenance this idea; but this may be partly accounted for by the æther with which it is prepared, and which adheres to it with much obstinacy. The formula would be $4\,(C_{28}\,H_{27}\,O_3) + (C_6\,H_5\,O_3)$.

	Atoms.		By calculation.
Carbon	118	9019·33	75·65
Hydrogen ...	226	1285·37	11·82
Oxygen	15	1500·00	12·53
		11804·70	100·00

Sericate of the Oxide of Ethyle.—This compound may be formed by sending a stream of muriatic acid gas through a concentrated alcoholic solution of sericic acid. The solution must be kept boiling, in order to ensure the complete decomposition of the chloride of ethyle by the fat acid. After some time, sericic æther collects upon the surface of the alcohol as a colourless oil, which may be purified by simple agitation with distilled water. The agitation with water must be continued until no further smell of muriatic æther can be perceived. It may be rendered still purer, by digesting it with a solution of carbonate of soda, in order to remove any excess of sericic acid; but in this case a considerable portion of the æther is lost. If the stream of muriatic acid be continued sufficiently long, such a precaution is unnecessary. The æther cannot be purified by distillation, as part of it, in this case, appears to be decomposed.

When sericic æther is obtained in the manner now described, it is an oily transparent fluid at common temperatures, destitute both of colour and smell; but when the muriatic æther has not been carefully removed, it possesses a pale straw-yellow colour, and a faint odour. It is lighter than water, having a specific gravity of 0·8641. It may be obtained in beautiful white crystals, by surrounding the liquid with a freezing mixture.

Sericic æther is insoluble in water, but soluble both in alcohol and æther: when boiled for a considerable time with an alcoholic solution of caustic potash, it is decomposed. The following are the results of its analysis, which was performed in the same way as that of other liquid bodies of small volatility.

I. 0·243 gramme of substance, burned with oxide of copper, yielded 0·653 gramme carbonic acid, and 0·273 gramme water.

II. 0·199 gramme of substance, burned with oxide of copper, yielded 0·535 gramme carbonic acid, and 0·221 gramme water.

	I.	II.	Atoms.	By calculation.
Carbon ...	74·30	74·34	60 = 458·61	74·75
Hydrogen	12·48	12·34	60 = 748·77	12·20
Oxygen...	13·22	13·32	8 = 800·00	13·05
	100·00	100·00	6134·87	100·00

Prof. Redtenbacher has shown* that stearic æther is a compound of stearate of the oxide of ethyle with the hydrate of stearic acid, and represented by the formula $2\,\underline{St}\,Ae\,O + 3\,HO$. As stearic acid is a bibasic acid, four atoms of base are combined with two of acid. Sericic æther is a body of an analogous composition, and forms a true double salt—sericate of oxide of ethyle and sericate of water. Its formula therefore, is $(Se + \underline{Ae}\,O) + (Se + \underline{H}\,O)$.

Two atoms of sericic acid ... $= C_{56}, H_{54}, O_6$.
One atom of oxide of ethyle $= C_4, H_5, O_1$.
One atom of water ... ... $= H_1, O_1$.

Sericate of the oxide of ethyle $= C_{60}, H_{60}, O_8$.

The formation of sericic æther may be easily explained; the muriatic æther, formed by passing muriatic acid gas through a solution of sericic acid in alcohol, is decomposed by the fat acid; sericic æther is thus produced, and attaches itself to an equivalent of the undecomposed hydrate.

Sericate of Barytes.—This salt may be prepared from sericate of potash, by adding an alcoholic solution of the latter to a pure salt of barytes. A bulky white precipitate is thus obtained, which must be thrown on a filter, and well washed. It is slightly soluble both in water and in alcohol; in the former it possesses nearly the same degree of solubility as gypsum.

I. 0·797 gramme of the salt left, after ignition, 0·266 gramme carbonate of barytes; and 0·858 gramme, burned with chromate of lead, yielded 0·691 gramme water, and 1·702 gramme carbonic acid.

II. 0·481 gramme of the salt left, after ignition, 0·161 gramme carbonate of barytes; and 0·319 gramme, burned with oxide of copper, yielded 0·257 gramme water, and 0·634 gramme carbonic acid.

	I.	II.	Atoms.		By calculation.
Carbon ...	56·91	57·09	28	2140·18	57·32
Hydrogen	8·94	8·95	27	336·94	9·02
Oxygen...	8·26	8·09	3	300·00	8·04
Barytes...	25·89	25·97	1	956·88	25·62
	100·00	100·00		3734·00	100·00

The absolute quantity of carbon obtained in the analysis was under that stated: in the first 54·85 is the quantity shown by the analysis, but 2·06 per cent. must be added for the carbonic acid retained by 25·89 of barytes, as carbonate of barytes. In the second also 54·95 is expressed by the analysis, but 2·14 being added for the carbon retained by 25·97 barytes, gives the number stated above.

---

* *Annalen der Chemie und Pharmacie*, xxxv. 1.

The mean of the two analyses makes the quantity of barytes contained in the salt 25·98 per cent.: the quantity found by calculation is 25·62.

Sericate of Silver.—This salt may be prepared by double decomposition in the same manner as sericate of barytes.

When it is newly precipitated, it is a bulky white powder, which speedily acquires a lilac colour by exposure to light. To avoid this it must be washed in the dark. It is insoluble in water, but very soluble in a solution of caustic ammonia, from which it may be obtained, by spontaneous evaporation, in large transparent colourless crystals. It melts upon the application of heat, but at the same time undergoes decomposition.

I. 0·361 gramme of substance, burned with oxide of copper, gave 0·267 water, and 0·646 carbonic acid.

II. 0·340 gramme of substance, burned with oxide of copper, gave 0·610 carbonic acid, and 0·243 water.

III. 0·553 gramme of substance, burned with oxide of copper, gave 0·992 carbonic acid, and 0·4015 water.

0·704 gramme of salt left, after ignition, 0·277 metallic silver.

	I.	II.	III.
Carbon.........	49·48	49·61	49·60
Hydrogen......	8·03	7·94	8·06
Oxygen.........	7·82	7·78	7·67
Oxide of silver	34·67	34·67	34·67
	100·00	100·00	100·00

If we suppose the salt to be composed like that of barytes of one atom of sericic acid and one atom of oxide of silver, then the numbers given below should represent the analysis.

			By calculation.
28	Carbon	2140·18	50·61
27	Hydrogen......	336·94	7·94
3	Oxygen	300·00	7·13
1	Oxide of silver	1451·61	34·32
		4228·73	100·00

The difference between the carbon found by analysis and calculation is very considerable, but the close correspondence between the hydrogen and the oxide of silver leave little doubt that the formula is the same as that of the salt of barytes. At the same time it is proper to mention that the close correspondence between the analyses themselves, which were executed on salts prepared at different times, and with scrupulous attention to their purity, lead us to question whether some other formula should not be adopted for it. Salts of silver are generally anhydrous, but this does not always appear to be the case; for Crasso has lately described* a salt of silver which contains water. Sericate of silver may be an example of a similar compound, corresponding to Johnston's sulphate of lime, in which two atoms of salt are united with one atom of water. This is merely brought forward as a conjecture, want of material having prevented my making any further experiments on this subject. The calculated result, however, agrees pretty closely with the analysis.

			By calculation.
56	Carbon	4280·36	49·94
55	Hydrogen......	686·37	8·00
7	Oxygen	700·00	8·19
2	Oxide of silver	2903·22	33·87
		8569·95	100·00

Hence the formula would be $2\,\underline{\mathrm{Se}}\,\mathrm{Ag\,O} + \mathrm{H\,O}$.

Sericate of Potash.—This compound may be prepared by melting pure carbonate of potash and sericic acid with a small quantity of water. The mixture must then be gently heated, in order to effect the complete saponification of the acid, and afterwards evaporated to dryness on the water-bath. The residue is now to be digested with absolute alcohol, which dissolves the sericate of potash, but leaves the carbonate of potash undissolved.

Sericate of potash is very soluble both in hot and cold alcohol and water. When it is dissolved in a hot solution of alcohol, it is partly deposited on cooling in the form of beautiful white crystalline scales. It is insoluble in æther.

We possess no data for determining how we should estimate the carbonic acid in the analysis of a salt of potash formed by an organic acid. Liebig states, in his treatise on Organic Analysis, that the potash remains after the combustion as a carbonate; and consequently that an atom of carbon should be added to the result of our analysis. But experiments made on this subject by Prof. Redtenbacher, Dr. Varrentrapp and myself, show that this cannot be relied on, when the salt is burned with oxide of copper. Dr. Varrentrapp mixed the carbonates of potash and soda with oxide of copper, and exposed them to the action of heat in a common tube of combustion. As the result of his experiments, he ascertained that a quantity of carbonic acid always passed into the apparatus, containing potash. This gain was generally about one-third of the potash employed, two-thirds still remaining in combination with the alkali. By subtracting this from the weight of the carbonic acid obtained in the experiment, and calculating the potash as a neutral carbonate, we cannot be far from the truth. This has been done by Redtenbacher * in his researches upon stearic acid, and is the only one at present which can be adopted.

---

* Liebig's *Annalen*, Band xxxiv. 1. 79.

I. 0·354 gramme of substance gave 0·324 water, and 0·797 carbonic acid.

II. 0·324 gramme of substance gave 0·296 water, and 0·727 carbonic acid.

0·404 gramme gave 0·130 sulphate of potash.

The first is equal to 62·25 per cent., the second 62·04 per cent. carbonic acid; but on the supposition that the potash remains after the combustion, as a carbonate, then 2·25 per cent. of carbon must have remained with it. Only two-thirds of this, however, should be added to the carbon actually procured in the analysis; hence the result is as follows:

	I.	II.	Atoms.		
Carbon ...	63·75	63·54	28	2140·18 ...	63·56
Hydrogen	10·16	10·15	27	336·94 ...	10·00
Oxygen ...	8·70	8·92	3	300·00 ...	8·92
Potash ...	17·39	17·39	1	589·91 ...	17·52
	100·00	100·00		3367·03	100·00

The formula of the salt is $\underline{Se}+KO$. Before sericate of potash is subjected to analysis, it must be repeatedly dissolved in water and evaporated to dryness, for the alcohol employed in its preparation adheres to it with so much tenacity that it cannot be expelled, unless this process is adopted.

Sericate of Soda.—This salt may be prepared in the same manner as the last. It is soluble in water and alcohol, but insoluble in æther. For the reasons already stated under "sericate of potash," the salt of soda was not analysed, as its analysis could lead to no satisfactory results.

Sericate of Lead.—This salt was obtained by digesting sericine for several days with basic acetate of lead ($\underline{Ac}+6\,PbO$). It is a dense white powder, insoluble in water, and insoluble or very slightly soluble in alcohol. After digesting for a considerable time, the salt must be thrown on a filter, and washed very carefully with water. The lead was estimated by igniting the salt, ascertaining the weight of the lead, and oxide of lead, which remained; washing the mixture with weak acetic acid by decantation, and calculating the loss as oxide of lead.

---

* *Annalen der Chemie und Pharmacie*, xxxv. 1.

It has been shown* that both benzoate and margarate of lead contain acetic acid in chemical combination, and not as a mere accidental constituent arising from imperfect washing. Hence it was necessary to ascertain whether or not sericate of lead contained this acid. For this purpose a few grains of the salt were moistened with alcohol and sulphuric acid, and subjected to distillation. Acetic æther passed over and was easily recognized by its well-known properties. The quantity of acetic acid contained in the salt, appeared to be very small.

0·985 gramme of salt left, after ignition, 0·421 gramme of lead and oxide of lead; of this 0·55 gramme was oxide of lead, and 0·366 gramme metallic lead: both together are equal to 45·58 per cent. of oxide of lead.

0·456 gramme, burned with oxide of copper, gave 0·684 gramme carbonic acid, and 0·273 gramme water.

		Atoms.		By calculation.
Carbon	41·48	116	8866·46	41·21
Hydrogen...	6·65	111	1385·22	6·48
Oxygen	6·29	15	1500·00	6·94
Oxide of lead	45·58	7	9761·50	45·58
	100·00		21513·18	100·00

We have already seen that sericine contained four atoms of sericic acid, and one atom of oxide of glyceril. In the sericate of lead made from sericine, the oxide of glyceril appears to be displaced by one atom of basic acetate of lead $(\underline{Ac}+3\,Pb\,O)$.

4 atoms of neutral sericate of lead $= C_{112}+H_{108}+O_{12}+Pb\,O_4$
1 atom of basic acetate of lead $= C_4+H_3+O_3+Pb\,O_3$

1 atom of sericate of lead $= C_{116}+H_{111}+O_{15}+Pb\,O_7$

The formula would therefore be $4\,(C_{28}\,H_{27}\,O_3+Pb\,O)$
$+\,(C_4\,H_3\,O_3+Pb\,O_3)$.

Another salt of sericic acid and oxide of lead may be obtained by adding acetate of lead to a solution of sericate of potash. But from the great disposition of oxide of lead to form basic salts, a mixture of salts appears, in this case, to be formed; at least the results of the analysis of a salt thus obtained showed no definite composition*.

---

* *Annalen der Chemie und Pharmacie*, xxxv. 1.

Before concluding this paper it may be interesting to mention a few facts connected with the butter of nutmegs. It has been already stated, that when this butter is digested with alcohol of the common strength, a soft fat of a red colour is dissolved: when this fat is distilled with a large quantity of water, a colourless oil passes over. This has an agreeable pungent odour, and is probably the same as that described by John and others.

But if this red fat be subjected to dry distillation, several interesting compounds are formed, which, however, have not been examined more closely. The same oil passes over as that obtained by distillation with water, but as the heat increases it is accompanied by a white crystalline fat, which after being purified presents the characters of paraffin. A black matter like humus remains in the retort, and may be easily saponified by continued digestion with caustic potash; when the soap thus formed is dissolved in water, and decomposed by muriatic acid, a mixture of fat acids is separated. By dissolving this in weak alcohol, and allowing it to evaporate, a black fat exactly similar in appearance to humus is deposited. Upon further evaporation a white fat is likewise separated, which may be purified by repeated solutions in alcohol, and digestion with animal charcoal. The black fat is soluble in alcohol and æther; with the latter it forms a solution of a syrupy consistence. This solubility in æther shows that it is not humic acid. The colour of the acid does not appear to be merely accidental; but the examination of these substances was not proceeded with, for being products of decomposition and not at all crystalline, there were no means of ascertaining when they were sufficiently pure for analysis.

St. Andrews, Oct. 28, 1840.

XXI. *Abstract of recent Researches on the Constitution of the fatty Substances, made by* MM. REDTENBACHER, VARRENTRAPP, MAYER *and* BROMEIS†.

FROM the original analyses of Chevreul, the composition of hydrated stearic acid appeared to be

$$\left.\begin{array}{lr}\text{Carbon} \ldots\ldots & 77\cdot42\\ \text{Hydrogen} \ldots & 12\cdot43\\ \text{Oxygen} \ldots\ldots & 10\cdot15\end{array}\right\} 100.$$

---

* Sericate of copper may be obtained by double decomposition; it is of a green colour, insoluble in water, and contains water in chemical combination; the sericates of zinc, lime, cobalt, &c. may be procured in a similar manner, but their examination was quite unnecessary.

† From the *Annalen der Pharmacie*, July, August, and September, 1840.

From which results the formula $C_{70}\,H_{69}\,O_7$, which was adopted by Berzelius and most other chemists; and the acid being bibasic, the formula for the anhydrous acid was $C_{70}\,H_{67}\,O_5$. As, however, in organic analyses the hydrogen is valued a little too high, Liebig proposed lately the formula $C_{70}\,H_{66}\,O_5$. In order to decide upon the constitution of these bodies, M. Redtenbacher commenced a complete investigation of them at Liebig's request.

The result was that the stearic acid was found, by a careful series of analyses executed on it in its hydrated condition, and on a great variety of its salts, to be represented in its anhydrous condition by the formula $C_{68}\,H_{66}\,O_5$, and when hydrated by $C_{68}\,H_{66}\,O_5 + 2\,Aq$; the latter is composed of

$$\left.\begin{array}{ll} \text{Carbon} \ \ldots\ldots & 77{\cdot}04 \\ \text{Hydrogen} \ \ldots\ldots & 12{\cdot}58 \\ \text{Oxygen} \ \ldots\ldots & 10{\cdot}38 \end{array}\right\} 100{\cdot}0$$

with which the numerous experimental results perfectly agreed.

Among other collateral evidence was the analysis of stearic æther, which was found to consist of $C_{72}\,H_{72}\,O_7$, being produced by

$$\left.\begin{array}{ll} \text{1 atom of Stearic acid} & C_{68}\,H_{66}\,O_5 \\ \text{1 atom of Æther} \ \ldots & C_4\,H_5\,O \\ \text{1 atom of Water} \ \ldots & \,H\,O \end{array}\right\} C_{72}\,H_{72}\,O_7.$$

The substance described by Lassaigne as stearic æther, could not be formed.

When stearic acid is distilled, there is generated a large quantity of margaric acid and other products, with which Chevreul did not occupy himself (margarone, &c.), and the first full explanation of the process is due to M. Redtenbacher.

The formula of hydrated margaric acid hitherto generally received was $C_{70}\,H_{66}\,O_8$, which was such that this acid might be looked upon as a compound of

$$\left.\begin{array}{ll} \text{1 atom of Stearic acid} & C_{70}\,H_{68}\,O_7 \\ \text{1 atom of Oleic acid} & C_{70}\,H_{62}\,O_7 \\ \text{2 atoms of Water} \ \ldots & \phantom{C_{70}}\,H_2\,O_2 \end{array}\right\} \frac{C_{140}\,H_{132}\,O_{16}}{2}.$$

Berzelius suggested also that a carbo-hydrogen, $C_{78}\,H_{67}$, may be supposed to be the common radical of the stearic and the margaric acid, which would then differ only in having different degrees of oxidation. The composition of the margaric acid has, however, been determined afresh by Varrentrapp, with the following results.

Margaric acid, whether prepared directly from human fat or by the destructive distillation of tallow, lard, oleic acid or olive oil, is identical in constitution, being when hydrated expressed by $C_{34}\ H_{34}\ O_4$, or when anhydrous, by $C_{34}\ H_{33}\ O_3$.

The margaric æther consists of an atom of margaric acid and one of æther, $C_{34}\ H_{33}\ O_3 + C_4\ H_5\ O$.

Varrentrapp analysed the margarone which is produced when margaric acid is distilled with lime. Its formula is $C_{33}\ H_{33}\ O$, or it is formed by the loss of the elements of one atom of carbonic acid.

From these results, however, it follows equally, as from the older, that the carbon and hydrogen is the same in the stearic and the margaric acids, which differ only in the proportion of oxygen,

1 equivalent of Stearic acid $= C_{68}\ H_{66}\ O_5$
2 equivalents of Margaric acid $= C_{68}\ H_{66}\ O_6$

and hence Berzelius's idea is still quite true, that if we express $C_{34}\ H_{33}$ by R, then

Margaric acid $= R + O_3$, like sulphuric acid $S + O_3$
Stearic acid $= R_2 + O_5$, like hyposulphuric acid $S_2 + O_5$.

But it still remains to be shown how destructive distillation, which is in general not an oxidating process, changes both the stearic and oleic acids into margaric acid. This has been explained by Redtenbacher. Besides margaric acid there are produced a small quantity of water and of carbonic acid, and margarone and a liquid polymeric with olefiant gas in large proportion, so that the process consists in four equivalents of stearic acid producing

6 equiv. of hydrated margaric acid $= C_{204}\ H_{204}\ O_{24}$
1 —— water . . . . $= H\ O$
1 —— margarone . . . . . . $= C_{33}\ H_{33}\ O$
1 —— carbonic acid . . . . $C\ O_2$
Polymeric carburetted hydrogen $C_{34}\ H_{34}$

$$C_{68}\ H_{68}\ O_7 \times 4 = C_{272}\ H_{272}\ O_{28}$$

The action of nitric acid on stearic acid has been accurately studied by Bromeis, and has produced extremely interesting results. In the first instance an equivalent of anhydrous stearic acid is converted into two equivalents of anhydrous margaric acid, by the absorption of an equivalent of oxygen, $C_{68}\ H_{66}\ O_5$, and O producing twice $C_{34}\ H_{33}\ O_3$.

When the action of the nitric acid is continued, suberic acid is produced in large quantity, the formula of it being $C_8\ H_6\ O_3 + Aq$.

The mother liquors from which the suberic acid had separated contains a large quantity of succinic acid, the formula of which is, in the crystallized form, $C_4\ H_3\ O_4$.

The oleic acid, when treated with nitric acid, produces also margaric and suberic acid. The mother liquor from which these have separated contain the pimelic and adipic acids discovered by Laurent. For the first, M. Bromeis verified Laurent's formula, $C_7\ H_5\ O_3 + Aq$, but for the second he obtained a different result, viz. $C_{14}\ H_9\ O_7 + 2\ Aq$.

Laurent had supposed that œnanthic acid was produced in this process, but Bromeis could not obtain any satisfactory evidence of its formation, nor has he as yet been able to verify the history of the lipic and azoleic acids as given by that chemist.

The drying oleic acid from linseed oil gave with nitric acid neither pimelic acid nor adipic acid, but a large quantity of oxalic acid, of which the proper oleic acid yields none.

It has been long known that when olive oil is mixed with a solution of proto-nitrate of mercury it becomes solid, and this property, which was once made use of to determine the purity of olive oil, has been found to belong to almost all fatty bodies in a greater or less degree. Boudet proved that the action of the mercurial solution depended exclusively on the hyponitrous acid in the liquor, and that the same effect was produced by hyponitrous or nitrous acid fumes alone. He also pointed out that the solid mass produced was a compound of glycerine with a peculiar acid, which he termed the *elaidic*, as he gave the name of *elaidine* to this artificial fat.

The only numerical results obtained of the constitution of this acid were given by Laurent. From his analyses the formula for the crystallized elaidic acid should be $C_{35}\ H_{33}\ O_3 + Aq$, and for the elaidic æther the formula $C_{35}\ H_{33}\ O_3 + C_4\ H_5\ O$.

From this constitution, however, no insight into its origin could be obtained, and Mayer undertook in Liebig's laboratory the complete revision of its history.

M. Mayer found the modes of preparation prescribed and followed by Boudet and by Laurent quite insufficient to obtain a product of absolute purity. He arrived ultimately at the result that it is only from the oleic acid that the elaidic acid is produced, and hence to form it he had recourse to absolutely pure oleic acid prepared by the method of Varrentrapp, which will be described by and by. It may also be prepared nearly, but not so absolutely pure, by passing a stream of nitrous acid vapour through oil of sweet almonds. Nitric oxide gas passes off, and the oil, when exposed to cold, gives a copious crop of crystals of elaidic acid.

Elaidic acid is of a brilliant white colour, crystallizes in large tables, and melts at 112° Fahrenheit. It is easily soluble in alcohol, from which it separates in crystals like benzoic acid. It is also soluble in æther. These solutions react acid.

When elaidic acid is treated with a great excess of caustic potash it is completely decomposed, and a new acid formed identical with that which Varrentrapp had found oleic acid to give under the same circumstances: with carbonated alkalies it produces, however, soaps from which it separates on the addition of a stronger acid unchanged.

The elaidates of silver, lead, and barytes are voluminous white precipitates; that of silver may be obtained crystallized from an ammoniacal solution.?

Numerous analyses of crystallized elaidic acid gave for its composition as follows:

72	atoms	of Carbon...	5503·3	78·04
68	——	Hydrogen	848·6	12·03
7	——	Oxygen...	700·0	9·93
			7051·9	100·00

The elaidate of silver consisted of

72	atoms of	Carbon.........	5503·3	56·56
66	——	Hydrogen......	823·6	8·46
5	——	Oxygen	500·0	5·14
2	——	Oxide of silver	2903·2	29·84
			9730·1	100·00

Its formula is therefore $C_{72} H_{66} O_5 + 2 Ag O$; and the crystallized acid is expressed by $C_{72} H_{66} O_5 + 2 Aq$, it being a bibasic acid.

The composition of the elaidic æther agrees also with these results; it consists of

76	atoms of	Carbon...... =	5809·1	78·42
72	——	Hydrogen...	898·5	12·13
7	——	Oxygen......	700·0	9·45
			7407·6	100·00

Its formula is therefore $C_{72} H_{66} O_5 + C_4 H_5 O + Aq$.

These results differ totally from those given by Laurent, who evidently worked upon an impure substance.

Sebacic acid.—This acid is produced whenever any fatty substances, as the fat oils, lard, &c. are distilled. Its composition has been ascertained by Redtenbacher, who confirmed fully the result long since given by Dumas, that the formula of the crystallized acid is $C_{10} H_8 O_3 + Aq$.

Redtenbacher has, however, established the interesting fact, that in this distillation it is only the oleic acid which produces, by its decomposition, the sebacic acid. Neither glycerine, nor the stearin, nor the margaric acids yield any trace of sebacic acid when distilled. Its origin must therefore depend upon the constitution of the oleic acid.

Oleic acid.—This acid, one of the most important of the series of fatty bodies, is still but imperfectly known. Since its original discovery by Chevreul it has been examined only by Laurent; but he distilled the acid he employed, and as in this process it is almost totally decomposed, the results at which he arrived were very incorrect.

The chemical history of oleic acid has been re-examined by Varrentrapp. He has found the purest source of it to be oil of sweet almonds, which contains no stearic acid, and but a trace of margaric acid. The almond oil is to be saponified by means of a solution of caustic potash, and this decomposed by dilute sulphuric acid. The impure oleic acid thus obtained is to be digested at 212° for many hours with oxide of lead, and then the mass produced macerated in the cold with æther. The oleate of lead dissolves readily in æther, but the margarate is insoluble therein, and by this means the former is obtained completely pure. The filtered liquor is to be then mixed with its own volume of water, and as much dilute muriatic acid to be added as converts the oleate into chloride of lead. This deposits itself completely in the water below, and the oleic acid remains dissolved in the æther. When this is distilled off the oleic acid remains with the properties described by Chevreul.

In the same manner the oleic acid of tallow or lard may be obtained pure.

A series of ten analyses, which agreed very well with one another, gave for the oleic acid the following composition:

44 atoms of	Carbon	3363·1	77·10
40 ——	Hydrogen ...	499·2	11·44
5 ——	Oxygen	500·0	11·46
		4362·3	100·00

The composition of oleate of barytes was found to be

44 atoms of	Carbon	=	3363·1	64·59
39 ——	Hydrogen	=	486·7	9·35
4 ——	Oxygen	=	400·0	7·68
1 ——	Barytes	=	956·9	18·38
			5206·7	100·00

Its formula is therefore $C_{44}\,H_{39}\,O_4 + Ba\,O$, and that of the acid analysed is $C_{44}\,H_{39}\,O_4 + Aq$.

The oleic æther was $C_{44}\,H_{39}\,O_4 + C_4\,H_5\,O$.

When oleic acid is distilled, there is produced a small quantity of carbonic acid, sebacic acid, a large proportion of a liquid having the composition of olefiant gas, and some charcoal remains: thus

$$\left.\begin{array}{lll} \text{1 atom of} & \text{Sebacic acid} & C_{10}\,H_9\,O_4 \\ \text{3 atoms of} & \text{Carbonic acid} & C_3\,O_6 \\ \text{4 ——} & \text{Carbon ......} & C_4 \\ \text{Carburet of} & \text{Hydrogen ...} & C_{71}\,H_{71} \end{array}\right\}\text{are formed from}$$

$$\text{2 atoms Oleic acid} = C_{88}\,H_{80}\,O_{10}.$$

As no other fatty acid produces sebacic acid when distilled, its formation is a test of the presence of oleic acid. Hence we may know if wax or spermaceti, or stearic acid, have been adulterated with tallow by submitting a small quantity to distillation.

When the oleic acid is warmed with an excess of caustic potash and a few drops of water, until it be completely saponified, and then more potash being added, the temperature be raised to the melting point of the latter, the mass, which should be constantly stirred, does not become brown, but disengages a quantity of hydrogen gas. When the mass so produced is dissolved in water and decomposed by an acid, a white solid separates in large crystalline grains like stearine. Elaidic acid treated in the same way yields an identical product.

When crystallized from its alcoholic solution this new acid fuses at 143° Fahrenheit; when analysed it gave the following result:

32 atoms of	Carbon...	2445·9	75·69
31 ——	Hydrogen	386·0	11·97
4 ——	Oxygen...	400·0	12·34
		3231·9	100·00

The silver salt gave as follows:

32 atoms of	Carbon	2445·9	53·54
30 ——	Hydrogen...	374·4	8·20
3 ——	Oxygen......	300·0	6·50
1 ——	Oxide of silver	1451·6	31·76
		4571·9	100·00

The formulæ are, therefore, for

the silver salt .... $C_{32}\,H_{30}\,O_3 + Ag\,O$
the crystallized acid $C_{32}\,H_{30}\,O_3 + Aq$.

The liquor from which this new acid is precipitated contains abundance of acetic acid formed at the same time. The reaction may be easily explained as follows:

$$\begin{array}{ll} \text{1 atom of oleic acid} & \left.\begin{array}{l} C_{44}\,H_{39}\,O_{4} \\ \quad H_{8}\;\,O_{8} \end{array}\right\} C_{44}\,H_{47}\,O_{12} \\ \text{and 8 atoms of water . . .} & \end{array}$$

may produce

$$\begin{array}{ll} \text{1 atom of the new acid} & \left.\begin{array}{l} C_{32}\,H_{30}\,O_{3} \\ C_{12}\,H_{9}\;\,O_{9} \\ \quad H_{8} \end{array}\right\} C_{44}\,H_{47}\,O_{12}; \\ \text{3 atoms of acetic acid} & \\ \text{and 8 atoms of hydrogen} & \end{array}$$

or from elaidic acid

$$\begin{array}{ll} \text{1 atom of elaidic acid} & C_{72}\,H_{66}\,O_{5} \\ \text{7 atoms of water . . .} & \quad H_{7}\;\,O_{7} \\ \hline & C_{72}\,H_{73}\,O_{12} \end{array}$$

may produce

$$\begin{array}{ll} \text{2 atoms of the new acid} & C_{64}\,H_{60}\,O_{6} \\ \text{2 atoms of acetic acid} & C_{8}\;\,H_{6}\;\,O_{6} \\ \text{and 7 atoms of hydrogen . .} & \quad H_{7} \\ \hline & C_{72}\,H_{73}\,O_{12} \end{array}$$

Some traces of carbonic and oxalic acids which appear are not here introduced.

The production of elaidic acid from oleic acid cannot as yet be explained. There are also other products, particularly one, a red or orange-coloured liquid, the formation of which is the cause of the citrine tinge in ordinary elaidine. This substance could not be got in a state fit for analysis.

XXII. *Mineralogical Notices from Foreign Journals*[*].

ANTIGORITE. BY MM. WISER AND SCHWEIZER.

[From Poggendorff's *Annalen*, vol. xlix. Part 4.]

THE characters assigned by M. Wiser to this mineral are as follows:—not crystalline; hardness = 2·5 (scratches gypsum, is scratched by calcspar); specific gravity 2·622, slightly lustrous; in thin plates semi-transparent; in very thin leaves transparent; colour by reflected light blackish green; by transmitted light leek-green; it does not affect the magnetic needle. Two analyses by M. Schweizer gave

Silica......................	46·22	46·18
Protoxide of iron	13·05	12·68
Alumina	2·08	1·89
Magnesia	34·39	35·19
Water	3·70	—
	99·44	99·64

---

[*] Communicated by Mr. W. Francis.

Its constitution may be expressed by the formula

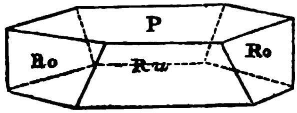
$$\left.\begin{array}{l}\dot{M}g^3\\ Fe^3\end{array}\right\}\dddot{Si}^2+\dot{M}g\,\underline{\dot{H}}.$$

which at the same time indicates its near relation to serpentine.

PENNINE. BY PROF. J. FRÖBEL AND M. SCHWEIZER.

[From Poggendorff's *Annalen*, vol. l. Part 3.]

Under this name (derived from the Pennine Alps, in which district, near Zermatt, it occurs) Prof. J. Fröbel has described a mineral bearing great resemblance to the chlorite from Achmatowsk and the Zillerthal. Hexagonal, with very distinct basal structure. The well-developed crystals generally present at times the tabular, at times the columnar combination of the faces of a rhombohedron with the horizontal pair of faces, as shown in the annexed wood-cut.



The values P on Ro = 99°, of Ro on Ru = 118°. Hardness on the basal faces somewhat above that of gypsum, on the rhombohedral faces not quite equal to that of calcspar. Thin leaves flexible, but not elastic. By reflected light, black-green; by transmitted light, in the direction of the principal axis between emerald-green and leek-green, at right angles to the principal axis brown to hyacinth-red. Lustre vitreous; when not too thick, perfectly transparent. The results of two analyses by M. Schweizer gave

Silica	33·32	33·07
Protoxide of iron	11·30	11·36
Alumina	9·32	9·69
Magnesia	33·04	32·34
Water	11·50	12·58
	98·98	99·04

leading to the formula

$$\left.\begin{array}{l}\dot{M}g^3\\ \dot{F}e^3\end{array}\right\}\dddot{Si}^2+\underline{\dddot{Al}}\,\dddot{Si}^3+7\,\dot{M}g\,\underline{\dot{H}},$$

which is constructed according to the same principle employed by M. Varrentrapp for chlorite.

CHLOROSPINELLE AND XANTHOPHYLLITE. BY PROF. G. ROSE.

[From Poggendorff's *Annalen*, vol. l. Part 4.]

Chlorospinelle occurs crystallized in octahedrons generally, but of from one to two lines in size; colour grass-green; transparent at the margins; lustre vitreous; of the hardness of topaz; specific gravity 3·594. According to two analyses performed by Prof. H. Rose, it consists of

Magnesia	26·77	27·49
Lime	0·27	—
Peroxide of copper ...	0·27	0·62
Alumina	64·13	57·34
Peroxide of iron	8·70	14·77
	100·14	100·22

resembling, therefore, spinelle and zeylanite, and belonging with these, since it likewise coincides in its crystalline form, to the same genus. Locality, Slatoust.

Xanthophyllite, so named from its wax-yellow colour and laminar structure, constitutes a new genus. In thin leaves transparent; on the surface of fracture it has a strongly nacreo-vitreous lustre. Hardness that of felspar; specific gravity =3·044. Before the blow-pipe it proved to contain alumina, lime, soda, peroxide of iron and silica, but no fluoric acid, magnesia or potash. It occurs near Slatoust.

PIKROPHYLL. BY A. F. SVANBERG.

[From Poggendorff's *Annalen*, vol. l. Part 4.]

This new mineral occurs near Sala. Structure foliated; hardness between that of mica and calcspar; specific gravity 2·73; colour, very dark green. Analysis gave

		Oxygen.	
Silica........	49·80	25·88	26·40
Alumina	1·11	0·52	
Lime	0·78	0·22	13·43
Magnesia	30·10	11·65	
Protoxide of iron	6·86	1·56	
Protoxide of manganese a trace			
Water	9·83		
	98·48	8·73	

whence the mineralogical formula $3\,MS^2 + 2\,Aq$.

XXIII. *On Artificial Oil of Ants.* *By* J. Stenhouse, *Esq.**

IT was first observed by Dœbereiner, when preparing formic acid by the action of sulphuric acid and oxide of manganese on sugar, that the liquor which distilled over contained a small quantity of oily matter which rendered it milky. To this he gave the somewhat fanciful name of "artificial oil of ants." From the extremely small quantity in which it is produced by this process, he did not succeed in collecting it, or in determining its properties.

---

* Communicated by the Author.

In repeating Professor Emmet's process for formic acid, which consists in distilling grain of various kinds with sulphuric acid, without any oxide of manganese, I observed that under certain circumstances the liquid in the receiver contained considerable quantities of this oil. After repeated trials I succeeded in pretty easily preparing it. It is a constant product, though in very different quantities, of the action of sulphuric acid on vegetable substances, and it may be conveniently procured from grain of any kind, saw-dust, husks of corn, &c.

The process which I found to succeed best is the following. Take equal weights of oatmeal or saw-dust and sulphuric acid, diluted with its own bulk of water, and introduce the mixture into a large copper still. As there is no oxide of manganese present there is no frothing up of the liquid, so that the still may be safely filled rather more than half-full; when the mixture has boiled some time and the meal is quite charred, pour back the liquid which has distilled, adding the same quantity of water as at first, and push the distillation nearly to dryness. The liquid which passes over into the receiver is slightly milky, owing to the presence of the oil, and at the same time strongly acid from the sulphurous and formic acid which it contains. It is to be neutralized with slaked lime, when it becomes of a deep yellow colour, and is again to be slowly distilled till about a fourth or fifth part of it has passed over. This is to be mixed with a considerable quantity of fused chloride of calcium, and subjected to a second distillation. The chloride of calcium retains a great part of the water, in which the oil is pretty soluble: if too much water still passes over after a second rectification, a new quantity of chloride of calcium must be added, and the distillation repeated a third time. The oil comes over mixed with a little water, and sinks to the bottom of the receiver; about twelve pounds of meal yielded nearly one ounce of oil, so that it is by no means an abundant product.

Properties.—The anhydrous oil is transparent when newly prepared, nearly colourless, having only a greenish-yellow tinge, but when kept some time it becomes of a brownish-red, from the formation of a resinous matter. Its taste and smell are very pungent and aromatic, resembling oil of Cassia. It stains paper, but not permanently. It catches fire very easily, and burns with a strong yellow flame, emitting much smoke. It boils at 168° C. Its specific gravity is 1·1006 at 27° C. It is pretty soluble in water, and much more so in alcohol and æther. Potassium effervesces strongly when placed in it, but neither the alcoholic nor aqueous solution of potash has any effect upon it. Solid potash, with the assistance of heat, converts it into a brown resin. It dissolves iodine very easily, but without any violent action even when heated. In the cold muriatic acid gives it a fine red colour, and with the assistance of heat turns it into a dark brown resin. Nitric acid first reddens, then chars it. Sulphuric acid has a similar effect.

The results of its analyses are as follows:

I. 0·330 substance gave 0·747 carbonic acid, and 0·130 water.

II. 0·324 gave 0·725 carbonic acid, and 0·1275 water.

III. 0·3185 gave 0·7205 carbonic acid, and 0·128 water.

	Found.			Calculated.
	I.	II.	III.	
Carbon ...	62·59	61·87	62·55	62·94
Hydrogen	4·37	4·37	4·46	4·11
Oxygen...	33·04	33·76	32·990	32·94
	100·00	100·00	100·00	99·99

This gives the formula:

Carbon 5 atoms ...... =382·175
Hydrogen 4 atoms ...... = 24·959
Oxygen 2 atoms ...... =200·000

Atomic weight 607·134

It is evident that this oil has a very different composition from oils in general, as it contains oxygen and hydrogen only in the proportions to form water, while all other known oils contain a great excess of hydrogen.

XXIV. *Facts and Observations relating to the Science of Phonetics.* *By* R. G. Latham, *Fellow of King's College, Cambridge.**

Part I.—*Upon the Aspirates* (*real and accredited*) *of* K *and* G.

THE object of the present paper is to show that the real aspirates of K and G are not the sounds that are generally considered as such; in other words, that they are not the powers of the German *ch* and Scotch *gh*, but that they are sounds perfectly distinct from either the one or the other of those articulations.

---

* Communicated by the Author.

The question in point, besides its value in etymology, has a value in acoustics; at least in that particular part of them, that deals with the affinities and analogies of articulate sounds.

This part of acoustics may conveniently be called the province of phonetics.

How far the articulate sounds are systematically related to each other; how far and in what cases they run into each other; how far the chain of relationship is lineal, and how far it is circular; how far vowels and consonants, mutes and liquids differ in kind from each other; these questions, and questions similar to these, constitute the province of phonetics.

These investigations must be distinguished, on the one hand, from those of physiology, and on the other hand, from those of etymology.

The science of phonetics determines how two given articulations are related; the science of physiology inquires how they are produced.

Phonetics tell us that between two articulations an immutable and essential relation exists; whilst etymology observes that under certain circumstances one articulation is changed into another. Very often etymology does more; it assumes the alliance from the change.

Now it does not follow, in etymology, that because in a given language one sound changes to another, those two sounds are, therefore, naturally allied to each other; although such is *often* (far, however, from always) the case.

Nor yet has it been proved in physiology that the sounds which to the ear sound alike, are produced by a like disposition of the parts of the mouth or larynx; although that such a correspondence exists is highly probable.

The truth is, that the special study of phonetics, instead of being promoted by the grammarian and anatomist, has been retarded by them. Etymological and physiological tests, etymological and physiological classifications, have been applied where acoustic principles alone ought to have been recognized. A necessary correspondence, moreover, between the three kinds of sciences, which in grammar may be proved non-existent, and in physiology has not been proved at all, has for the most part been gratuitously assumed; and more than this, in those cases where the three tests have not coincided, the disposition has been to sacrifice the phonetic test to the other two.

The Phonetic test stands thus—Two sounds are allied, because, to the ear, they sound alike.

The physiological test thus—Two sounds are allied, because, in the mouth and larynx, they are formed alike.

The etymological test thus—Two sounds are allied, because, under certain circumstances, one is changed to the other.

Expressed in Latin axioms, the etymological test is, *Propter fortunas articulatio est id quod est*; the physiological one, *Propter formationem articulatio est id quod est*; and, finally, the phonetic one is, *Propter sonum articulatio est id quod est*. The coincidence of the three is (in the present state of our knowledge) to be considered as an accident.

Phonetic science considers sounds irrespective of the signs by which they are expressed, and irrespective of the names by which they are called. The sound of the *b* (in *bat*) is the same, whether it be expressed by the sign *b* or *β*; it is also the same whether it be called *bee* or *beta*. A sound is not double because it is spelt with two letters, nor yet single because it is expressed by one. The *th* in *thin* is a simple sound, irregularly expressed: the *x* in *box* is a double one compendiously spelt. In questions of the kind in point, the eye has so often misled the ear that the above given truisms are scarcely to be considered superfluous. In points of acoustics the blindest guides are the best.

To guard against the influence of names and signs it is convenient to express the relations of certain sounds, arithmetically; and besides this, in the case of such simple single sounds as have no simple single sign (or letter) to denote them, to coin or borrow appropriate signs as the occasion requires.

The subjoined scales of relationship being those that are generally recognized, it is unnecessary to go beyond the mere exhibition of them.

The sound of *b* (in *bin*) is in a certain relation to the sound of *p* (in *pin*). Now,

v : f :: b : p . and
d : t :: v : f . and
ð[1] : þ[2] :: d : t . and
g : k :: ð : þ . and
z : s :: g : k . and
ζ[3] : σ[4] :: z : s .

[1] The *th* in *thine*. [2] The *th* in *thin*. [3] The *z* in *azure*. [4] *sh* in *shin*.

Again, the sound of *f* (as in *fa*) is in a certain relation to the sound of *p* (as in *pa*); or, expressed more loosely, *f* is in a certain relation to *p*. Now

v : b :: f : p . and
þ : t :: v : b . and
ð : d :: þ : t . and
σ : s :: ð : d . and
ζ : z :: σ : s .

To these two classes of relation the names Hard and Soft, Lene and Aspirate may (for want of better) be allowed to stand; so that the double relationship is expressed in the following table:—

Lene.		Aspirate.	
Hard.	Soft.	Hard.	Soft.
p	*b*	*f*	*v*
t	*d*	þ	ð
k	*g*	?	?
s	*z*	σ	ζ

or

Hard.		Soft.	
Lene.	Aspirate.	Lene.	Aspirate.
p	*f*	*b*	*v*
t	þ	*d*	ð
k	?	*g*	?
s	σ	*z*	ζ

Here we have (so to say) four parallel lines of double relationship. The sequence, however, although perfect as far as it goes, is incomplete. We miss the sounds that are to *k* and *g*, as *f* is to þ and *v* to *b*, &c.&c. Were it not for this deficiency we should have a mute-system of sixteen allied articulations, square and symmetrical, each articulation being to the other doubly related, *i. e.* hard or soft, lene or aspirate. As it has been said before, it is the object of the present paper to fill up the deficiency in question, and, subordinately to the main end, to show that the sounds that are considered to be the aspirates of *k* and *g* are not so in reality. The sounds that in all the works (philological or physiological) with which I am acquainted, have been supposed to hold the relationship in point, are the German *ch* (as in *auch*) and the Gaelic *gh* (as in *lough*) respectively. Let these two sounds be expressed by *kh* and *gh* respectively, and let the undetermined aspirates of *k* and *g* be expressed by K_x and G_x respectively.

The affinities which invalidate the current opinion, are, first, the affinity which exists, and which is recognized, between the mute sounds of *b*, and *v* and the semivowel sound of *w*; and secondly, the affinity (existing and recognized) between the semivowel sounds of *y* (as in *ya*) and the mute sound of *g* (as in *ga*).

The sound of *w* (as in *wa*) is to the sound of *v* (as in *va*) in a certain degree of relation; and, through this, it is allied to *b* (as in *ba*), in a manner determinable by the relation of *v* and *b*: and, again, *y* (as in *ya*) is to *g* (as in *ga*) as *w* is to *v*. Hence it arises that the sound which shall be to *g* as *v* is to *b*, shall be to *g* and *y* as *v* is to *b* and *w*. Expressed formally, the fact stands thus:

G_x : *g* and *y* : : *v* : *b* and *w*.

In a short tract, published in 1835, I stated that the sound in point must be on the "*y*-side of *g*," and that, although a ludicrous mode of speaking, expressed my meaning fully.

The conditions then *of* G_x are, that it must be in a certain relation to *g* and *y*; in which relation I am satisfied that *gh* is not.

It may not be thought superfluous to remark, that the two last assertions, that is, the *assumptions* of the question in hand, are made irrespective of any previous reasoning. I consider that they rest upon the evidence of our sense of hearing. Expressed formally, the facts and inference are as follows:

G_x : *g* and *y* : : *v* : *b* and *w*. which *gh* is not.

* * * * * *

What *gh* is, is another question. Unless the number of mutes is to be reduced to four, sounds like *b*, *p*, *f*, *v*, &c. &c. must be considered not as *varieties* of a given typical sound, but as sounds *specifically distinct*: and, again, unless the number of mutes is to be indefinitely increased, sounds like the *t* and *d cerebral* of the Hindoos (I speak in the way of illustration) must be considered, not as sounds specifically distinct, but as *varieties* of a given typical sound.

To come, then, under the necessary conditions, the sound of G_x must be specifically different from that of *G*. Now *Gh* (as may be shown at another time) is no *specific* sound, but only a *variety*.

The force of the words *specific* and *variational* depends upon their definitions. To say that þ is a variety of *t* is true or false, according to the definition of the term *variety*; and the assertion is objectionable only on the score of inconvenience. To say, however, that þ is a variety of *s* (as is often said by those who are strangers to the sound) involves something more than an inconvenient definition. It is an error in respect to the fact.

Having (to my own satisfaction at least) inferred that *Gh* was not G_x, I was left to consider what really was G_x, and whether such a sound either actually existed or was capable of existing.

I drew the distinction between the actual and the possible existence of a sound for the following reason. It is very evident that although an articulation may be capable of being both formed by the larynx and distinguished by the ear, it may not exist in any spoken language; in other words, it does not follow that because a sound exists *in posse* it exists *in esse* also. If the French were the universal language, the sound of the English *th* would exist only in the capabilities of the ear and larynx; it would be a sound *in posse*, or a latent sound.

Now against G_x being a possible sound, I knew of no reason except the *accident* of its not being found in any of the well-known languages; whilst in favour of its being one were the analogies of the other mutes. This latter fact having convinced me that the sound in point was within the compass of the vocal organs, I conceived it possible that (two facts being ascertained) it might be formed *à priori*, that is, independent of imitation. These two facts were, first, the determination of the difference between an aspirate and its lene in respect to their sounds; secondly, the determination of the same in respect to the disposition of the parts of the mouth and larynx. In the midst of my investigations an accident intervened, that solved all the difficulties. I found, in Rask's Laplandic Grammar, the description of a rare and peculiar sound. It was not *Gh*; nor yet was it either *G* or *Y*. *It was something between the two.* This threw a light upon the question; so that all that now remained for me to do, was to hear the sound. In the town where I was residing there was a Laplander from Norwegian Finmark, whom the Norse government were instructing in reading and writing. Having sent for this person, and having set before him the words that Rask had given as examples, I found that his pronunciation of the sound in point coincided with the description of Rask, with my own previous notions as to its nature, and, consequently, with the conditions of G_x; in other words, the Lappish ʒ (so expressed in Rask's Grammar) was G_x.

Now, in the foregoing notices, nothing has been said concerning *K* and K_x. Nothing has been said concerning them, because it is so evident that they follow the analogies of G and G_x, that any mention of them would have been superfluous. The existence *in posse* of K_x I consider to be all but demonstrated; of its existence *in esse* I am less certain. It is probable that in some of the Greek dialects *k* is pronounced as K_x.

In certain dialects of the same language I imagine that $\gamma = G_x$. The letter *g* in the mouth of a native of Berlin, is pronounced (as I am told) intermediately to *G* and *Y*.* If this be true, it is in all probability G_x. It is probable that in certain dialects, or in certain stages of the Anglo-Saxon, ȝ has been sounded as G_x.

* * * *

K_x then being represented by χ, and G_x by γ, we have, as an exhibition of the system formed by the sixteen mutes, the following table of double relations.

Lene.		Aspirate.	
Hard.	Soft.	Hard.	Soft.
p .	*b* .	*f* .	*v* .
t .	*d* .	ϑ .	ð .
k .	*g* .	χ .	γ .
s .	*z* .	σ .	ζ .

I consider that these relations are immutable and essential, independent of the physiological arrangement, in their formation, of the parts of the mouth and larynx, and independent of their interchanges in the grammars of particular languages. I consider, moreover, that the number of specifically distinct mutes, independent of the fact of their being found in languages, is neither more nor less than sixteen, and that, in the present state of our knowledge, there is no more convenient mode of distinguishing mutes from other articulations, than by saying that every mute is one of four sounds, each of which forms a part of a system of four, and each of which is to some other hard or soft, and lene or aspirate.

XXV. *Reply to Mr.* Airy's *Remarks on Professor* Challis's *Investigation of the Motion of a small Sphere vibrating in a resisting Medium. By the Rev.* J. CHALLIS, *Plumian Professor of Astronomy in the University of Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ALLOW me, through the medium of your Journal, to express my thanks to Mr. Airy for calling my attention, by his letter in your Supplementary Number, to a step in my solution of the problem of the resistance to a sphere vibrating in an elastic medium, which I had left unexplained. I have certainly considered it possible that the velocity of the fluid at a given distance from the centre to or from which it is directed may, at a given instant, be different in different directions from the centre, provided there be no *abrupt* variation.

[* The letter *g*, at least when final, is precisely so pronounced by the Swedes.—EDIT.]

For in the case of motion under consideration, if $v_{\prime}$ be the velocity impressed on the fluid by the vibrating sphere at the extremity of the radius drawn in the direction of its motion, the velocity impressed at the same instant at any point of the surface, the radius to which makes an angle θ with that direction, is *in fact* $v_{\prime} \cos\theta$, the sphere being supposed perfectly smooth; also the ratio of the two velocities, which is $\cos\theta$, does not vary with the time. It is, however, undoubtedly true, as Mr. Airy contends, that if the motion be of this kind, the lateral pressures, namely, those which take place at the surface of the sphere in directions perpendicular to its radii, should be taken into account; and the explanation which Mr. Airy desires will, I suppose, be given, if it be shown, as I am about to do, that these pressures *are* taken into account in my solution of the problem under discussion.

It will be admitted that if the same accelerative force be impressed at every instant on the sphere and on all points of the fluid surrounding it, in the same direction, no motion of the fluid along the surface of the sphere and no alteration of density will be thereby produced. Conceive to be impressed on the sphere at each instant its own accelerative force in a direction contrary to that in which it takes place, and the same accelerative force to be impressed on all points of the fluid in the same direction. The sphere will thus be reduced to rest, and the case of motion will become that of a variable stream impinging on a stationary sphere. The effective accelerative force at any point of the surface of the sphere will be the resultant of the above-mentioned impressed force, and of the force impressed at that point by the sphere in motion. This resultant is in the direction of a tangent to the surface, and if $\frac{dv}{dt}$ be the accelerative force of the sphere at the time t, is equal to $\frac{dv}{dt}\sin\theta$, the angle θ being that which has already been defined. Now by a known theorem of hydro-dynamics, if p be the pressure, ϱ the density, and f the effective accelerative force at any point s of a line drawn always in the direction of the motion of the particles through which it passes,

$$\frac{dp}{\varrho\, ds} + f = 0.$$

In the instance before us the line s is along the surface of the sphere, $ds = r\,d\theta$ (r being the radius of the sphere), and if $v = m \sin bt$, $f = mb \sin\theta \cos bt$. Hence by substituting and integrating,

$$p = p_{\prime} + mbr \cos\theta \cos bt.$$

The pressure $p_{\prime}$ corresponding to $\theta = \frac{\pi}{2}$ is that part which is common to every point of the surface of the sphere. Hence the pressure which tends to put the sphere in motion, or $p - p_{\prime}$, is

$$b\,m\,r \cos\theta \cos b\,t.$$

This, by the remark made at the commencement of the reasoning, is the same pressure as when the fluid is stationary and the sphere in motion, and is identical, if very small quantities be neglected, with the value obtained by a very different process at the bottom of p. 465 of my communication to the December Number. The consideration of the lateral pressure does not therefore lead to any variation from the former result.

The force of this explanation will, perhaps, be more clearly seen by the following remark. Supposing the sphere at rest and the fluid in motion, the velocity at the position corresponding to $\theta = 0$ is nothing. But as the lines drawn at any instant in the direction of the motion of the particles through which they pass nowhere intersect the surface of the sphere, it follows that the particles about that position are successively carried along the surface till at a position corresponding to $\theta = \frac{\pi}{2}$ they acquire the velocity of the *stream*. The lateral pressure is concerned in effecting this transfer of the individual particles, while the law of its variation at any instant along the surface of the sphere is that which is given by the preceding investigation. When the fluid is at rest and the sphere in motion the velocity impressed on any particle at the position $\theta = 0$ is reduced to nothing when it arrives at the position $\theta = \frac{\pi}{2}$ by the same pressure.

I am, Gentlemen, yours, &c.

Cambridge Observatory, Jan. 18, 1841. J. CHALLIS.

XXVI. *Correction in a paper published in the Philosophical Magazine for January. By* G. B. AIRY, *Esq., M.A., F.R.S., Astronomer Royal.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I AM indebted to the kindness of Mr. Tovey for pointing out an omission in my investigation published in your Number for January; arising, as will frequently happen, from the difficulty of attending to every small step when hastily writing out an investigation with whose results we are familiar.

I request you to insert in your February Number the following correction:

Philosophical Magazine, January 1841, page 9, line 6, *read* as follows:

"Here $a = 0$, and the approximate investigation for $r_{,}$ at the bottom of page 3, does not apply. Remarking, however, that $r_{,}$ is now $= 0$, the expression for $\frac{2\pi}{\lambda}\sqrt{h^2 + r_{,}^2}$ is reduced to $\frac{2\pi}{\lambda}h + 0$: the term corresponding to $\cos(e_{,} \cos\theta)$ is $\cos 0$ or 1: and the term corresponding to $\frac{1}{2\pi}\int_{\theta}(\cos e_{,} \cos\theta)$ (from 0 to 2π) is $\frac{1}{2\pi} \times 2\pi = 1$. The expression for the intensity therefore becomes $1 + E^2_{,r}$"

No alteration is required after line 7, and the results of the investigation are in no way affected.

I am, Gentlemen, yours, &c.

Royal Observatory, Greenwich,
Jan. 20, 1841.

G. B. Airy.

XXVII. *On the Electricity of expanding Air, as connected with the Electrical Phænomena of Effluent Steam.* *By* Wm. Geo. Armstrong, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In connexion with the experiments which I have recently published on the electricity of effluent steam, it occurred to me that it would be well to inquire whether similar effects to those I have described, could be produced by compressing common air in a receiver, and then suffering it to escape in a jet, in the same manner as steam had been discharged from a boiler in the experiments alluded to. With this view, therefore, I condensed about eight atmospheres into a strong vessel, the capacity of which was nearly six quarts; I then insulated the vessel, and discharged the air through a glass tube which I had previously inserted for the purpose.

On the first trial I obtained no indications of electricity whatever, but upon repeating the experiment on a subsequent day, the insulated vessel became so highly electrified, when the air was discharged, as to yield sparks fully a quarter of an inch long. I afterwards tried the same experiment a great number of times, and, strange to say, the electricity of the vessel, though generally *negative*, occasionally proved to be *positive*. Sometimes the electricity was very strong, and sometimes very weak, and frequently I could get no electricity at all. By means of an insulated conductor terminating in a number of points, I also obtained electricity from the ejected air, and found it to be *positive* every time I tried it.

I more frequently succeeded in producing an electrical development when the receiver was cold, and contained a little moisture, than when it was warm and dry, so that it is not improbable that evaporation may even here be the source of the electricity. I am by no means sure, however, that my better success when the receiver was cold and damp, was not mere chance, and I only mention the circumstance as a suggestion to persons who may think proper to repeat the experiment.

Newcastle-upon-Tyne, Jan. 23, 1841. WM. GEO. ARMSTRONG.

XXVIII. *On Irish Tin Ore.* *By* AQUILA SMITH, *Esq., M.D., M.R.I.A.*[*]

THE question has been often asked, is tin ore found in Ireland? and I believe the only reply which could be given is, that it was said to have been found in the county of Wicklow, about the year 1796, when the gold mines were worked on account of the Government.

In the first report on the working of the gold mines, drawn up by Messrs. Mills, King, and Weaver, dated 1st August, 1801, and published in the second volume of the Transactions of the Dublin Society, the directors state, that "in every instance, where the gold has been found, there have been also found fragments of *magnetic iron ore*, and quartz containing *chlorite*, *iron ochre*, and *martial pyrites*, attended, more particularly at the works of Ballinvally (on the north-east declivity of Croaghan mountain), with *specula iron ore*, *brown* and *red ironstone*, *tin-stone crystals*, *wolfram*, and gray ore of *manganese*" (Part ii. p. 147.).

Here we find the existence of "tin-stone crystals," announced for the first time in Ireland; and, strange to say, its discovery does not appear to have attracted any attention in a commercial point of view, nor even to have been sought for as a curiosity by the mineralogists of that time; for although many minerals newly discovered in Ireland are briefly described in the catalogue of the minerals in Trinity College Museum, drawn up in 1807 by the late Rev. Walter Stephens, no specimen of Irish tin-stone is mentioned; and even at a later period, in 1818, when my respected friend, Dr. Whitley Stokes, published his catalogue, Irish tin-stone is not to be found in the list.

---

[*] Read before the Geological Society of Dublin, Wednesday, December 9th, 1840; and now communicated by the Author.

These circumstances would almost induce us to suspect that some other substance must have been mistaken for tin-stone by the directors of the works at the gold mine.

The next notice of this metal being found in Ireland is in the catalogue of Irish minerals in the museum of the Royal Dublin Society, published in 1832 by the late Sir Charles Giesecke. No. 213 is thus described: "tin-stone of a hair-brown colour, accompanies frequently, in small grains, the native gold found in streams at Croaghan mountain, county Wicklow."

Ever since I have turned my attention to mineralogy, I have anxiously sought for a specimen of Irish tin ore, in our public museums, as well as in several private collections of Irish minerals, but was unsuccessful in discovering any such specimen.

About six weeks ago I received, through my friend Mr. Robert Mallet, a small paper of washed sand from the gold works at present carrying on at Croaghan mountain, under the superintendence of Mr. Suter, who sent me the sand for the purpose of having it examined.

I found in it the following minerals:—

1. Several minute particles of native gold.
2. Magnetic iron ore consisting chiefly of very minute and brilliant octahedral crystals, readily separated by the magnet.
3. Fine-grained specula iron ore, giving a red streak when scratched with a knife, and becoming magnetic when heated.
4. Numerous minute garnets of a pale red colour, some of them exhibiting the planes of a rhombic dodecahedron, and fusing readily without intumescence into a brilliant black globule.
5. Many fragments of an earthy looking mineral, some of them presenting crystalline planes, abraded by friction; the fracture was shining, and the hardness about = 6·0 or that of felspar. Not being certain as to what this mineral was, I proceeded to examine it more particularly. In the forceps when heated it became yellow, and in a strong heat deposited a white sublimate on the points of the forceps, but it did not fuse. I then tried it on charcoal, but from the difficulty of keeping so small an assay steadily fixed under the flame, the

only result was that it became yellow when heated. I next tried it with borax, in which it dissolved very slowly, and did not colour the glass. I was still at a loss to say what it was, but suspecting that it contained some metal, which was indicated by the white sublimate, I tried it with carbonate of soda on charcoal, and it speedily yielded brilliant metallic globules, which tarnished rapidly after cooling; they were malleable, and when flattened out presented the appearance of tin.

No doubt now remained as to its nature; and I have only to add, in corroboration of the assertion of Messrs. Mills, King, and Weaver, that "native oxide of tin" exists in the county Wicklow.

XXIX. *Introduction to an Essay on the Amount and Distribution of the Multiplicity of the Roots of an Algebraic Equation. By* J. J. SYLVESTER, *F.R.S. &c., Professor of Natural Philosophy in University College, London*[*].

I USE the word *multiplicity* to denote a number, and distinguish between the total and partial multiplicities of the roots of an algebraic equation.

There may be r different roots repeated respectively h_1 h_2 ... h^r times.

r is the index of distribution.

h_1 h_2 ... h_r are the partial mutiplicities, and if $h = h_1 + h_2 + \ldots + h_r$

h is the *total* multiplicity.

The total multiplicity it is clear may be defined as the difference between the index of the equation and the number of its roots distinguishable from one another.

In this Introduction, I propose merely to consider how existing methods may be applied to determine the amount and distribution of multiplicity in a given equation, and conversely, how equations of condition can be formed which shall imply a *given* distribution and amount.

Let the greatest common factor between fx (the argument of the proposed equation) and $\frac{dfx}{dx}$ be called $f_1 x$.

And in like manner, let the greatest common factor of $f_1 x$ and $\frac{df_1 x}{dx}$ be called $f_2 x$. and so on, till in the end we come to $f_r . x$, which has no common factor with $\frac{df_r x}{dx}$.

Let k_1 k_2 ... k_r denote the degrees in x of fx $f_1 x$ ... $f_r . x$ respectively.

---

[*] Communicated by the Author.

It is easy to see that

$k_1 - k_2$, partial multiplicities, are less than 2, *i. e.* are each units.

$k_2 - k_3$, partial multiplicities, will be less than 3, and therefore either 1 or 2 in value respectively, and so on till we come to

$k_{r-1} - k_r$ which will severally be between zero and $r-1$, and $k_r \quad - 0$ of values intermediate between zero and r.

Hence there will be

$k_1 - 2k_2 + k_3$ multiplicities each of the value 1.
$k_2 - 2k_3 + k_4$ ... ... 2.
... ...
$k_{r-1} - 2k_r$ ... of the value $r-1$.
and k_r ...... of the value r.

In place of fx with $\frac{d_1 fx}{dx}$ we might employ $\frac{dfx}{dx}$ with $\frac{d^2 fx}{dx^2}$ and so on for the rest; the values of $k_2\, k_3 \dots k_r$ will remain unaffected by this change; but the former method would be more expeditious in practice.

The total multiplicity is, of course, $= k_1$.

Suppose now that we propose to ourselves the converse problem to determine the conditions that an algebraic equation may have a given amount of multiplicity distributed in a given manner.

If $h_1\, h_2\, h_3 \dots b_r$ be used to denote the given number of partial multiplicities which are respectively of the values 1 2 3 ... r, it is easy to see that the quantities derived above by $k_1\, k_2 \dots k_r$ are respectively equal to

$$\begin{array}{l} h_1 + 2h_2 + \dots\dots + rh_r \\ h_2 + 2h_3 + \dots\dots + rh_{r-1} \\ h_3 + 2h_4 + \dots\dots + rh_{r-2} \\ \quad \dots \quad \dots \quad \dots \quad \dots \quad \dots \\ h_r. \end{array}$$

Now from $\frac{dfx}{dx}$ having a factor of the degree k_1 common with fx we obtain k_1 conditions from $\frac{df_1 x}{dx}$ having a factor of the degree k_2 common with $f_1 x$ we obtain k_2 more, and so on. So that altogether we obtain in this way

$$k_1 + k_2 + \dots\dots k_r \text{ conditions.}$$

But it may easily be seen that the total multiplicity being k, the number of conditions *need* never to exceed k_1 in number, no matter what its distribution may be. Hence, besides the enormous labour of the process, and the extreme complexity of the results, we obtain by this method more equations by far than are necessary, and it requires some caution to know which to reject.

In my forthcoming paper (to appear in Phil. Mag. of next month) I shall show, by a most simple means, how without the use of derived or other subsidiary functions, to obtain the simplest equations of condition which correspond to a given distribution of a given amount of multiplicity.

The total multiplicity, say m, being given in as many ways as that number can be broken into parts, so many different systems of m equations can be formed differing each from the other in the dimensions of the terms.

These systems may be arranged in order so that each in the series shall imply all those that follow it, and be implied in all those that go before, without the converse being satisfied.

The subject of the unreciprocal implication of systems of equations is a very curious one, upon which the limits assigned to me prevent me from enlarging at present. It is closely connected with a part of the theory of elimination, which, as far as I am aware, has either been overlooked, or has not met with the attention which it deserves; I mean the theory of *Special Factors*.

An *example* may make what I mean by these clear.

Let C be a function (if my reader please) void of x, which equivalent to zero implies two given equations in x having a common root.

Let C be rid of all irrelevant factors, *i. e.* let C be the simplest form of the determinant, when the coefficients of the two equations are perfectly independent qualities. Now suppose, as is *quite possible in a variety of ways*, that such relations are instituted between the coefficients alluded to as make C split up into factors, so that $C = L \times M \times N = 0$.

Only one of the factors, L, M, N will satisfy the condition of the co-existence of the two given equations: the others are clearly, however, not to be confounded with factors of solution, or irrelevant factors, as they are termed, but are of quite a different nature, and enjoy remarkable properties, which point to an enlarged theory of elimination, and constitute what I call special or singular factors.

I shall feel much obliged to any of the readers of your widely circulated Journal, interested in the subject of this paper, who would do me the honour of communicating with me upon it, and especially if they would (between now and the next coming out of the Magazine) inform me whether any thing, and if so how much, different from what is here stated has been done in the matter of determining the relations between the coefficients of an equation corresponding to a given amount and distribution of multiplicity in its roots.

I ought to add, that my method enables me not merely to determine the conditions of multiplicity, but also to decompose the equations containing multiple roots into others free of multiplicity, *i. e.* to find, *à priori*, the values of the several quantities

$$\frac{fx \cdot f_2 \cdot x}{(f_1 \cdot x)^2}, \frac{f_1 \cdot x \cdot f_3 \cdot x}{(f_2 \cdot x)^2}, \ldots\ldots, \frac{(f_{r-1} \cdot x}{(f_r \cdot x)^2}, f_r \cdot x.$$

Moreover, other decompositions, not necessary to be enlarged upon in this place, may be obtained with equal facility.

University College, London, or 58 Doughty Street.

[To be continued.]

XXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xvii. p. 387.]

Nov. 19, 1840. A PAPER was read, entitled, Supplement to a paper "On the Theoretical Explanation of an apparent new Polarity in Light;" by George B. Airy, Esq., M.A., F.R.S., Astronomer Royal.

In a paper published in the second part of the Philosophical Transactions for 1840*, the author explained, on the undulatory theory of light, the phænomena observed by Sir David Brewster, and apparently indicating a new polarity in light. That explanation was founded on the assumption that the spectrum was viewed out of focus; an assumption which corresponded with the observation of the author and of other persons. But the author having, since the publication of that memoir, been assured by Sir David Brewster that the phænomenon was most certainly observed with great distinctness when the spectrum was viewed so accurately in focus that many of Frauenhofer's finer lines could be seen, he has continued the theoretical investigation for that case, which had been omitted in the former memoir, namely, when the spectrum is viewed in focus; and he has arrived at a result, which appears completely to reconcile the seemingly conflicting statements, and to dispel the obscurity in which the subject had hitherto been enveloped.

Nov. 26, 1840.—Description of the Electro-magnetic Clock. By C. Wheatstone, Esq., F.R.S.

The object of the apparatus forming the subject of this communication, is stated by the author to be that of enabling a single clock to indicate exactly the same time in as many different places, distant from each other, as may be required. Thus, in an astronomical observatory, every room may be furnished with an instrument, simple in its construction, and therefore little liable to derangement, and of trifling cost, which shall indicate the time, and beat dead seconds audibly, with the same precision as the standard astronomical clock with which it is connected; thus obviating the necessity of having several clocks, and diminishing the trouble of winding up and regulating them separately. In like manner, in public offices and large establishments, one good clock will serve the purpose of indicating the precise time in every part of the building where it may be required, and an accuracy ensured which it would be difficult to obtain by independent clocks, even putting the difference of cost out of consideration. Other cases in which the invention might be advantageously employed were also mentioned. In the electro-magnetic clock, which was exhibited in action in the Apartments of the Society, all the parts employed in a clock for maintaining and regulating the power are entirely dispensed with. It consists simply of a face with its second, minute and hour hands, and of a train of wheels which communicate motion from the arbor of the second's hand to that of the hour hand, in the same manner as in an ordinary clock train; a small electro-magnet is caused to act upon a peculiarly constructed wheel (scarcely capable of being described without a figure) placed on the second's arbor, in such manner that whenever the temporary magnetism is either produced or destroyed, the wheel, and consequently the second's hand, advances a sixtieth part of its revolution. It is obvious, then, that if an electric current can be alternately established and arrested, each resumption and cessation lasting for a second, the instrument now described, although unprovided with any internal maintaining or regulating power, would perform all the usual functions of a perfect clock. The manner in which his apparatus is applied to the clocks, so that the movements of the hands of both may be perfectly simultaneous, is the following. On the axis which carries the scape-wheel of the primary clock a small disc of brass is fixed, which is first divided on its circumference into sixty equal parts; each alternate division is then cut out and filled with a piece of wood, so that the circumference consists of thirty regular alternations of wood and metal. An extremely light brass spring, which is screwed to a block of ivory or hard wood, and which has no connexion with the metallic parts of the clock, rests by its free end on the circumference of the disc. A copper wire is fastened to the fixed end of the spring, and proceeds to one end of the wire of the electro-magnet; while another wire attached to the clock-frame is continued until it joins the other end of that of the same electro-magnet. A constant voltaic battery, consisting of a few elements of very small dimensions, is interposed in any part of the circuit. By this arrangement the circuit is periodically made and broken, in consequence of the spring resting for one second on a metal division, and the next second on a wooden division. The circuit may be extended to any length; and any number of electro-magnetic instruments may be thus brought into sympathetic action with the standard clock. It is only necessary to observe, that the force of the battery and the proportion between the resistances of the electro-magnetic coils and those of the other parts of the circuit, must, in order to produce the maximum effect with the least expenditure of power, be varied to suit each particular case.

---

* An abstract of which appeared in L. E. & D. Phil. Mag., vol. xvii. p. 381.—Edit.

In the concluding part of the paper the author points out several other and very different methods of effecting the same purpose; and in particular one in which Faraday's magneto-electric currents are employed, instead of the current produced by a voltaic battery: he also describes a modification of the sympathetic instrument, calculated to enable it to act at great distances with a weaker electric current than if it were constructed on the plan first described.

ROYAL ASTRONOMICAL SOCIETY.

February 14, 1840. *Annual Meeting.*

Extracts from the Report of the Council.

"Twenty years have now elapsed since this Society was instituted: during which period, the advances in Astronomy, both theoretical and practical, must be evident to the most ordinary observer. For, not only have public and private observatories been multiplied beyond any former example, but a great impulse has, generally, been given to every department of the science. There is now scarcely a single branch of astronomy that is not pursued in detail by one or other of the many active astronomers of the present day; whether requiring the laborious exertions of the observatory, or the equally arduous investigations of the closet. In the construction and manipulation of instruments also, great and important improvements have been made, which have introduced a system of accuracy and minuteness unprecedented in former times. If this Society has not, indeed, been the originator of this happy state of things, it has at least cordially assisted in the impulse: and let us hope that the next twenty years will be distinguished by similar exertions, and crowned with equal success.

"Amongst the losses by death, during the past year, the Council have to announce the decease of his Majesty the King of Denmark, one of the honorary members of the Society, and a great patron of the science of Astronomy. In the year 1832, his Majesty founded a gold medal, for the first discovery of a telescopic comet not previously known; and in November 1835, the conditions on which the medal was to be awarded were read at the meeting of this Society, and published in the Monthly Notices. It is somewhat remarkable, that from that period till within a few hours of his Majesty's decease, when the recent comet was observed by M. Galle at Berlin, no discovery of the kind had been made; although, some years previously, a considerable number of comets had made their appearance: and his Majesty's liberal intention had, consequently, been nearly frustrated. It will be gratifying to the Society to know that his Majesty's successor, the present King of Denmark, is also a patron of our science; and it is, therefore, with much pleasure, that the Council propose that he be elected an honorary member, in the place of his late predecessor*.

"The other losses that the Council have to deplore, are those of Mr. Davies Gilbert, Professor Rigaud, and Mr. Epps.

"Mr. Davies Gilbert was born on the 6th of March, 1767, in the parish of St. Erth, in the west of Cornwall. His father was the Rev. Edward Giddy, his mother Catherine Davies, a descendant of William Noye, attorney-general in the reign of Charles the First. The subject of this memoir was reared with great care and attention as a child of promise but not of robust health. His early education was conducted at home by his father, who was well qualified for the task as a scholar, and as a man of acknowledged ability and attainments; but his pupil's taste soon led him to prefer the study of the severer sciences to the elegancies of classical literature; and these studies were pursued with an ardour not to be repressed. As he grew up it was thought expedient to place him in the grammar-school of Penzance (of which the Rev. James Parkin was then the master); and for that purpose, his parents removed for about eighteen months to that town. In the year 1782 they removed to Bristol, where their son's studies were assisted, for a time, by Mr. Benjamin Donne. In the year 1786 he was matriculated at Oxford, after having been entered as a gentleman-commoner of Pembroke College. He had already made himself master of considerable mathematical and physical knowledge, and had acquired the greater portion of it by almost unassisted application. His efforts had been guided and aided, indeed, by a very friendly intercourse with the Rev. Malachy Hitchins, vicar of St. Hilary (whose connexion with the late Dr. Maskelyne and the 'Nautical Almanac' is well known), from whom, as he has himself recorded, he obtained, whenever it was asked, information 'in those sciences which afforded him uninterrupted entertainment and delight throughout the whole continuance of a protracted life.'

"During his residence at Oxford, he was a regular attendant at the lectures on anatomy and mineralogy of Dr. Thomson, at Christ Church. He likewise attended with assiduity the lectures on chemistry and botany of Drs. Beddoes and Sibthorp, and formed with them alliances of reciprocal friendship, which terminated only with their lives. His society was, in fact, courted by his seniors, and included a long list of the most eminent among the professors and other distinguished men of the University.

"He took the honorary degree of M.A., and continued to reside much in his college, until, in 1793, he returned to Cornwall to serve the office of sheriff, where his time was divided between the cultivation of science and literature, and the duties of a magistrate in a populous and busy county.

"Soon after this, we find him brought into closer contact with the world, and enabled to display, upon the wider stage of the metropolis, those powers of mind which had been hitherto confined to the narrower sphere of his native county, and a few private admirers and friends. He represented Helston in parliament in the year 1804; and at the general election in 1806, he was chosen to represent Bodmin, and continued to sit for that borough, till, in December 1832, he ceased to be a member of the legislative body.

---

* A resolution to this effect was proposed at the meeting, and unanimously carried.

"On the 8th of April, 1808, he married Mary Ann Gilbert, only niece of Charles Gilbert of Eastbourn, in Sussex, under whose will he came into possession of considerable estates in that county; and, in compliance with its injunctions, assumed the name and arms of Gilbert.

"Mr. Gilbert's exertions to promote the objects of science in general, and to forward the views of the different scientific Societies of London and other parts of the United Kingdom, with which he was connected, both in the House of Commons and with the leading men of the state, are well known, and will be long held in remembrance. He had been early admitted into the Royal Society, of which he never ceased to be an active and distinguished member; and, in 1827, was elected, without opposition, their President, on the resignation of Sir Humphry Davy, whose friend and patron he had been in the early life of that illustrious chemist and philosopher. He resigned that chair in November 1830, and was succeeded in it by H.R.H. the Duke of Sussex. He filled the office of President of the Royal Geological Society of Cornwall, from its origin till his death.

"Mr. Gilbert, as a member of the legislature, and a distinguished cultivator of science, appeared to government a proper assistant in all public inquiries for which station in society, and a competent knowledge of mathematics, formed necessary or useful qualifications; and he was accordingly appointed a member of the Board of Longitude, a Commissioner of Weights and Measures, and a Commissioner to settle the Boundaries of Boroughs under the first Reform Bill. He was also Chairman of the Committee appointed to consider the subject of the Measurement of the Tonnage of Shipping; whose plan (proposed by one of our Fellows, Mr. Riddle) was passed into a law. He was, indeed, at all times willing, when called upon, to devote his time and acquirements either to the public service of his country, or to the private aid of those who appealed to him for assistance or advice.

"After his retirement from Parliament and the Chair of the Royal Society, he did not resign himself entirely to repose; but continued, notwithstanding his increasing infirmities of body, actively to co-operate with his fellow-labourers in the great field of science. He accepted a commission of investigation into the Stannary Laws of Cornwall, and frequented the Societies of which he was a member with undiminished alacrity and zeal, always prepared to obey the calls of duty or friendship with readiness of purpose and gentleness of manners.

"Mr. Davies Gilbert was not a very large contributor to the Philosophical Transactions. In 1826, he gave a paper on Suspension Bridges, accompanied by tables, which is the most complete account yet published of the points on which it treats. In 1827, he read a paper on the expediency of assigning specific names to all such functions of simple elements as represent definite physical properties; and in 1830, a description of the progressive improvements made in Cornish steam-engines. Very different from these, in the subject treated, is a paper on the nature of negative and imaginary quantities, read in 1830. His last contribution, a continuation of the tables relative to suspension bridges, was made on May 19, 1831.

"He had for some years exhibited symptoms of decay. What were the causes of the atrophy so visible in his form and countenance is unknown; but in 1839, he became weaker in strength and spirits; and though he made a journey to Durham, and afterwards into Cornwall, where he presided for the last time at the Anniversary of the Royal Geological Society of Penzance, and attended that of the Polytechnic Society of Falmouth, he was evidently unequal to the task he had imposed upon himself, though his powers of mind were clear, quick, and apparently unimpaired. His last visit was to Oxford, which had, some years before, conferred on him one of the highest titles in her power to bestow. From that period he never went into public, but took his last journey from London to his house at Eastbourn, on the 7th November. He expired in the presence of his family, on the 24th of December, 1839; and his memory will be long cherished as that of an ardent lover of science, a liberal patron of merit, a kind friend, a mild and accomplished gentleman.

"Mr. Rigaud* was descended from a French family of consideration, who, on the revocation of the Edict of Nantes, resigned their property, and fled to a foreign land for conscience sake. His maternal grandfather and father had each successively filled the office of Observer to the King, at Kew; an office which was afterwards graciously conferred on Mr. Rigaud himself. Mr. Rigaud was born in the year 1774; and in 1791, when little more than sixteen years of age, was matriculated at Exeter College, Oxford. He was elected a Fellow of that Society before he was of sufficient standing to take the degree of B.A.; and, as soon as his age permitted, was appointed one of the College tutors. On the death of Dr. Hornsby, in 1810, Mr. Rigaud was appointed Savilian Professor of Geometry, and Reader on Experimental Philosophy; the duties of which latter office he had for some time before been discharging for Dr. Hornsby. In 1827, Mr. Rigaud succeeded Dr. Robertson in the office of Savilian Professor of Astronomy and Radcliffe Observer, thereby relinquishing the Professorship of Geometry. As an astronomer, Mr. Rigaud's attention was principally directed to the literary and historical department of the science, for which his extensive learning, and accuracy of research, especially qualified him. In 1831, he published in a 4to volume, "The Miscellaneous Works and Correspondence of the Rev. James Bradley," to which he pre-

---

[* See L. and E. Phil. Mag., vol. xv. p. 524.—Edit.]

fixed copious memoirs of Bradley himself. This work contains, besides several other unedited papers of Bradley, his original observations for the determination of the constants of aberration and nutation. These observations had been lost sight of for upwards of seventy years; and, but for Mr. Rigaud's zeal and exertion, would, in all probability, never have been recovered. Fortunately, however, it came to his knowledge that several MSS. of Bradley still existed among the papers of Dr. Hornsby, then in the possession of his family, by whom they were readily given up on application from the University. Mr. Rigaud was requested to undertake the office of editor; a task which was not an easy one, owing to the confused state of the materials from which he had to derive his information. He succeeded, however, in producing a work which will ever be regarded as a most valuable record in the history of our science. So highly was it esteemed on the Continent, that in the very next year after its publication, the Royal Academy of Sciences of Copenhagen made the reduction of the observations for aberration and nutation the subject of their prize; which was adjudged to Dr. Busch of the Kœnigsberg Observatory, the assistant of that illustrious astronomer to whom the reputation of Bradley is so deeply indebted. As connected with this subject, it may be mentioned, that it was through the instrumentality of Mr. Rigaud, that his late Majesty, King William IV., was pleased to cause a monument to be erected at Kew, to mark the spot where Bradley made the observations which led to his great discoveries. In 1835, Mr. Rigaud published a small pamphlet, containing an account of the 'Astronomiæ Cometicæ Synopsis' of Halley, and, in 1838, the last work he lived to complete, 'An Historical Essay on the first publication of Sir Isaac Newton's Principia;' which exhibits, in every page, the author's minute acquaintance with the events of that important period. Besides these works, Mr. Rigaud was the author of many papers read before the Ashmolean Society in Oxford; and also of one 'On the Principal Instruments at Greenwich in the time of Dr. Halley,' which is inserted in the ninth volume of our Memoirs.

"In 1827, Mr. Rigaud met with a severe domestic affliction in the loss of his wife; an event which left him sole guardian of a large family of young children; to the superintendence of whose education much of the attention of the latter years of his life was devoted. The affection and solicitude with which he discharged this duty was rewarded by his being spared to witness the academic distinction of his eldest son, who is now a Fellow of our Society; and the Council are happy in being able to state that he is completing the publication of a collection of letters from scientific men in the beginning of the last century, upon which his father was engaged at the time of his death; and the original documents of which formerly belonged to Mr. Jones, the father of Sir William Jones, but now in the possession of the Earl of Macclesfield.

"Many members of this Society have had opportunities of observing the kindness and unaffected simplicity of manner which marked Mr. Rigaud's intercourse in private life; and some of them, the more sterling qualities of his character. For many years past, however, he had entered but little into society. His almost constant residence, for nearly half a century, was Oxford; and there he has left a large circle of friends, who had abundant opportunities of knowing his virtues, and who will long regret his removal. In him the University has lost a most devoted son; and it is now a consolation to remember that he was ever foremost among those whom she delighted to honour.

"Mr. James Epps was appointed Assistant-Secretary of this Society in 1830, and during the eight years he officiated in that capacity, he not only merited the approbation of the Council by the ability and zeal with which he discharged the duties of his office, but also rendered himself acceptable to the Fellows at large by his uniform urbanity, his cheerful disposition, and his readiness to oblige on all occasions. Although he had not the advantages of a regular education, and the occupations of his early life left but little leisure for the cultivation of the sciences, he had acquired, nevertheless, very considerable knowledge both of theoretical and practical astronomy; and he had also much skill and experience in astronomical computation. He was the author of several papers printed in our Memoirs; namely, one in the fourth volume, accompanied by some useful tables for computing the azimuthal deviations of a transit instrument from the observed passages of two stars through the vertical it describes; another, in the same volume, on the errors of the same instrument occasioned by the inclination of the axis to the horizon; one, in the sixth volume, on the method of ascertaining the comparative rates of chronometers; and one, in the ninth volume, on the investigation of formulæ for reducing observations made with the annular micrometer. He likewise recently contributed another paper on the errors that may be produced in determining differences of longitude by observations of moon-culminating stars, when there are no corresponding observations; accompanied by a table of results deduced from comparisons of such observations, with the places given in the 'Nautical Almanac,' which has been ordered by the Council to be printed in the forthcoming volume.

"In 1838, Mr. Epps resigned his office in the Society, and removed to Hartwell to superintend the private observatory of our excellent Treasurer, Dr. Lee. For this appointment he was eminently well qualified. He entered on its duties with his usual ardour; thus meriting the friendship and esteem of his patron, which he continued to enjoy without interruption to the hour of his death. On his removal to Hartwell, he was elected a Fellow of this Society.

"Mr. Epps was a man of varied accomplishments and extensive general information; and knew well how to turn theoretical knowledge to practical account. He was born in 1773, of humble but respectable parents residing in Kent; and died at Hartwell on the 10th of August last, regretted by all who knew him.

"In the Report of the Council to the Society in the year 1838, it was stated that the pendulum observations made by the late lamented Lieut. Murphy in Asia, had been received, and placed in the hands of Mr. Baily, for examination and reduction. Since that period, the pendulums themselves have arrived, and Mr. Baily has repeated his experiments on them, for the purpose of comparing the results before and after the voyage, with those made by Lieut. Murphy. A Report on the whole of these experiments, and on their general result, will be made by Mr. Baily, and read to the Society at one of the evening meetings. These pendulums are now entrusted to Capt. James Clark Ross, as already mentioned, for the purpose of making further experiments at such places as he may find it convenient, during his present scientific voyage: and they are thus, for the third time, placed in active operation.

"As connected with this subject, it may be mentioned that when Mr. Maclear departed for the Cape of Good Hope, to take the superintendence of the Observatory there, he took with him one of Kater's invariable pendulums, that had been previously swung in this country by Mr. Baily. That pendulum has recently been returned to this country, together with a detail of Mr. Maclear's experiments. The whole have been placed in the hands of Mr. Baily, who will, in this case also, report upon the general result.

"In alluding to the labours of Mr. Maclear at the Cape of Good Hope, the Council may mention also his intention of remeasuring the arc of meridian formerly measured by Lacaille. Already have the stations of Lacaille been satisfactorily identified, and the latitudes of the extreme stations been observed, as we have seen by the paper recently read to this Society: and the requisite apparatus has been sent out for finishing what has been so auspiciously begun. Under the direction of this able and zealous astronomer, there is every reason to expect a satisfactory result to so important an operation.

"The Council have great pleasure in stating that the eleventh volume of the Memoirs of the Society is now in the press, and that considerable progress has been made in the printing. Amongst the papers that will appear therein, is a very valuable catalogue of all the stars that were observed by Mr. Airy during the time that he had the superintendence of the Observatory at Cambridge. Partial lists of those stars had from time to time been printed in the several volumes which annually proceeded from that Observatory; subject however to a slight correction for reducing them to one and the same equinox. In the catalogue about to appear in our Memoirs, they are all uniformly reduced to the epoch 1830, with the annual precessions annexed; and are thus immediately available for occasional reference and application. This catalogue will be found to fill up many *lacunæ*, and tend to rectify many errors in former catalogues, arising either from imperfect observation, or from mistakes in transcribing or computing authentic records. It has been found of great assistance in perfecting and enlarging the catalogue which goes under the name of the Catalogue of this Society; as it contains several stars that had not been observed since the original observations by Hevelius, Flamsteed, and Bradley. With a view to extend its utility still further, the Council have directed that an additional number of copies should be printed, and presented to the Rev. Mr. Challis, the director of the Cambridge Observatory, with a request that they might be distributed with the forthcoming volume of the 'Cambridge Observations:' a request with which he has readily complied.

"It is also with much pleasure that the Council can state that the extension of the Society's 'Catalogue of Stars,' just alluded to, and which has been undertaken at the suggestion and at the expense of the British Association, is in great progress, and will probably be completed before the next anniversary. It is intended that this enlarged catalogue shall contain not only every known star in the catalogues of Hevelius, Flamsteed, Bradley, Mayer, Lacaille, and Zach, but also every star, in any of the more modern catalogues, of the sixth magnitude, in whatever part of the heavens it may be situated, and every star in such catalogues not less than the seventh magnitude, within 10° of the ecliptic; together with every other star that, from its peculiar position, suspected proper motion, or other extraordinary circumstance, may be deserving of being thus recorded, and pointed out for further observation.

"In the execution of this work, considerable assistance has been afforded by the four several catalogues published by Mr. Thomas Glanville Taylor, at Madras. The second of those catalogues contains nearly all the stars in the Society's catalogue, visible in that latitude; and the last two exhaust nearly the whole of Piazzi's celebrated catalogue. The total number of stars contained in these four volumes is upwards of 8800; most of which have been observed more than once, and many of them more than five times. The whole have been of essential advantage in completing and perfecting the extension of the Society's catalogue above mentioned: since it has enabled the computers not only to verify the positions of nearly all the stars, but also, in most cases, to deduce the proper motion (if any) that belongs to each of them respectively. The establishment of this observatory is highly honourable to the East India Company, and the fruits which it has produced reflect great credit on the zeal and assiduity of Mr. Taylor, the active superintendent.

"Another subject also undertaken by the British Association, is the reduction of the stars in the '*Histoire Céleste*' (a work containing about 50,000 observations), together with the annual precession annexed to each star. About one-half of this work is already executed; and, when completed, it will afford a ready and convenient reference to almost all the stars (not circumpolar) that are visible in this latitude with an ordinary telescope. The positions of the stars are reduced to the epoch 1800, by means of the very convenient tables of M. Schumacher; a work which renders it scarcely necessary that the computations should be done in duplicate; for, as every star will be referred to its original authority in the printed work, the astronomer will have an easy and ready mode of verifying any suspected result, and of rectifying any error that may be discovered.

"To the British Association also, astronomers are indebted for another work of a similar kind: namely, the reduction of *all* the observations of stars made by Lacaille at the Cape of Good Hope. It is well known that only 1942 of those stars were reduced by Lacaille himself, and formed into a catalogue, which is printed at the end of his '*Cœlum Australe Stelliferum*:' but the great mass of his observations, consisting of upwards of 10,000, have never yet been reduced, although they are of equal authority with those in the published catalogue. The execution of this work is proceeding under the direction of Professor Henderson of Edinburgh, who has been kind enough to supply the elements for the reduction, and to superintend the process of the computations: and there is every reason to believe that Lacaille's new and enlarged catalogue will, as far as the stars in the old catalogue are concerned, be more entitled to confidence (since it is founded on more accurate elements) than the original catalogue published by Lacaille himself. It will, moreover, contain a great number of other stars of equal authority, unknown to astronomers till the appearance of the recent catalogue of Sir Thomas Brisbane.

"Connected with this subject is another work of great interest, likewise proposed by the British Association; namely, a revision of the nomenclature of the stars, and of their division into constellations. It is well known that much confusion at present exists in the notation that has gradually crept into practice: a notation now without order, system, regularity, or uniformity. Hevelius was the first to break in upon the arrangement of the constellations as propounded by Ptolemy; and Flamsteed (or his editors), although he did not disturb the order or number of the constellations in the catalogue of Hevelius, yet introduced some confusion by inserting stars in one constellation that had previously been considered as belonging to another: Bayer's mode of indicating the relative magnitudes of the stars in each constellation was in a great measure lost sight of; and thus the way was led for that mass of confusion which is contained in Bode's large catalogue of 17,240 stars. In the southern hemisphere, we find Halley and Lacaille introducing totally new constellations, frequently overlapping each other, and the stars themselves indicated by such a profusion of letters (many of them precisely similar and several times repeated in the same constellation), that it is often difficult and not always possible to identify them. With the view of applying a remedy to this species of scientific annoyance, the British Association has appointed a Committee, and placed funds at their disposal, for a new arrangement and classification of the stars; preserving as much as possible the old constellations, and Flamsteed's system of numerical order; but correcting gross errors in such arrangement, and confining the adopted constellations to known and definite limits. The present time appears peculiarly favourable for such an undertaking, when so many catalogues are about to be formed into one uniform system. Those only, who have had much experience in such matters, can fairly estimate the convenience and advantage to be gained by such a reform. It is to be hoped that one of the leading members of the Committee will favour this Society with his views and proposals on this subject, in order that it may have the opportunity and benefit of a free discussion, prior to the adoption of so important an alteration.

"From amongst the several distinguished names that were proposed for the Medal this year, the Council have selected that of M. Plana, for his elaborate treatise entitled '*Théorie du Mouvement de la Lune*:' and they trust that this award will meet the approbation of the meeting. The medal will be delivered in the usual manner at the close of the meeting; and the President will, in his address, explain the grounds on which the Council have formed their decision."

The President (Sir J. F. W. Herschel, Bart.) afterwards addressed the Meeting on the subject of the award of the Medal, as follows:—

Gentlemen,—The Report of the Council to which we have just listened (with the painful exception of the losses the Society has sustained in the persons of those respected and lamented members who have been so ably commemorated in that Report) is one calculated to afford most lively satisfaction; and it is so full and complete on every point as to have left me nothing to say, except on that one subject on which, by ancient usage, it has been considered right for the Chairman of this meeting to add a few words of explanation—I mean the award of the Medal for the year.

The award of our medal for this year, gentlemen, to Signor Plana is an act, as it may at first sight appear, of somewhat tardy justice. Those great works on the lunar theory (for which that award is made), and on the perturbations of the planets, especially of Jupiter and Saturn, have now been so long before the public, that it may almost appear as if, in the dearth of matter of sufficient interest of later date, your Council had been ransacking the annals of modern astronomy to find something on which they might rely in a kind of inglorious safety for a justification of their award.

This would be a very erroneous view, indeed, to take of this subject. So far from experiencing a lack of matter to choose from —so far from a deficiency of interest in the subjects which have shared the consideration of the Council in coming to the conclusion they have done—there have been, in fact, on probably no occasion, such powerful countervailing claims—and so far from seeking, in this award, a merely safe and justifiable course—it has required no common share of boldness and decision in your judges to put aside those claims, in favour of M. Plana's—of that boldness I mean which is based on justice and a long-sighted view of public utility.

Before I proceed, therefore, to state the reasons which have weighed with the Council to take the step they have done, it will be right for me to mention, at least in general terms, two of the subjects which have chiefly divided their attention on the occasion; and this I am fortunately enabled to do, infinitely better than I could pretend to do it on my own knowledge and reading, by the aid of most excellent reports on those subjects laid before the Council by Professor Airy and Mr. Main—the one on the subject of

Professor Hansen's general researches in physical astronomy, the other on Professor Bessel's and Mr. Henderson's observations on the parallax of those remarkable double stars, 61 *Cygni* and *α Centauri* —observations which it would appear, beyond question, have brought us to the very threshold of that long-sought portal which is to open to us a measurable pathway into regions where the wings of fancy have hitherto been overborne by the weight or baffled by the vagueness of the illimitable and the infinite.

M. Hansen's researches on the lunar and planetary theories are every way most remarkable, and seem likely to lead to results of the utmost generality and importance. He has attacked the great problem of three bodies (extended, in the conception and application of his methods, to the mutual perturbations of *four*) by a method entirely novel in its idea, although based on and starting from Lagrange's idea of the variation of the elements. Of this method, it would not be easy, in words unaided by symbolic expression, to give any distinct account; but its principle may be stated in general terms, as assuming not the elliptic *elements*, but the elliptic *time*, to be subject to perturbation; or, in other words, as considering the perturbed co-ordinates, each to arise from the combination of invariable *elements* with a varied or perturbed *time*, the amount of whose variation shall exactly account for all that the variation of the elements accounts for in Lagrange's method. The mere mention of this refined and abstruse mode of conceiving the problem must suffice to show, that, to carry it into effect, must require at every step a contention of mind, a degree of intellectual effort, far surpassing what is required for the mere management of algebraic symbols and developments, however intricate.

Whatever be the skill and dexterity, however, exhibited by the author of this truly original conception, and whatever promise it must be considered as holding out for the future advancement of our knowledge in this intricate research, it can hardly yet be regarded as having attained that extent of development which it will require to supersede in the construction of tables, and the actual calculation of the lunar and planetary perturbations, the methods already in use, which the researches of Clairaut, Laplace, Lagrange, Poisson, Damoiseau, and Plana, have wrought up to such a pitch of practical perfection. Hansen's theory appears to afford what, in the actual state of our knowledge, must be regarded as most precious—a new handle by which to seize this refractory problem—one of universal applicability and gigantic power and *purchase*, but of which the management is not yet fully reduced to practice, and of which even the author himself can scarcely yet be said to have acquired the entire mastery. In the theory of Jupiter and Saturn, indeed, the final numerical results are obtained, and tables calculated; but in the lunar theory, which (in the words of Mr. Airy) "must be considered as the ground of his chief analytical triumph, there exist at present only what may be termed the *foundations* for such a theory." "No man living" (I continue to use the words of the eminent geometer last-mentioned), "No man living, probably, except M. Hansen himself, could work it into a complete lunar theory; and the exhibition of numerical results is here, therefore, still distant." Let us hope that he will not long leave them so.

On the other subject to which I alluded—the parallax of the fixed stars—it would be doing an injustice to the valuable report of Mr. Main, which, as a beautiful specimen of astronomical history, I hope to see ere long adorning our Transactions, if I were to avail myself more largely of it on this occasion than is absolutely necessary. It has long been understood by astronomers, that the research of parallax ought not to be confined to the largest stars, but that, in order to determine our choice of stars for this research, other *primâ facie* grounds for suspecting a proximity to our system ought to be taken into consideration; such as great proper motion, or, in the case of a double star, great apparent dimensions of the orbits described about each other. In the case of the double star 61 *Cygni*, both these indications combine to point it out as deserving inquiry. In that of *α Centauri* they also conspire; for it is well known that this fine double star has a considerable proper motion; and my own observations prove, that the mutual orbit described by its individuals about each other, is of unusually large angular dimension. The great brilliancy of the star also, and its situation in a region of the heavens in which the stars, generally speaking, seem to be less remote than in others, all favour the expectation of a measurable parallax being detected in it: and such Mr. Henderson, from his own observations, assigns to it. I am not about to criticise this result; on the contrary, I am disposed to attribute much weight to his conclusion; but it is only on a very long series of observations of *absolute places*, affected as they are by instrumental error and uncertainty of refraction, that any conclusion of this kind can rest with security.

Bessel has attacked the question in a different way, by measuring at all times of the year the angular distance of the stars composing the double star 61 *Cygni* from two small stars visible in the same field of view, and within limits adapted to secure micrometrical measurement. The method is unexceptionable, the measurements conducted with consummate skill, and their reduction executed with all possible regard to every thing likely to influence the result. And that result is, to assign a minute, it is true, but perfectly unequivocal amount of parallax, in a way so striking as hardly to allow a doubt of its reality. Such is the impression on merely reading the numerical statement; but, put in the light in which Mr. Main has placed it, by the graphical projection of the measures, the conclusion seems quite irresistible:

> "Segnius irritant animos demissa per aures,
> Quam quæ sunt oculis subjecta fidelibus, atque
> Ipse sibi tradit spectator."

It may now be reasonably asked, If all this be so, why have your Council hesitated to mark this grand discovery with that distinct stamp of their conviction and applause, which the award of their annual medal would confer? A problem of this difficulty and importance solved, so long the cynosure of every astronomer's wishes—the ultimate test of every observer's accuracy—the great land-mark and *ne plus ultra* of our progress, thus at once rooted up and cast aside, as it were, by a *tour de force*, ought surely to have commanded all suffrages. It is understood, however, that we have not yet all M. Bessel's observations before us. There is a second series, equally unequivocal (as we are given to understand) in the tenour, and leading to almost exactly the same numerical value of the parallax, and not yet communicated to the public. Under these circumstances, it became the duty of your Council to suspend their decision. But, should the evidence finally placed before them at a future opportunity justify their coming to such a conclusion, it must not be doubted that they will seize with gladness the occasion to crown, with such laurels as they have it in their power to extend, the greatest triumph of modern practical astronomy.

M. Plana is well known to the astronomical world as the director of the Observatory at Turin, from which have emanated some valuable series of observations. In conjunction with M. Carlini, he also carried on that extensive and important triangulation of the Savoy Alps, which have made his name celebrated as a geodesist. His works, too, on many other subjects, both astronomical and purely analytical, are of great importance; particularly his investigations on the subject of refraction prefixed to the 'Turin Observations,' from 1822 to 1825, published in 1828; those on the motion of a pendulum in a resisting medium, &c. But it is of his researches on the lunar theory for which our medal has been actually awarded; and of these it behoves me now to speak; and I cannot do so in more clear, concise, and discriminating terms, than those used by Mr. Airy in his Report already alluded to:—

"The method pursued by Plana, in his '*Théorie de la Lune*,' is slightly, but not importantly, different (I mean in the fundamental equations) from those of his predecessors, Clairaut, Laplace, and Damoiseau. He first starts with the method of variation of elements, and pursues it to such an extent as to ascertain generally the form of the expressions connecting the longitude, the latitude, and the time. He then reverts to Clairaut's equations; and, as these equations require for the successive substitutions an approximate expression for time in terms of longitude, he adopts a peculiar form (suggested by the variation of elements) for the principal part of it, and attaches to that principal part a subordinate part marked with the prefix δ. The same thing is done for the latitude. The process then is tolerably direct, and is almost similar to that of antecedent writers. In the fundamental algebra, therefore, there is no very great originality in the plan; but the mode followed in the detail of the work is beyond all praise. In the whole of the analytical combinations of this immense work, every part arising from the combination of any one term (however small) with any other term, is given separately, in such a form as to leave no difficulty in the detection of error to any careful examiner. The terms of peculiar difficulty (as, for instance, that depending on twice the distance between the node and the perigee) are made the subject of special discussion; and, in some instances, the origin of discordance between the author's results and those of Laplace is investigated with the same clearness which prevails through the other operations.

"In one respect, the plan of investigation differs much from those of his predecessors, as well as from Hansen's. The investigation is wholly symbolical: no numerical value is introduced, and no consideration of relation of values entertained, till the final substitutions are made. As an example of theory, there can be no doubt of the beauty of this process. As a subject for practical accuracy, it may not be so certain whether it is advisable. The convergence of the series is sometimes extremely slow. As far as I can observe, the accuracy of this method is exactly and properly that of successive substitution: but, in various parts of the lunar theory (in all places where the terms rise two orders by integration), the method of successive substitution is not sufficient; in fact, it is necessary to assume a term in order to find its correct value. Adopting this method, however, the author has pushed it as far as, probably, it will ever be carried. The whole is worked to the fifth order, and some parts to the seventh order.

"Finally, the author has determined from observations the principal constants which require to be substituted in the symbolical expressions, and has substituted them, and has thus produced a set of numerical expressions which may immediately be used for the formation of lunar tables.

"In terminating the remarks on the works of these two authors, Plana and Hansen, I must again express my very great admiration for both. But their merits are of very different kinds. The theory of Hansen is undoubtedly of the higher order, but it can hardly yet be said to be practical (at least in the lunar theory): many years will yet elapse before it will influence the lunar tables. The theory of Plana is very good, and probably adequate in all respects: it is eminently practical in form: it has already influenced the investigations of other writers, and will probably soon influence the tables."

There is but one thing more to add to this clear and powerful summary, and I will supply it by a quotation from the work itself:—

"Je n'ai pu me faire aider par personne; j'ai du traverser *seul* cette longue chaîne des calculs, et il n'est pas étonnant si par inadvertence j'ai omis quelques termes qu'il fallait introduire pour me conformer à la rigueur de mes propres principes." When we look at the work itself there seems something almost awful in this announcement.

A very important memoir of M. Plana, on the theory of the planetary perturbations, has adorned the Transactions of this Society. The points of which it treats are miscellaneous, and some of them, perhaps, not of the highest importance, except in one point of view, and that, perhaps, the most important of all. Every one who is at all conversant with these researches must be impressed with the enormous interval which separates—I will not say the mere differential equations of the planetary motions—but their integrals after much and intricate development—from the final numerical results on which their tables are to be constructed; that is to say, the computed values of the coefficients of terms having the same argument, when assembled from all the points whence they arise in the algebraic processes and amalgamated together. M. Plana appears to have proposed to himself the gigantic task of revising and correcting not only those algebraic developments, but the actual numerical calculations of the whole '*Mécanique Céleste*;' and this paper contains many examples sufficiently proving the necessity of such revision, and leading the way to those further and more elevated researches on the theory of Jupiter and Saturn, to which the latter part of this memoir must be considered as having given occasion; and which are further developed in several other memoirs published in various academical and other collections.

Neither the time nor the nature of this occasion would allow of my entering into any history of the controversy to which the revision thus set on foot, and the discordant results arrived at in this memoir, gave occasion. Suffice it to say, that errors—venial, no doubt, and such as it would be miraculous did they not exist—were discovered on all sides, and the absolute necessity established not merely of a thorough revision of every part of these immense computations, but of printing and publishing the steps in that regular and methodical form, which alone can put it in the power of subsequent calculators to lay their finger on the precise point where error shall have crept in; and to resume the calculations from that point without sacrificing the whole of what precedes.

It is this methodical clearness—this letting in of the light on every dark corner of every intricate combination and heart-breaking numerical calculation, which may be regarded as marking from this time a new era almost in the planetary theory itself. In the '*Mécanique Céleste*,' we admire the elegance displayed in the alternate interlinking and development of the formulæ, and exult in the power of the analytical methods used; but when we come to the statement of numerical results, we quail before the vast task of filling-in those distant steps, and while cloud rolls on after cloud in majesty and darkness, we feel our dependence on the conclusions attained rather to partake of superstitious trust, or of amicable confidence, than of clear and demonstrative conviction. Let me not be misunderstood as by these expressions casting any reflection on the conduct of that immortal work. The surest proof of its titles to such immortality which can be given, is that microscopic examination subsequently lavished on every point embraced in its immense outline. It is no disparagement to the agriculturist, whose energies have extirpated the wilderness, and established in its place cultivation and wealth, that a period shall arrive when his furrows shall, in their turn, be replaced by the garden, and his system of culture, by a measured and calculated succession. Neither would I be understood to lay the sole stress of our applause of M. Plana's researches on the luminousness of their statement. His analysis is always graceful, his combinations well considered, and his conceptions of the ultimate results to be expected from them perfectly just, and justified by the results when obtained.

It cannot but be agreeable to this meeting to know that our award is duly appreciated by M. Plana himself, and regarded by him in the light which it is ever most desirable it should be,—as a stimulus to fresh researches and further exertions of his powerful talents in the same line where they have already reaped so rich a harvest. No sooner had the Council decided on their award, than, as in private regard no less than in public duty bound, I communicated to him the result; and his reply, which breathes the warmest spirit of attachment to the Astronomical Society, and of undiminished zeal in his own peculiar line of research, is now before me. In the absence of any personal friend to receive it for him, I shall now, therefore, present our medal to Mr. Rothman (in the absence of our Foreign Secretary, Captain Smyth), in his name, and request him to forward it to him, with our best wishes for his health and happiness.

XXXI. *Intelligence and Miscellaneous Articles.*

ANALYSIS OF CHYLE AND LYMPH. BY G. O. REES, M.D., F.G.S., ETC.*

THE following analyses of chyle and lymph have lately been made by Dr. Rees, with the view of ascertaining what those ingredients of chyle are which disappear from the lymph, and which are consequently of most service for the nourishment of the body. The fluids were both obtained from the same animal immediately after death. The subject of experiment was a young ass, which was killed by a blow on the head about seven hours after taking a full meal of oats and beans. The chyle was taken from the lacteal vessels before entering the thoracic duct, and was therefore free from lymph; the lymph was from the absorbents of the lower extremity. Analysis yielded the following results:—

	Chyle.	Lymph.
Water	90·237	96·536
Albuminous matter	3·516	1·200
Fibrinous matter	0·370	0·120
Alcoholic extractive	0·332	0·240
Aqueous extractive	1·233	1·319
Fatty matter	3·601	a trace only.
Salts { alkaline, chloride, sulphate and carbonate, traces of phosphate, oxide of iron.. }	0·711	0·585
	100·000	100·000

On these analyses Dr. Rees makes the following observations:—

The albuminous matter, as obtained from the chyle, contained a substance in admixture which gave it a dead white colour, and which Dr. Rees is inclined to believe is identical with a substance existing in the saliva. It may be obtained from both the chyle and saliva by agitating them with æther, when the peculiar principle appears floating above the inferior stratum of fluid. Iron was found in considerable quantity in the aqueous extractive of chyle; and Dr. Rees throws out the suggestion, whether this peculiarity in the locus of iron may not in some way relate to the absence of the red colouring matter of the blood. The colour of chyle has been generally supposed attributable to the fatty matter contained in it; Dr. Rees, however, is of opinion that the white matter mentioned above as analogous to that existing in the saliva, has a considerable share in the production of the opake milky character of the chyle.

---

* Abstract from the Med. Gaz. for Jan. 1st, 1841.

NEW MINERAL FROM LANGBANSHYTTA, NEAR FAHLUN; DESCRIBED AND ANALYSED BY PROFESSOR O. B. KÜHN OF LEIPZIG.

This mineral lies immediately on a grayish-black mass, of a metallic lustre, which contains oxide of iron, but no trace of arsenic; and this mass itself lies on fine-grained bitter-spar.

The colour of the mineral is in some places like pale honey, in others dirty white; but there are no definite limits between two differently-coloured parts. It is of a waxen lustre. Its specific gravity, determined in water, is 2·52; its hardness between 5 and 6; it is brittle and easily pulverized; only in one direction could I perceive foliated cleavage; in that one the fragments are tolerably even, but in all other directions it splits unevenly.

Heated by itself with the blow-pipe it becomes gray, but does not melt even at the edges; heated in a glass tube it does not yield the smallest quantity of water; with borax and phosphoric salt it effervesces, and a smell resembling that of arsenic is evolved; both glasses are, when melted on platina, transparent, and in every respect nearly colourless; with soda there is, in like manner, effervescence; and indications of manganese are more or less strongly marked in different specimens.

In nitric acid it is perfectly soluble, with greater or less effervescence, in different specimens, though it is in all but trifling: in the fluid we can detect lime, magnesia, some manganese, besides a trace of iron and much arsenic acid: also a trace of chlorine. Fluorine was not to be found in the mineral.

Three analyses have given the following results expressed in numbers:—

Lime	23·22	21·31	20·96
Magnesia	15·68	17·07	15·61
Protoxide of manganese	2·13		4·26
Iron, only a trace			
Arsenic acid	58·52		56·56
Loss by ignition	0·30		2·95
Insoluble matter			0·23
	99·85		99·57

Though the last analysis was even made with the greatest possible care, still the great difference between the quantities of lime and manganese cannot be accounted for; still more the quantity of manganese varies in different parts, which, indeed, might be expected from its external appearance; and then, according to the law of isomorphism, it replaces an equivalent quantity of lime. We may assume, with certainty, that it is carbonic acid which is lost by ignition, but we cannot determine with which base it had been combined; most probably with all three, for the three sorts of carbonates are found in almost immediate contact with each other. Now if we calculate on the last values obtained, being the more carefully determined, and if we assume that the three bases are combined with the quantity of carbonic acid in the same proportions in which they are found throughout, then the lime becomes 1·21; magnesia 0·90; and protoxide of manganese 0·25. After subtracting these carbonates and the insoluble matter, the remaining constituents give the following proportions, to which we shall add the twofold calculation from the atomic weights, and from the quantity of oxygen present in each body.

$\mathrm{Ca\,O}$....	7	199·5	20·90	20·81	5·84	12·81	3
$\mathrm{Mg\,O}$....	7	144·2	15·11	15·50	6·02		
$\mathrm{Mn\,O}$....	1	35·7	3·74	4·22	0·95		
$\mathrm{As\,O_{2\frac{1}{2}}}$..	10	575·0	60·25	59·47		20·69	5
		954·4	100·00	100·00			

The formula hence derived is

$$\left(\begin{matrix}1\frac{1}{2}\,\mathrm{Ca\,O} + \mathrm{As\,O_{2\frac{1}{2}}}\\ \frac{1}{15}\,\mathrm{Mn\,O}.\end{matrix}\right) + \left(\begin{matrix}1\frac{1}{2}\,\mathrm{Mg\,O} + \mathrm{As\,O_{2\frac{1}{2}}}\\ \frac{1}{15}\,\mathrm{Mn\,O}\end{matrix}\right)$$

or, after the mode of Berzelius,

$$\left.\begin{matrix}3\ \mathrm{Ca\,O}\\ 3\ \mathrm{Mg\,O}\\ 3\ \mathrm{Mn\,O}\end{matrix}\right\}\mathrm{As_2\,O_5}.$$

This mineral requires a particular name as a new combination. Neither the composition nor the external properties are sufficiently marked to be expressed by it. I shall therefore make use of a customary resource in mineralogy, and call the substance after one who has advanced the science; and as no one doubts that Berzelius, who, through the invention of the doctrine of proportions and the formation of formulæ, as well as through his innumerable most accurate analyses of mineral bodies, has done so much true service to mineralogy, should be awarded as much honour as those whose names are only brought into the science through the friendship or partiality of some mineralogist, I do not hesitate to propose calling this new compound, first found in Sweden, by the name of Berzeliit.

REMARKABLE SOLAR BOW.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

The following account of a meteorological phænomenon of, I believe, rather a rare character, may not be uninteresting to you.

Being at St. Day with J. S. Enys, Esq., on the afternoon of December 24th, 1840, my attention was called by that gentleman to a solar arc in the west. Around the western horizon were broken masses of cumuli, between and above which the sky was deeply blue, and with the exception of a few light cirri, quite free of clouds. The sun was not more than ten degrees above the horizon; and, at an angle of not more than 12 degrees between the hills to the west of St. Day, appeared the limb of a solar bow, strongly painted, but deficient of the rays of greatest refrangibility, or rather from the singular crimson character of the portion occupied by the red ray, it appeared as if the spectrum was doubled on itself, and the violet mingled with the less refracted rays. The very remarkable appearance and situation of the bow necessarily attracted the attention of many persons, and some who saw it from Falmouth harbour, described it to me as being nearly in a line with the sun.

It is necessary to notice, that no rain was falling: the day was unusually fine for the season; and the temperature, which had during the day been above the freezing point, was then certainly below it. Over the hill on which this phænomenon appeared, was slowly spreading the lightest possible veil of vapour, so thin that a casual observer would have stated that the rays were painted on the most transparent air. It has been suggested, that the phænomenon was produced by an image of the sun reflected from some of the clouds behind the orb itself, and it certainly appears to me highly probable that such was the case; and from its similarity to some of the phænomena I have observed in my experiments on polarized light, I am almost inclined to look upon it as an instance of atmospheric polarization.

I expect the frost bows, of which some accounts have recently been given, are of analogous character to the bow I have described.

I remain, Gentlemen, yours, &c.

ROBERT HUNT,

Falmouth, Jan. 2, 1841. Secretary Royal Corn. Polytechnic Society.

METEOROLOGICAL OBSERVATIONS FOR DEC. 1840.

Chiswick.—Dec. 1. Hazy: overcast and mild. 2. Very fine. 3. Frosty: fine. 4. Sharp frost: overcast. 5, 6. Hazy. 7. Overcast. 8. Rain. 9. Frosty: clear. 10. Thawing: hazy. 11. Hazy. 12, 13. Overcast and cold. 14. Sharp frost. 15. Dry frosty air. 16. Overcast: snowing. 17. Snowing: cloudy: severe frost at night. 18. Frosty: overcast and cold. 19. Hazy: rain. 20, 21. Overcast and cold. 22—24. Severe frost. 25. Intense frost: Dense fog. 26. Thick hoar frost. 27—29. Foggy. 30. Clear: cloudy: rain at night. 31. Cloudy: clear and fine.

The mean temperature of this month was lower than that of any December within at least the last forty years.

Boston.—Dec. 1. Cloudy: rain early A.M. 2—4. Fine. 5—7. Cloudy. 8. Rain: rain early A.M. 9. Fine. 10—13. Cloudy. 14. Cloudy: snow P.M. 15. Cloudy: snow A.M. 16. Snow. 17, 18, Cloudy. 19. Rain. 20, 21. Cloudy. 22—24. Fine. 25—29. Cloudy. 30. Fine: rain P.M. 31. Fine.

This is the coldest December since 1829.

Applegarth Manse, Dumfries-shire.—Dec. 1. Raw but fair. 2, 3. Fine and fair. 4. Slight showers. 5, 6. Drizzling. 7. Wet and stormy. 8. Fair, but cloudy. 9, 10. Fair, but wet preceding night. 11—13. Fair throughout. 14—16. Hard frost. 17. Thaw, with slight drizzle. 18. Frost again. 19. Slight frost A.M.: drizzle. 20. Frost: Aurora Borealis. 21—24. Frost. 25. Frost A.M.: thaw P.M. 26. Frost again, but cloudy. 27. Thaw A.M.: cloudy and dark. 28. Frost but slight. 29. Frost—moderate. 30. Thaw and snow. 31. Raw and drizzly.

Sun shone out 21 days. Rain fell 9 days. Snow 1 day. Frost 14 days.

Wind north 1 day. North-north-east 1 day. North-east 7½ days. East-north-east 1 day. East 4 days. East-south-east 3 days. South-east 3 days. South-south-east 1 day. South 1 day. South-west 3½ days. West-south-west 1 day. West 2 days. North-west 2 days.

Calm 12 days. Moderate 8 days. Brisk 6 days. Strong breeze 1 day. Boisterous 4 days.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTON; *by* Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. VEALL *at Boston, and by* Mr. DUNBAR *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1840. Dec.	Barometer.						Thermometer.								Wind.				Rain.				Dew point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dum-fries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·960	29·998	29·924	29·44	29·68	29·88	51·7	52·8	45·8	56	38	51	48	44	SE. var.	W.	calm	SW.	...	...	·05	...	45
☽ 2.	30·188	30·311	30·137	29·69	30·05	30·33	44·6	55·5	44·7	51	30	39	42	35½	WNW.	N.	calm	NW.	...	...	...	...	42
3.	30·538	30·493	30·493	30·11	30·35	30·32	36·3	47·4	35·7	43	22	31	41	25½	NW.	NE.	calm	WSW.	...	...	...	...	36
4.	30·480	30·465	30·375	30·02	30·22	30·18	32·8	42·3	32·5	41	29	33	44½	40	SW.	SW.	calm	SW.	...	...	...	...	33
5.	30·466	30·353	30·250	29·96	30·16	30·10	39·4	41·0	32·7	42	37	36	46½	34½	SW.	W.	calm	SE.	...	...	...	...	34
6.	30·186	30·171	30·018	29·79	29·90	29·69	40·8	43·5	38·6	42	25	39	46	42	E.	SW.	calm	S.	...	...	...	...	35
7.	29·808	29·817	29·435	29·43	29·33	29·10	39·2	43·7	35·2	41	33	37·5	46	42	E.	S.	s.	SE.	...	...	...	0·50	35
8.	29·230	29·384	29·229	28·95	28·93	29·22	39·7	40·3	34·2	43	26	35	42	38	E.	S.	s.	ESE.	...	·10	·05	...	34
○ 9.	29·738	29·923	29·692	29·36	29·48	29·65	36·3	44·3	36·2	45	26	33·5	41½	33½	S.	SW.	calm	ESE.	·066	...	·26	...	35
10.	29·970	29·910	29·892	29·60	29·66	29·83	40·8	42·0	36·2	44	36	36	46	37	E.	SE.	calm	SE.	...	...	...	...	38
11.	29·958	29·947	29·897	29·65	29·93	29·99	37·6	45·0	35·2	37	32	36	42½	34	ENE.	E.	calm	ESE.	...	...	...	...	36
12.	30·124	30·195	30·086	29·80	30·10	30·22	38·5	39·2	36·0	38	32	38	36½	32½	N.	NE.	calm	ENE.	...	...	...	...	34
13.	30·282	30·257	30·248	30·00	30·28	30·31	33·8	39·7	33·7	37	24	34	37	33	N.	NE.	calm	E.	...	...	...	...	33
14.	30·236	30·227	30·207	29·96	30·25	30·26	27·3	35·4	27·3	32	23	31	34	24	N.	NE.	E.	E.	...	...	...	0·30	25
☾ 15.	30·276	30·245	30·202	29·98	30·29	30·32	27·0	31·3	27·3	26	18	28	34	26½	N.	NE.	calm	NNE.	...	...	...	...	25
16.	30·132	30·154	29·818	29·86	30·11	29·94	28·3	28·5	24·0	33	28	28·5	35½	27	NW.	NW.	w.	NW.	...	...	·20	...	25
17.	29·772	29·762	29·758	29·50	29·82	29·85	31·4	34·6	27·7	28	17	35	36	34	NE.	NW.	calm	NE.	...	...	...	...	28
18.	29·614	29·600	29·500	29·46	29·81	29·74	28·3	31·7	21·2	33	28	35	35½	33	N.	NE.	E.	NE.	...	...	...	...	25
19.	29·588	29·644	29·579	29·39	29·68	29·82	31·3	35·5	29·2	34	32	36	36½	34	N.	NE.	calm	NE.	...	·19	...	...	27
20.	29·840	30·108	30·826	29·65	30·01	30·26	35·8	36·3	29·3	36	34	36·5	38½	33	N.	SE.	calm	N.	...	...	·02	...	29
21.	30·262	30·303	30·247	30·00	30·34	30·38	35·6	37·2	35·0	36	30	37	35½	28½	NE.	NE.	calm	NE.	...	...	...	0·00	30
22.	30·344	30·329	30·255	30·10	30·36	30·25	32·2	37·3	32·3	33	19	32	36½	28	ENE.	NE.	E.	E.	...	...	...	...	28
● 23.	30·176	30·179	30·055	29·90	30·13	30·14	27·5	32·8	26·9	32	21	28	45	26½	NNW.	NE.	calm	NE.	...	...	...	...	25
24.	30·100	30·162	30·088	29·85	30·15	30·33	32·3	34·4	27·0	36	16	31	38½	23	NNW.	NE.	calm	NE.	...	...	...	...	28
25.	30·328	30·366	30·308	30·03	30·34	30·35	30·0	36·7	28·9	28	15	33·5	36½	32	N.	E.	calm	NE.	...	...	...	...	28
26.	30·562	30·595	30·553	30·20	30·42	30·59	29·0	33·4	25·0	31	27	32	38½	33	NW.	N.	calm	SW.NE	...	...	...	...	25
27.	30·656	30·644	30·532	30·30	30·58	30·50	32·3	32·6	29·2	33	29	32	36½	32	NW.	NE.	calm	E.	...	...	...	...	26
28.	30·408	30·400	30·275	30·10	30·30	30·23	30·3	35·3	29·4	32	18	31	37½	31½	E.	E.	calm	SSE.	...	...	...	0·12	28
29.	30·256	30·306	30·248	29·91	30·17	30·34	26·4	33·0	24·2	36	19	28	37½	31	S.	W.	calm	W.	...	...	...	...	26
30.	30·406	30·412	30·164	30·05	30·26	29·77	27·8	37·6	26·2	40	31	27·5	51½	19½	S.	W.	calm	SW.	...	·16	...	...	27
☽ 31.	29·886	29·990	29·873	29·49	29·72	29·83	41·3	42·0	28·5	45	25	41	45	39	W.	NW.	NW.	W.	·347	...	·27	...	30
Mean.	30·121	30·150	30·069	29·78	30·026	30·055	34·4	38·8	31·5	37·55	26·45	34·2	40·2	32·4					Sum. ·413	·45	·85	0·92	Mean.

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

MARCH 1481.

XXXII. *On certain points in the Theory of Undulations, related to the Hypothesis of the Symmetrical Arrangement of the Molecules. By the Rev.* BADEN POWELL, *M.A., F.R.S., &c., Savilian Professor of Geometry, Oxford*[*].

(1.) AMONG the numerous and important accessions which have of late years been made to our knowledge of the theory of light, and the chief of which have from time to time found a place in the pages of this Journal, not the least valuable have arisen out of the free discussion of points of difficulty or objection, whether affecting the stability of the theory itself, or referring to the structure of some of its subordinate parts.

Among such topics, my present remarks relate to one which some time since struck me as of peculiar importance; originating in the first instance in the researches of Mr. Tovey on elliptic polarization[†], and further illustrated by several communications from Sir J. Lubbock[‡], yet involved in some difficulties, from which I do not think anything hitherto published has completely relieved it.

(2.) More precisely: Mr. Tovey's investigation[§] involves an important principle, viz. the existence of a *relation* between the peculiar supposed *arrangement of the molecules* of the ætherial medium in space, and the *nature of the vibration as elliptic or rectilinear.* The character of such arrangement is indicated mathematically by the presence or evanescence of certain terms in the differential equations. These terms had been regarded as *evanescent* by M. Cauchy and others: and this as a consequence derived from the hypothesis of a *uniform* distribution of the ætherial molecules in space. Mr. Tovey's object in the paper referred to is to show that when this is *not the case*, elliptic polarization is the result.

---

[*] Communicated by the Author.

[†] L. & E. Phil. Mag. vol. xii. No. 71. Jan. 1838.

[‡] Vol. xi. No. 69. Nov. 1837; Vol. xii. No. 71. Jan. 1838; Vol. xv. No. 97. Nov. 1839.

[§] While this sheet was passing through the press I have been informed by a friend, that Fresnel adverts to a similar principle, but the time does not allow of my referring to it now more particularly.

Highly valuing this investigation, but conceiving that the remarkable conclusion was not so fully explained as its importance seemed to render desirable, in a paper inserted in the Phil. Trans. for 1838, part ii.*, I endeavoured to establish and further elucidate the conclusion by what seemed to me a more direct and perspicuous method.

The intimate connexion between these theoretical views and the important points discussed in the brief but masterly papers of Sir J. Lubbock, was soon rendered evident. The direct object of these papers was chiefly the illustration of Fresnel's views respecting the axes of elasticity and the wave surface; and there appeared at first sight some degree of contradiction between these deductions and the views just referred to. I endeavoured to draw attention to the difficulty in a short communication to the British Association at Birmingham, 1839†.

In consequence of some correspondence and further discussion, I drew up a supplement to my last-named paper, which was inserted in the Phil. Trans. 1840, part i., in which I pointed out the modifications which the former conclusions would receive in connexion with the principles thus elucidated.

This supplement, however, was very brief: other points also appeared to call for further notice, and the whole investigation seemed to require re-casting. My present object is therefore to offer in the first instance a general review of the main investigation in such a form as I trust may meet all the difficulties which have been urged; and further, to point out its bearing on the several topics with which it stands connected.

(3.) To express undulations we shall suppose the common formulas adopted for the vibration of a single molecule of æther, the assemblage of which expressed by the summation Σ, represents a ray of light: and as in all the investigations on this subject, if $\xi\ \eta\ \zeta$ be the displacement of a molecule in the direction of the three rectangular axes of $x\ y\ z$, and further if we take the ray as coinciding with the axis x, then on the principle of transverse vibrations, we have

$$\begin{aligned} \xi &= 0 \\ \eta &= \Sigma\left[\alpha \sin (n t - k x)\right] \\ \zeta &= \Sigma\left[\beta \sin (n t - k x)\right] \end{aligned} \qquad (1.)$$

[* Of which an abstract appeared in L. & E. Phil. Mag. vol. xiii. p. 222. —Edit.]

† See Sectional Proceedings, p. 3.

where t is the time from the commencement of the disturbance, while the velocity of the propagation of the wave is expressed by $v = \frac{n}{k} = \frac{1}{\mu}$ (2.) and $k = \frac{2\pi}{\lambda}$ (3.) λ being the wave-length in the medium.

In common or unpolarized light the vibrations are in all possible azimuths round x; hence the coefficients α and β are wholly arbitrary and independent.

In plane polarized light we have, on Fresnel's principles,

$$\alpha = \mathrm{A} \cos i \qquad \beta = \mathrm{A} \sin i \qquad (4.)$$

whence i is the angle formed by the plane of vibration with another plane, which he terms the plane of polarization: also the squares of the amplitudes expressing the intensities of light,

$$\alpha^2 + \beta^2 = \mathrm{A}^2.$$

The formulas in this case represent *two* rays polarized in planes at right angles.

If we consider only one ray wholly polarized in either plane, it is equivalent to supposing either $i = 0$, or $i = \frac{\pi}{2}$, or that one of the formulas disappears. (5.)

M. Cauchy, Professor Maccullagh, Mr. Tovey, and other mathematicians term the plane of polarization that *in* which the vibration is performed. M. Fresnel uses the same term to signify the plane *perpendicular* to this. This difference in terms, however, involves consequences which affect the subsequent applications of the theory*. But this will not influence our present investigation.

In the case of elliptical vibrations we have to consider *not*, as in the other cases, a *rectilinear* displacement and its resolved parts, but a *curvilinear* displacement, which is the result of two virtual rectilinear displacements at right angles to each other, and in a plane perpendicular to the ray, and one of which is *retarded* behind the other by an interval b. Thus the expression will be

$$\begin{aligned} \eta &= \Sigma\, [\alpha \sin (n\,t - k\,x)] \\ \zeta &= \Sigma\, [\beta \sin (n\,t - k\,x - b)], \end{aligned} \qquad (6.)$$

in which, taking a single term, and substituting, we readily find the equation to the ellipse described by a vibrating molecule, the origin being at the centre, the conjugate axes parallel to the coordinate axes of y and z, and their values being

$$\alpha \sin b = \tfrac{1}{2} \text{ axis} \qquad \beta \sin b = \tfrac{1}{2} \text{ conj. axis} \qquad (7.)$$

---

* See L. & E. Phil. Mag. and Journal of Science, vol. xii. No. 74, p. 259.

This constitutes elliptically polarized light: if $\alpha = \beta$, it is circularly polarized.

The retardation b is constant for the whole ray; and if we suppose that for all the ellipses the values of α and of β respectively are equal, these quantities will become constant coefficients in the summation, and we may write

$$\Sigma\,\alpha = h\,\alpha \qquad \Sigma\,\beta = h\,\beta \tag{8.}$$

And the formulas (6.) will become

$$\begin{aligned} \eta &= \alpha\,\Sigma\,(\sin(n\,t - k\,x)) \\ \zeta &= \beta\,\Sigma\,(\sin(n\,t - k\,x - b)). \end{aligned} \tag{9.}$$

(4.) In order to obtain the equations which express the motions of a system of molecules, connected by their attractive and repulsive forces so as to form an elastic medium, (which is the idea originally pursued by M. Navier and since by M. Cauchy and others,) I will suppose the same method followed as in all these investigations, and which it will not be necessary here to repeat; these equations in their general form will be found in several papers in this Journal, as in my abstract of M. Cauchy[*] (though in a different form); in Mr. Tovey's paper[†]; or again in my paper in the Phil. Trans. 1838, part ii. eq. (13.), and in Mr. Kelland's memoir, Camb. Trans., vol. vi. p. 158.

When we adopt the supposition of the ray coinciding with x, as above, the equations in question are

$$\frac{d^2\eta^2}{d\,t^2} = m\,\Sigma\,[\phi\,(r)\,\Delta\eta + \psi\,(r)\,\Delta y\,(\Delta y\,\Delta\eta + \Delta z\,\Delta\zeta)] \tag{10.}$$

$$\frac{d^2\zeta}{d\,t^2} = m\,\Sigma\,[\phi\,(r)\,\Delta\zeta + \psi\,(r)\,\Delta z\,(\Delta z\,\Delta\zeta + \Delta y\,\Delta\eta)], \tag{11.}$$

where m is the unit of force, r the distance from the molecule first agitated, $\Delta y\;\Delta z$ the differences of coordinates of the molecules from the first, $\Delta\eta\;\Delta\zeta$ the corresponding differences of the displacements.

It is also material to observe, that these equations have been obtained *without any particular supposition being made as to the arrangement of the molecules in space: they consequently apply if we imagine the molecules distributed in the most irregular or unsymmetrical manner.*

(5.) If we consider the two component displacements $\eta\;\zeta$ which enter the above equation as related in the way expressed by the formula (6.),

$$\begin{aligned} \eta &= \Sigma\,[\alpha\sin(n\,t - k\,x)] \\ \zeta &= \Sigma\,[\beta\sin(n\,t - k\,x - b)] \end{aligned}$$

(in which, if $\alpha\,\beta$ and b are assumed as before explained, we can express the varieties of elliptically or circularly polarized light,) it is easy to show this formula is the solution of the differential equations in the form just given (10.) (11.), if n have a certain value, which we proceed to determine.

---

[*] L. & E. Phil. Mag. and Journal of Science, vol. vi. No. 31, p. 25.

[†] Ibid. vol. viii. No. 43.

Taking the increments of these expressions, we have

$$\Delta\eta = \Sigma \left\{ \begin{array}{l} \Big[-2 \sin^2 \dfrac{k \Delta x}{2} \sin (n t - k x) \\ \quad - \sin k \Delta x \cos (n t - k x) \Big] \end{array} \right\} \qquad (12.)$$

$$\Delta\zeta = \left\{ \begin{array}{l} \Sigma \Big\{ \beta \cos b \Big[-2 \sin^2 \dfrac{k \Delta x}{2} \sin (n t - k x) \\ \qquad\qquad - \sin k \Delta x \cos (n t - k x) \Big] \Big\} \\ + \Sigma \Big\{ \beta \sin b \Big[\quad \sin k \Delta x \sin (n t - k x) \\ \qquad\qquad - 2 \sin^2 \dfrac{k \Delta x}{2} \cos (n t - k x) \Big] \Big\}. \end{array} \right\} \qquad (13.)$$

Also differentiating them, we find

$$\frac{d^2 \eta}{d t^2} = - n^2 \Sigma \, \alpha \sin (n t - k x) \qquad (14.)$$

$$\frac{d^2 \zeta}{d t^2} = - n^2 \Sigma \{ \beta \cos b \sin (n t - k x) - \beta \sin b \cos (n t - k x) \} \qquad (15.)$$

Now for brevity writing

$$\left. \begin{array}{l} p \ \ = \phi (r) + \psi (r) \Delta y^2 \\ p' \ = \phi (r) + \psi (r) \Delta z^2 \\ q \ \ = \psi (r) \ \Delta y \, \Delta z \\ 2 \theta = k \Delta x \end{array} \right\} \qquad (16.)$$

the equations (10.) and (11.) will be expressed by

$$\frac{d^2 \eta}{d t^2} = m \left[\Sigma (p \Delta \eta) + \Sigma (q \Delta \zeta) \right] \qquad (17.)$$

$$\frac{d^2 \zeta}{d t^2} = m \left[\Sigma (p' \Delta \zeta) + \Sigma (q \Delta \eta) \right]. \qquad (18.)$$

And here substituting the above values of $\Delta \eta \, \Delta \zeta$, and arranging the terms, these equations become

$$\frac{d^2 \eta}{d t^2} = m \left\{ \begin{array}{l} \left. \begin{array}{l} + \sin b \, \Sigma [\beta q \quad \sin 2 \theta] \\ - \cos b \, \Sigma [\beta q \, 2 \sin^2 . \theta] \\ - \qquad\quad \Sigma [\alpha p \, 2 \sin^2 . \theta] \end{array} \right\} \sin (n t - k x) \\ \left. \begin{array}{l} - \qquad\quad \Sigma [\alpha p \quad \sin 2 \theta] \\ - \cos b \, \Sigma [\beta q \quad \sin 2 \theta] \\ - \sin b \, \Sigma [\beta q \, 2 \sin^2 \ \theta] \end{array} \right\} \cos (n t - k x) \end{array} \right. \qquad (19.)$$

$$\frac{d^2 \zeta}{d t^2} = m \left\{ \begin{array}{l} \left. \begin{array}{lll} + \sin b & \Sigma & [\beta p' \ \sin 2\theta] \\ - \cos b & \Sigma & [\beta p' \ 2\sin^2\theta] \\ - & \Sigma & [\alpha q \ 2\sin^2\theta] \end{array} \right\} \sin (n t - k x) \\ \left. \begin{array}{lll} - & \Sigma & [\alpha q \ \sin 2\theta] \\ - \cos b & \Sigma & [\beta p' \ \sin 2\theta] \\ - \sin b & \Sigma & [\beta p' \ 2\sin^2\theta] \end{array} \right\} \cos (n t - k x). \end{array} \right. \qquad (20.)$$

On comparing these expressions with those for the same functions (14.) (15.), which must be identical, and equating the respective coefficients of sin $(n t - k x)$ and of cos $(n t - k x)$, since they must hold good for all values of those terms, we have the following equations:

$$-n^2 \Sigma \alpha = m \left\{ \begin{array}{lll} + \sin b & \Sigma & [\beta q \ \sin 2\theta] \\ - \cos b & \Sigma & [\beta q \ 2\sin^2\theta] \\ - & \Sigma & [\alpha p \ 2\sin^2\theta] \end{array} \right. \qquad (21.)$$

$$0 = m \left\{ \begin{array}{lll} - & \Sigma & [\alpha p \ \sin 2\theta] \\ - \cos b & \Sigma & [\beta q \ \sin 2\theta] \\ - \sin b & \Sigma & [\beta q \ 2\sin^2\theta] \end{array} \right. \qquad (22.)$$

$$-n^2 \cos b \, \Sigma \beta = m \left\{ \begin{array}{lll} + \sin b & \Sigma & [\beta p' \sin 2\theta] \\ - \cos b & \Sigma & [\beta p' \, 2\sin^2\theta] \\ - & \Sigma & [\alpha q \, 2\sin^2\theta] \end{array} \right. \qquad (23.)$$

$$-n^2 \sin b \, \Sigma \beta = m \left\{ \begin{array}{lll} - & \Sigma & [\alpha q \ \sin 2\theta] \\ - \cos b & \Sigma & [\beta p' \ \sin 2\theta] \\ - \sin b & \Sigma & [\beta p' \, 2\sin^2\theta] \end{array} \right. \qquad (24.)$$

From the two last forms (23.) (24.), by multiplication and addition, we obtain

$$-n^2 \Sigma \beta = m \left\{ \begin{array}{lll} - & \Sigma & [\beta p' \ 2\sin^2\theta] \\ - \cos b & \Sigma & [\alpha q \ 2\sin^2\theta] \\ - \sin b & \Sigma & [\alpha q \ \ \sin^2\theta] \end{array} \right. \qquad (25.)$$

In the case of elliptic polarization from the conditions before stated (7, 8, 9), we can obtain from the forms (21.) and (25.),

$$n^2 = \frac{m}{h(\alpha^2 + \beta^2)} \left\{ \begin{array}{lll} \ \ \beta^2 & \Sigma & [p' \ 2\sin^2\theta] \\ + \alpha^2 & \Sigma & [p \ \ 2\sin^2\theta] \\ + 2\cos b \, \alpha\beta & \Sigma & [q \ \ 2\sin^2\theta] \end{array} \right. \qquad (26.)$$

Also the form (24.) gives, on transposing,

$$\sin b = \frac{m \left[\alpha \Sigma (q \sin 2\theta) + \beta \cos b \, \Sigma (p' \sin 2\theta) \right]}{n^2 h \beta - m \beta \Sigma (p' \, 2\sin^2\theta]} \qquad (27.)$$

Upon the whole, then, we see that the formula (9.) for elliptically polarized light, involving the above value of n, is the solution of the differential equations (10.) (11.) for the motion of a system of molecules constituted as at first supposed.

It is important to bear in mind that this solution has been obtained solely from the conditions of elliptic polarization, the original equation *being in the forms* (10.) (11.) *retaining all its terms.*

(6.) If instead of the formula (9.) we had taken the expressions for the component displacements η ζ, as not involving such a relation, but simply as in the original expressions (1.), or supposing $b = 0$ in the forms (6.), we might still pursue steps analogous to those above exhibited, though with different values. To trace these results we have only to alter these formulas agreeably to the new conditions. Thus, for unpolarized light, on making $\sin b = 0$ $\cos b = 1$, we have

$$\text{from (21.)} \quad 0 = \left\{ \begin{array}{l} \left\{ \begin{array}{l} n^2 \Sigma\, \alpha \\ -m\, \Sigma\, [\alpha\, p\, 2 \sin^2 \theta] \end{array} \right\} \\ -m\, \Sigma\, [\beta\, q\, 2 \sin^2 \theta] \end{array} \right. \qquad (28.)$$

$$\text{from (22.)} \quad 0 = \left\{ \begin{array}{l} -\ \Sigma\, [\alpha\, p \sin 2\, \theta] \\ -\ \Sigma\, [\beta\, q \sin 2\, \theta] \end{array} \right. \qquad (29.)$$

$$\text{from (24.)} \quad 0 = \left\{ \begin{array}{l} -\ \Sigma\, [\beta\, p' \sin 2\, \theta] \\ -\ \Sigma\, [\alpha\, q \sin 2\, \theta] \end{array} \right. \qquad (30.)$$

$$\text{from (23.)} \quad 0 = \left\{ \begin{array}{l} \left\{ \begin{array}{l} n^2 \Sigma\, \beta \\ -m\, \Sigma\, [\beta\, p'\, 2 \sin^2 \theta] \end{array} \right\} \\ -m\, \Sigma\, [\alpha\, q\, 2 \sin^2 \theta] \end{array} \right. \qquad (31.)$$

Now in all these equations it is evident that since α and β are by the original condition *wholly arbitrary and independent* both of each other and of the other quantities, these equations can only hold good for *all values whatever*, of α and β, if each of the terms involving respectively α and β are *separately* $= 0$, that is, we must have

$$\text{from (29.)} \left\{ \begin{array}{l} 0 = \Sigma\, [\alpha\, p \sin 2\, \theta] \qquad (32.) \\ 0 = \Sigma\, [\beta\, q \sin 2\, \theta] \qquad (33.) \end{array} \right.$$

$$\text{from (30.)} \left\{ \begin{array}{l} 0 = \Sigma\, [\beta\, p' \sin 2\, \theta] \qquad (34.) \\ 0 = \Sigma\, [\alpha\, q \sin 2\, \theta] \qquad (35.) \end{array} \right.$$

$$\text{from (28.)} \quad 0 = \Sigma\, [\beta\, q\, 2 \sin^2 \theta] \qquad (36.)$$

$$\text{from (31.)} \quad 0 = \Sigma\, [\alpha\, q\, 2 \sin^2 \theta] \qquad (37.)$$

$$\text{from (28.)} \quad 0 = n^2\, \Sigma\, \alpha - m\, \Sigma\, [\alpha\, p\, 2 \sin^2 \theta] \qquad (38.)$$

$$\text{from (31.)} \quad 0 = n^2\, \Sigma\, \beta - m\, \Sigma\, [\beta\, p'\, 2 \sin^2 \theta]. \qquad (39.)$$

Hence the formula for unpolarized light is *only* a solution, provided those conditions are fulfilled in the original equations.

From the last two forms (38.) (39.) we have

$$n^2 = \frac{m}{\Sigma\,\alpha}\,\Sigma\,[\alpha p\; 2\sin^2\theta]. \qquad (40.)$$

$$n^2 = \frac{m}{\Sigma\,\beta}\,\Sigma\,[\beta p'\; 2\sin^2\theta]. \qquad (41.)$$

For plane polarized light from the conditions (5.) let $\beta = 0$, and the form (22.) will become

$$0 = \Sigma\,[\alpha\, p \sin 2\,\theta]. \qquad (42.)$$

In like manner (23.) will give

$$0 = \Sigma\,[\alpha\, q\; 2\sin^2\theta] \qquad (43.)$$

and (24.) $$0 = \Sigma\,[\alpha\, q \sin 2\,\theta]\,; \qquad (44.)$$

while from (21.) we find

$$n^2 = \frac{m}{\Sigma\,\alpha}\,\Sigma\,[\alpha\, p\; 2\sin^2\theta]. \qquad (45.)$$

Hence the formula for plane polarized light is *only* a solution, provided these conditions are fulfilled in the original equations.

Since $\Delta\,\eta$ and $\Delta\,\zeta$ are both of the form

$$\mathrm{M} \sin 2\,\theta \pm \mathrm{N}\; 2\sin^2\theta,$$

the above conditions give

$$\Sigma\,(q\,\Delta\,\eta) = 0 \qquad (46.)$$

$$\Sigma\,(q\,\Delta\,\zeta) = 0. \qquad (47.)$$

Thus when they are fulfilled in the terms of the original equations, those equations become, for unpolarized light,

$$\frac{d^2\eta}{d\,t^2} = \Sigma\,[p\,\Delta\,\eta] \qquad (48.)$$

$$\frac{d^2\zeta}{d\,t^2} = \Sigma\,[p'\Delta\,\zeta], \qquad (49.)$$

while for plane polarized light the second of these forms disappears.

(7.) Now recurring to the supposed constitution of the medium, to examine the conditions under which these terms can vanish, we may first observe, that since none of the factors can separately become $= 0$, the terms can only become nothing by sums with opposite signs being equal and destroying each other.

That this may happen depends on an hypothesis respecting the arrangement of the *ætherial molecules* in spaces, viz. that they are *distributed uniformly.* This is the supposition adopted by M. Cauchy and other writers.

On this hypothesis the axis x passing through the first molecule m in any direction, the sums of the corresponding distances of all the other molecules on each side of it, whether in the plane of y or z, will be equal for all positions of x in the medium.

It is easily seen that the respective sums of products

$$\phi\, r \sin 2\,\theta \qquad \psi\, r\, \Delta\, y^2 \sin 2\,\theta \qquad \psi\, r\, \Delta\, z^2 \sin 2\,\theta$$
$$\psi\, r\, \Delta\, y\, \Delta\, z \sin 2\,\theta$$

with opposite signs will be equal. Thus we shall always have

$$\left.\begin{array}{l} \Sigma\, [\alpha\, q \sin 2\,\theta] = 0 \\ \Sigma\, [\alpha\, p \sin 2\,\theta] = 0 \\ \Sigma\, [\alpha\, p' \sin 2\,\theta] = 0 \end{array}\right\}. \qquad (50.)$$

Whenever these terms are evanescent, it is easy to show that we always have also

$$\Sigma\, [\alpha\, q\, 2 \sin^2 \theta] = 0\,; \qquad (51.)$$

and similarly for the like terms involving β, by a simple transformation of coordinates, as explained by Sir J. Lubbock in his valuable paper *. That paper indeed relates to the more general views of the subject, to which I shall refer in the sequel; but the particular process in question is independent of these views.

Thus the hypothesis of symmetrical distribution gives

$$\Sigma\, [q\, \Delta\, \eta] = 0 \qquad \Sigma\, [q\, \Delta\, \zeta] = 0 \qquad (52.)$$

and the original equations are reduced to

$$\frac{d^2 \eta}{d t^2} = \Sigma\, [p\, \Delta\, \eta] \qquad (53.)$$

$$\frac{d^2 \zeta}{d t^2} = \Sigma\, [p'\, \Delta\, \zeta]\,; \qquad (54.)$$

or restoring the original values,

$$\frac{d^2 \eta}{d t^2} = \Sigma\, [(\phi\,(r) + \psi\,(r)\, \Delta\, y^2)\, \Delta\, \eta] \qquad (55.)$$

$$\frac{d^2 \zeta}{d t^2} = \Sigma\, [(\phi\,(r) + \psi\,(r)\, \Delta\, z^2)\, \Delta\, \zeta]. \qquad (56.)$$

That is, the equations are reduced to precisely the same form by the hypothesis of uniform distribution, as they are on the hypothesis of unsymmetrical distribution, by the conditions of plane polarized and unpolarized light.

Thus the formulas for *plane polarized and unpolarized* light are *only* solutions of the original equations when in the same form, to which they are reduced by *symmetrical distribution.*

---

* L. & E. Phil. Mag. and Journal of Science, vol. xv. November, 1839.

For elliptically polarized light, on the hypothesis of *symmetrical* distribution, we can follow out results analogous to those above obtained. We should have instead of the form (21.),

$$n^2 h = m \Sigma [p \, 2 \sin^2 \theta]; \qquad (57.)$$

and instead of (25.),

$$n^2 h = m \Sigma [p' \, 2 \sin^2 \theta], \qquad (58.)$$

which is identical with the former, whence we have $p = p'$; also,

$$n^2 = \frac{m}{h} \Sigma [p \, 2 \sin^2 \theta]. \qquad (59.)$$

The formula (27.) is thus reduced to

$$\sin b - \tfrac{0}{0} \qquad (60.)$$

Thus (although with altered values) the formula for *elliptic polarization* is a solution of the original equation *equally* in the form (10.) (11.), and when reduced to the form (55.) (56.) by the hypothesis of *symmetrical distribution*. In other words, of the equations, in the form (10.) (11.), the formula for elliptic polarization is the only solution: in the form (55.) (56.) the formulas for elliptic vibrations or rectilinear indifferently are solutions.

(8.) Thus it follows, that if we suppose the ætherial molecules *unsymmetrically* distributed, then *elliptic polarization alone* is the result. *Æther so constituted cannot admit rectilinear vibrations.* Light, therefore, entering such a portion of æther necessarily becomes elliptically polarized.

If we suppose the molecules symmetrically distributed, this is compatible with either elliptic or rectilinear vibrations indifferently. Either therefore will be propagated according to the condition of the intromitted ray.

Thus elliptic polarization is traced to its cause in the simple consideration that the vibrations which constitute it are necessarily produced when waves are propagated through any portion of æther in which a symmetrical arrangement of the molecules does not subsist.

The investigation conducted by Mr. Tovey's method*, is directed to showing by the equations (4.) of his paper, that when the sums involving the odd powers of the differences are *not* evanescent, the quantities b and ρ (the ratio of the semiaxes of the ellipse) are *determinate*; or, in other words, the expressions must belong to ellipses, or in a medium so constituted as to make those sums finite, elliptical polarization will result. When the sums just mentioned vanish, then it is seen that those quantities are altogether arbitrary, and the movements will be the same as those expressed by the author's formulas in his other paper [*]; or such a medium will propagate elliptic or rectilinear vibrations indifferently[†].

---

* L. & E. Phil. Mag. and Journal of Science, vol. xii. p. 10.

(9.) Mr. Whewell [‡] had observed the difficulty of conceiving any mechanical conditions for the production of elliptic polarization, and that not even a plausible hypothesis had been proposed so as to give a physical interpretation to the language of analysis, especially as conveyed in the equations obtained by Prof. Maccullagh.

From what has just been stated, this difficulty appears now to be, at least in a general way, overcome by the conclusion of Mr. Tovey.

It is easy to conceive the physical possibility of a portion of the æther possessing an unsymmetrical arrangement of its molecules. For example, at the bounding surface of a medium and of vacuum, or generally of two media of different densities, we can hardly suppose the change of density in the æther to take place abruptly; but must from all analogy imagine a thin stratum on either side, within which there is a gradual alteration in the arrangement of the molecules; and this more considerable as the difference of the refractive powers is greater. It is conceivable that this variation may in some instances be of sufficiently great amount to give the requisite conditions of unsymmetrical distribution within this stratum, though on either side of it the symmetrical arrangement may subsist.

(10.) With regard to the value of n, experiment shows that the state of the same ray as to polarization produces no difference in the magnitude of its refractive index. Hence it follows that in all the preceding different cases in which the value of n has been expressed (whether on the hypothesis of unsymmetrical or of symmetrical distribution) the terms involved must vary in magnitude, so that the whole expression shall remain constant, or the values (26. 40. 41. 45. 59.) all equal. Thus, in general, writing h for the sum of the arbitrary terms α and β, we express the value of n by the formula

$$n^2 = \frac{m}{h} \Sigma \left[\alpha p \, 2 \sin^2 \theta\right]. \qquad (61.)$$

---

[*] L. & E. Phil. Mag. and Journal of Science, vol. viii. p. 426 and p. 502.

[†] See also the same author's paper on the nature of the vibrations in quartz, vol. xiv. 1839, pp. 169 and 323.

[‡] Hist. of Ind. Science, vol. ii. p. 448: 1837.

If we return to the values (2.) (3.),

$$k = \frac{2\pi}{\lambda} \qquad \theta = \frac{\pi\,\Delta x}{\lambda}, \tag{62.}$$

we have obviously

$$\frac{1}{\mu^2} = \frac{n^2}{\left(\frac{2\pi}{\lambda}\right)^2} = \frac{1}{4}\,\frac{n^2\,\Delta x^2}{\left(\frac{\pi\,\Delta x}{\lambda}\right)^2} = \frac{1}{4}\,\frac{n^2\,\Delta x^2}{\theta^2}; \tag{63.}$$

and on substituting in the form (61.) we obtain

$$\frac{1}{\mu^2} = \frac{m}{2h}\,\Sigma\left\{\alpha\,(\phi\,(r) + \psi(r)\,\Delta y^2)\,\Delta x^2 \left(\frac{\sin^2\left(\frac{\pi\,\Delta x}{\lambda}\right)}{\left(\frac{\pi\,\Delta x}{\lambda}\right)^2}\right)\right\} \tag{64.}$$

which for abridgement may be expressed by

$$\frac{1}{\mu^2} = \Sigma\left\{\mathrm{H}^2\,\frac{\sin^2\left(\frac{\pi\,\Delta x}{\lambda}\right)}{\left(\frac{\pi\,\Delta x}{\lambda}\right)^2}\right\} \tag{65.}$$

the formula for the dispersion.

(11.) I have here confined my remarks strictly to the illustration of the one primary question of the criterion of elliptic polarization; but in all that has been said relative to the evanescence of the terms on the hypothesis of symmetrical arrangement, by the transference to new axes, &c., I have touched upon the far more extensive relations between our immediate subject and that of the axes of elasticity and the wave-surface: some remarks on these must be reserved to a future communication; meanwhile I will venture to hope that the particular subject of the preceding observations has now been placed in such a light as to free it from the ambiguity and doubt in which it seems to have been involved.

XXXIII. *On Lightning Conductors, and on Experiments relating to the Defence of Shipping from Lightning.* By W. Snow Harris, *Esq., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

YOU will greatly oblige me, by allowing the following communication to appear in the pages of your Journal, being the concluding observations I have to offer on Mr. Sturgeon's

Memoir on Marine Lightning Conductors, to which I have alluded in former papers *.

Had the question been one of private, rather than of public interest, I should certainly have followed the course pursued by others, who have been honoured with Mr. Sturgeon's censure, and have shown by my silence how little I care to deprive him of any advantage which he may consider he derives by his coarse treatment of me. Such, however, is not the case: my method of defending shipping from lightning by permanent conductors of electricity fixed in their masts, being still carrying out in several of H.M. ships, I feel myself called on to afford a clear elucidation of certain circumstances intimately associated with this highly important question, which have been lately much misrepresented; moreover, the subject is one of increasing interest, and intimately connected with an important department of physical science.

In the year 1780, the French began to turn their attention to the more effectual defence of their buildings and shipping from the effects of lightning.

Mons. Le Roi was sent to visit Brest and the various seaports of France for that purpose. His memoir in the *Histoire de l'Académie des Sciences* for the year 1790, will be read with advantage by every one interested in the subject.

Impressed with the necessity of adopting some more permanent security than was derived from the use of chains, temporarily applied in the rigging, he endeavoured to place, he says, such conductors in ships as might be deemed "fixed and durable." With this view long linked rods of small dimensions were led from a point at the mast-head along a part of the rigging, and fixed to plates of metal at the ship's side communicating with the sea. These, however, he admits did not stand the working of the rigging, and were broken in a storm; so that he was induced to change them for similar linked rods of small size led in divided stages along the masts.

This form of conductor was fixed in the French ships of war Etoile, Resolution, Experiment, Boussole, Astrolabe, and in some ships destined for America. Although Mons. Le Roi had reason to believe that it was sufficiently secure from the effects of mechanical violence, yet it was eventually abandoned in the French service, probably from its still being found inconvenient and ill calculated to meet the violent forces incidental to a ship's mast and rigging. The only kind of conductor ever generally used by the English, consists of links of small copper rods under the form of a flexible chain triced up, as occasion required, to the mast-head and allowed to touch the sea, a contrivance recommended by the learned Dr. Watson in the year 1762, in a letter to Lord Anson, then First Lord of the Admiralty.

---

* L. & E. Phil. Mag., December 1839, February 1840, May 1840.

The damage, however, which, notwithstanding every suggestion of the kind, continued to occur to shipping from atmospheric electricity, and especially to our navy, showed that such methods had not fully met all the circumstances of the case; hence the necessity of resorting to some system of defence, differing essentially from those hitherto tried. Now that system which experience indicated evidently embraced the following principles.

1. The conductor should be permanently fixed, so as to be always in place, and at all times ready to meet the most unexpected danger.

2. It should be continuous and of considerable electrical capacity, so as to admit of a rapid equalization of the electrical action.

3. It should in no way interfere with the standing or running rigging; should admit of the motion of the sliding masts without deranging its existing state; and in case of either being removed by accident or design, the remaining portion of the conductor should be still as efficient as before.

4. It should be independent of the crew of the ship, and not be left to them to put in place or not, as may be deemed requisite; or require any handling, to their great annoyance, —under many circumstances to their imminent peril.

5. It should be so applied that a discharge of lightning falling on the vessel, could not enter into any circuit of which it did not form a part.

The desirableness of such a plan as that which in the year 1820 I proposed to the Admiralty, embracing, as it did, every required condition, was admitted. The doubts, however, existing at the time were, whether it was fully borne out in all its details by a sufficient number of facts in science; and whether it could be carried out so perfectly as to meet the many variable circumstances in which the general fabric in all its casualties might become placed; but no doubt existed of the propriety of adopting such a form of conductor, were these questions satisfactorily disposed of.

Your readers are no doubt aware, that the method I proposed, was to make the masts themselves perfect conductors of electricity, by incorporating with them in a peculiar way two laminæ of copper sheet of considerable thickness and extension; uniting them with the metallic masses in the hull by other laminæ, and giving the whole a perfectly free communication with the sea in all directions. The reasonableness of this as an abstract question in electricity is quite evident; since all the damage found to occur on ship-board from lightning arises principally from the effects of expansion, where the electrical discharge passes over or through bad conducting matter. If we could conceive for an instant that the vessel, sails, rigging, &c. were metallic throughout, the possibility of its receiving any damage from lightning would be inconceivable.

This, in fact, has been shown by the perfect way in which the iron steam-vessels in Lander's last voyage met discharges of lightning whilst the timber vessels were damaged. The nearer, therefore, we can approximate to this condition, the more perfect the security.

I satisfied the Navy Board, together with many eminent scientific men of that day, of the completeness of my views, and of their practical application to the variable conditions of a ship's mast; and the celebrated Dr. Wollaston, in a letter to the Comptroller of the Navy, gave my plan his "decided approval." Under these circumstances it was at last ordered to be carried out in ten of H.M. ships, including line-of-battle ships, frigates and corvettes; a small brig of 10 guns was subsequently added to the list; and thus the conductors were tried in the navy from the largest to nearly the smallest vessel in it. After a period of ten years, during which time these vessels had been exposed to several storms of lightning in all parts of the world, the Lords Commissioners of the Admiralty appointed, under the countenance of the House of Commons, a commission of nautical and scientific men to investigate and report on this momentous topic. The members of the commission were all named without any reference to myself, and were all men of unquestionable integrity and ability. When the names of Admiral Griffith, Sir James Gordon, K.C.B., Captain J. C. Ross, Professor Daniell, Mr. Fincham, and Mr. Clifton of the Admiralty, are mentioned, no one of the least character in society would question the fairness of any investigation placed under their direction.

The Commission, in carrying out this measure, called in evidence the officers who had commanded the ships in which my conductors were tried, and obtained valuable information from various others who had witnessed the effects of lightning at sea. They also invited the opinions of men acknowledged to be the best acquainted with this department of science; and they examined various other forms of lightning conductors for ships. But upon the whole evidence, they conclude with an earnest recommendation to the Board to carry out the plan fully in the Royal Navy*.

No sooner had this report been laid on the table of the House of Commons, than several persons interested in superseding my plan, endeavoured in various ways, notwithstanding its ten years' trial, to get it set aside; hence arose what Mr. Sturgeon calls Experimental and Theoretical Researches in Electricity, Fourth Memoir on Lightning Conductors; especially addressed to the British Association, and to all learned societies in Europe and America,—a list of which is ostentatiously displayed at the head of the memoir.

Mr. Sturgeon's production having been thus paraded forth, we should of course expect to find it contain new and important facts in science, commensurate with so loud an announcement, and with its pretensions, to arrest the attention of all the various learned bodies above mentioned, and as he states in another place, "the ablest electricians which the world can produce." After so great an advancing shadow, we should naturally expect something like an adequate substance.

The following brief outline of the memoir is sufficient to show how little such expectations are realized.

The first five pages, consist of unhandsome insinuations of a want of honesty and ability on the part of the members of the Commission, of pretensions on his own part to put "the question in a proper light," and of deceptive statements of the nature and objects of my experiments, called by him an

---

* "Having now completed our remarks on the several points to which their Lordships' instructions directed our attention, we trust we have shown, from the evidence of facts derived from the experience of many years, as well as by the opinions, not only of scientific but professional men, the efficacy of Mr. Harris's lightning conductors; and considering the number of lives which have been lost by lightning, the immense amount of property which has been destroyed, as shown by Mr. Harris, and is still exposed without adequate protection, the inconvenience which has arisen, and is still liable to arise from the loss of the services of ships at moments of great critical importance, the *difficulty of procuring new spars in times of war on foreign stations* (not to mention the great expense of wages and victuals for the crews of ships while rendered useless till repaired),—we again beg to state our *unanimous opinion* of the great advantages possessed by Mr. Harris's conductors *above every other plan,* affording permanent security at all times, and under all circumstances, against the injurious effects of lightning, effecting this protection without any nautical inconvenience or scientific objection whatever; and we therefore most earnestly recommend their general adoption in the Royal Navy."

"examination." The next eight pages consist of notices of the damage done to the Rodney, copied from the newspapers of the day, of a few extracts from the statements made by naval officers who had witnessed the protecting effects of my conductors, and of a few desultory remarks on those statements; here then are thirteen pages to begin with, containing no kind of scientific research whatever. The next three pages and a half contain a few ill-contrived repetitions of Priestley and Cavallo's experiments, made above fifty years since, on the residual electricity of a Leyden jar, and which Mr. Sturgeon, by a most unhappy blunder, has taken as a lateral discharge produced at the instant of the primary discharge *. "This residual electricity (observes Cavallo) should be carefully considered in performing delicate experiments †".

The remaining 15 pages contain merely ill-digested remarks, resting on the above fallacy; designing appeals to the fears of the uninformed; an account of a few common-place and inevitable results of ordinary electrical action on a kite; and a proposal to place copper rods in various ways about the rigging of a ship as a defence against lightning, in open defiance of his own previous admissions; with a few considerations of the expense attendant on it.

How such a paper as this can fairly come under the denomination of "Theoretical and Experimental Researches in Electricity," when not above two out of thirty-two pages, or about one sixteenth of the whole, contain any experiment at all,—the experiments not original, and the reasoning upon them a fallacy,—I am really at a loss to determine.

In my former communications, which appeared in this Journal in December, 1839, and February and May, 1840, I have given a complete exposition of these and other points contained in this memoir; so that but little more remains to notice than the deceptive colouring in which Mr. Sturgeon has thought fit to disguise my experiments, and his illiberal dealing with every one holding opinions adverse to himself.

The simplicity and convenience of my fixed conductors having, as I have already stated, been in the year 1820 fully admitted, the Navy Board at that time thought proper to call upon me to illustrate and investigate experimentally, so far as possible, their probable operation through the ship.

---

* Lond. and Edinb. Phil. Mag. for December, 1839.

† Mr. Sturgeon appears to be well aware of the great mistake he has made on this point. In no place, so far as I know, has he attempted to defend it.

I was called upon, for instance, to show what had been the general laws of the action of lightning on ship-board; that the connexion of my conductors with the sea through the metallic masses in the hull was in no way detrimental to their action, or liable to objection; that, in short, electrical discharges might find their way and become as safely dispersed in the sea by the metallic bolts driven through the keel and keelson, and other parts of the vessel, as by a chain in the rigging.

I was further called upon to show, so far as possible, how a system, such as I proposed, would operate in this way under various positions of the masts, and that the circumstance of their motion upon each other would in no way interfere with the action of the conductor. In order to meet the views of the officers of the Board, I naturally enough resorted to such practical illustrations as were within my reach. By way of showing the operation of my conductors through the bolts in the hull, strong charges from twenty-five square feet of coated glass were passed over a vessel's mast fitted with the conductors and floating in the sea, the negative side of the battery being connected with an interrupted circuit passing over a small gun placed in a boat (in some instances) forty fathoms from the vessel. Percussion powder was placed over the joints of the conductor on the mast, and in an interrupted circuit at the mast head. The sliding masts were placed in various positions, and occasionally were put in motion at the time of the experiment; but in all cases at the instant of completing the circuit, the whole charge, equivalent to fuse fifteen feet of small iron wire, passed freely through the conductor and the sea in all positions of the masts without igniting the powder placed over the joints, but firing the powder at the mast-head, and the gun, instantaneously, thereby showing that the charge had reached from the mast head to the water.

Any one will, I think, perceive that these experiments illustrated, so far as they went, the points in question, viz.

1st. The operation of the conductor through the hull.

2nd. Its perfect continuity on the masts.

3rd. Its complete operation under every possible position, &c. of the masts.

4th. The reception of the charge by the sea.

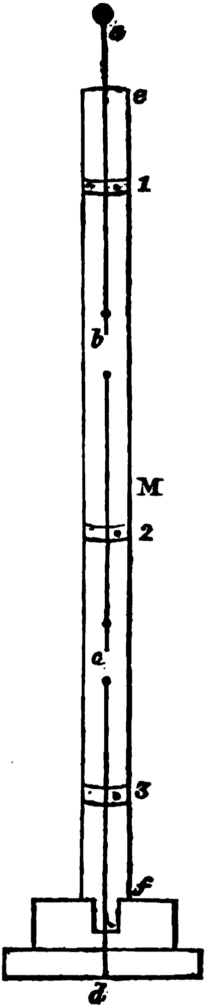
Now Mr. Sturgeon fairly shuts his eyes to the unpretending nature and object of these experiments, and perverting their meaning, tells his readers that they prove nothing peculiar to my system, and serve only to show that copper is a conductor of electricity, and that detonating powder can be ignited by the electrical spark. This he calls a *fair* and *candid explanation* of my experiments before the Navy Board at Plymouth.

As he gives every one who in any way treated this question with fairness, a most liberal credit for ignorance, he thinks I ought to have explained to the Board that a *wire could be made red-hot by electricity*, and if then brought into contact with *gunpowder would ignite it*; together with some other truisms of a similar form. Now, however ignorant Mr. Sturgeon may suppose the officers of the Board to have been, they certainly understood the question much better than he appears to do; they entered very completely into its merits, and left no point unexplored; they required of me information relative to the conducting power of different metals; their respective resistance to fusion by electricity; the ratio in which they became heated, either by the same or by different quantities, the relative quantities required to heat wires of different diameters to the same degree, &c. &c.; for the perfect elucidation of which new experiments and apparatus were invented, and the results exhibited in a way not before done.

Among other points to which the Board directed my attention, was the electrical effect of the incorporation of the conductor with the mast and hull, and the certainty of its confining the course of the discharge to certain lines and to the surface of the masts without being productive of any lateral effect upon the masses of the metal, such as iron hoops, bolts, &c. which either entered into the body of the mast, or were otherwise connected with it; points which Mr. Sturgeon, in the happy consciousness of his own superior sagacity, says were either "not known or unaccountably neglected."

My first illustrations were confined to small models about six inches in length, which could be splintered by artificial discharges, and defended by small lines of metallic leaf placed along them, in imitation of the continuous conductor. But being desirous to exemplify how completely the conductor would direct and confine the discharge upon itself, I placed the experiment under new and very delicate circumstances; and as I consider this experiment an important one, as bearing on the theory of lightning conductors, and directly applicable to all the objections made to them on account of lateral discharges, I may, I hope, be excused for giving a particular account of it.

A model of a mast, M, about ten feet in length, was made in parts, and an interrupted line of metal, *a*, *b*, *c*, *d*, placed in the heart of it. Percussion powder, which it is well known inflames with the least spark of electricity, was placed between these interruptions. On its surface M was placed a continuous conductor *e* M *f*, and the interior and exterior lines of metal connected with common points of junction at *a* and *d*. To make the experiment more complete, bands of metallic leaf, &c. were made here and there to surround the mast, together with other metallic bodies, which could enter into the mast itself and touch the metallic line within, as at 1, 2, 3.



An intense electrical accumulation from four jars of five square feet each, highly charged, was now allowed to fall upon a ball at *a*, with a view of discovering (since the electric matter had a choice of two lines) whether it would pass upon the metal within in preference to that without, or upon both, or whether it could in passing down the exterior conductor cause a lateral discharge to enter the mast, or affect the interior by any other lateral result.

Now the extreme facility with which percussion powder becomes inflamed by the most minute spark, rendered it a very severe test of the presence of passing electricity, so that if the least action had taken place upon the metallic bodies adjacent to the conductor, it would have been immediately shown by an explosion within the substance of the model. Almost every one must I think perceive that this was a fair experiment, and as good a one as could have been resorted to under the circumstances of the case. Even Mr. Sturgeon himself must allow that it completely meets the very points he has insisted on, and which he thinks I had so much overlooked. Thus in sect. 202 of his memoir, mistaking the residual electricity of a discharged surface for what he calls a lateral discharge produced by the passing shock, he says, "this kind of lateral discharge will always take place when the vicinal bodies are sufficiently capacious, and *near to the principal conductor which carries the primitive discharge*, or *to any of its metallic appendages*;" and in sects. 198 and 199, he says he can produce lateral discharges *half an inch long*, though the jar be of the capacity of a quart only, &c. &c., and "at a distance of fifty feet from the direct discharge;" and in sect. 200, he admits that a discharge from such a jar would "*imitate a flash of lightning striking a similar conductor on the mast.*"

Here then is an experiment which at once brings to the bar Mr. Sturgeon's own position; and I appeal to any one, if what he has thus advanced has any foundation, whether "an electrical accumulation not merely from a jar of a quart capacity, but from twenty-five square feet of coated glass highly charged, and equivalent to destroy fifteen or twenty feet of small iron wire, should not have ignited the powder and caused the mast to be blown in pieces. Yet such was not the case, and never can be so long as the continuous conductor remains on its surface. When the exterior conductor, however, was removed and a similar charge thrown on the model, then the mast was blown in pieces, proving, that if the discharge had under any form pervaded the interior, this effect would have resulted in the first case.

Now Mr. Sturgeon roundly asserts, that "this experiment happens to have no bearing on the subject *whatever*," notwithstanding that in sect. 200 he actually refers to the passing of electricity over conductors placed in given directions, and says, "that the discharge of a jar of only a quart capacity would imitate a flash of lightning striking a similar conductor on the mast:" this is very one-sided reasoning indeed.

In a subsequent communication he inquires if "I mean to be considered as a philosopher or necromancer by endeavouring to persuade the British Association that blowing asunder two pieces of wood by gunpowder was a true representation of the effects of lightning on a ship's mast."

This may be all very well as the best means Mr. Sturgeon had of parrying the direct bearing of the experiment in question on his own position; he must, however, necessarily feel how immediately it involves all the conditions he has himself pointed out as essential to the exhibition of his imaginary results, by which he pretends to have illustrated and proved the effects of lateral discharges of lightning.

How does it happen, if his views have the slightest foundation, that the heaviest "primitive discharges," to use his own phraseology, can be passed along the exterior conductor of the mast without in any way affecting the detonating powder within, even though connected with the interior metals by short metallic nails? it surely should do so, if what he says in sect. 198 be true, viz. "that by this kind of lateral discharge a dense spark may be produced when the bodies are half an inch apart, though the jar be only of a quart capacity that chemical decompositions are easily performed by it, and every other class of electrical phænomena, exhibited with a sufficiently large apparatus, as decidedly as by the primitive discharge itself." Now the plain truth is, there is no such effect as he describes produced by a passing charge of electricity. It is altogether a fallacy, arising out of his being very ill informed on the subject, and from his having mistaken the common result of the residual accumulation for a lateral explosion, as I think I have fully shown; Cavallo and the old writers have taken great pains to guard the *inexperienced manipulator* against this source of error.

In short, if Mr. Sturgeon will look at Cavallo, Priestley and other writers of above half a century since, he will find his theoretical and experimental researches very *perfectly imitated*; by a most extraordinary coincidence, the experiments are virtually the same; the deductions, however, from them are widely different. With such careless manipulation, and such a great lack of knowledge of his subject, it is not surprising that Mr. Sturgeon should deem others ignorant of it.

These experiments were not the only experiments which were made matters of discussion by the Board. Thus, to show that in the case of the sliding masts being entirely, or partially lowered, that part of the conductor below the caps would be the same in respect of the discharge as if it did not exist, strips of gold-leaf were laid on paper in the same relative positions as the conductors would assume on the masts, and powerful discharges sufficient to disperse the gold passed over them.

These strips were affected in certain parts only, showing how completely the discharge was confined to such parts, and the total absence of all lateral explosion.

Experiments on the expansive effects of electricity on bodies, the fusion and heating of metals, &c. &c. were also entered upon. In short, the series was as complete as could well be desired. The experiments were all original, or otherwise new, of their kind, and many of them embraced points not before considered; such experiments had for the most part never been tried before; and they were considered by Dr. Wollaston and others who investigated them to be of great interest, and to have an important bearing on the question of lightning conductors in ships.

I suppose it will be admitted that Dr. Wollaston had at least as clear a comprehension of this subject as Mr. Sturgeon, and was just as likely to have detected the "necromancy" to which Mr. Sturgeon alludes, had such existed.

This explanation of the practical nature of the experiments, and which, amongst other considerations, led the Navy Board to try my plan for defending ships from lightning in the navy, I have, for the reasons already assigned, thought proper to offer; they at the same time show the scrupulous care which the Board exhibited on the occasion, and how much they were alive to the question.

[To be continued.]

XXXIV. *Remarks on* Prof. Whewell's *Paper on the Mean Level of the Sea. By* Richard Thomas, *Civil Engineer*[*].

I OBSERVE that Mr. Whewell has in the Philosophical Magazine for November honoured with his notice my observations relative to the mean level of the sea, which were inserted in the Magazine for August. I should not have thought it needful to say another word about it, but leave the matter to be determined, as I think it ought to be, by extending the levels westward to the Land's-end, connecting these levels with others which might be taken at convenient places across from the north-west coast to the south-east of Cornwall; but it appears that Mr. Whewell has mistaken the drift of my argument, by which I endeavoured to show, that owing to the tides, the sea at Axmouth might be kept up to a higher mean level than on the coast of Cornwall. I do not draw such inference from the mere narrowing of the English Channel by the projection of Cape la Hogue, but by that projection in combination with the deeply embayed form of the opposite coast of England between the Start and the Bill of Portland, giving such directions to the tide-currents as would continually, both on the flood and ebb, tend to set them over against that part of the coast on which Axmouth is situated, and consequently tend to keep up the level of the water there; for it must be kept in mind that at the time of high water at Axmouth, the tide is strong flood in the Channel, and at low water the ebb tide is strong; and there appears to be strong evidence that the flood sets in the way I state, from the velocity with which it passes along by the back of Portland Island and occasions the "Race" off the Bill.

Mr. Whewell says, "If the surface of low water were a level surface, as Mr. Thomas supposes," &c.; and in reference to a comparison of the tides of Plymouth and Axmouth, that "Mr. Thomas has no authority either from theory or observation for assuming the high waters to coincide," &c. &c. The best answer I can give to these remarks, is a reference to my paper as printed in your Magazine.

However, as Mr. Whewell agrees with me in the propriety of settling the question by extending the levels as above stated, I need not say more, but leave the question to be decided, as I hope it soon will be, by such extension of the levels.

---

[*] Communicated by the Author.

I do not mean to dispute Mr. Whewell's *theory* as to the mean levels of the tides, supposing there were no obstructions to their free action; but I do conceive that it is very possible that circumstances may operate to prevent the mean level of the tides from being at the same elevation at every part of the coast.

My object is to prevent any general conclusion with respect to tide-levels from being acted on until the fact be placed beyond doubt; and whatever may be the result of the inquiry, the levels which may be determined will be available for establishing the elevations of certain fixed points or *bench-marks*, which will always be much more useful for reference than any determination depending on the tides.

Falmouth, Jan. 4, 1841.

XXXV. *Analysis of the Oils of Elemi and Olibanum.*

By John Stenhouse, *Esq.*[*]

MOST of the resins contain, as is well known, volatile oils, to which they owe their peculiar odours. A considerable number of these oils have been carefully examined, but there are several, and among the rest those of elemi and olibanum, which, as far as I am aware, have not previously been subjected to analysis.

I was induced therefore to prepare a quantity of each of them for this purpose.

Elemi Oil.—Preparation. A quantity of pulverized elemi resin was distilled with water in the usual way. The larger portion of the oil came over at an early period of the distillation, and collected on the surface of the water in the receiver, from which it was easily separated by means of a sucker. It was then allowed to remain some days over fused chloride of calcium to render it anhydrous, and afterwards rectified. It is stated in most systems of chemistry, on the authority of Bonastre, that elemi resin yields 12½ per cent. of volatile oil, but though I operated on an apparently fresh and unadulterated specimen, I did not succeed in obtaining more than 3⅛ per cent.

Properties.—Elemi oil is a transparent colourless liquid of an agreeable smell, similar to that of the resin, and of a pungent taste. Its specific gravity is 0·852 at +24° C. It boils at 140° C. It burns with a bright smoky flame, like oil of turpentine. It does not dissolve in water. It is very little soluble in weak spirits, but it dissolves very rapidly in alcohol and æther. Potassium remains unaltered in it, but solid potash, with the assistance of heat, converts it into a brown resin. Iodine acts on it with energy; considerable heat is evolved; part of the iodine is driven off into vapour, and the oil is converted into a thick red magma. Elemi oil absorbs muriatic acid gas very readily, and becomes of a dark-brown colour, but I did not succeed in obtaining an artificial camphor. Nitric acid in the cold quickly changes it to a brownish yellow; when assisted by heat it explodes, evolving deutoxide of azote, and converting the oil into a resinous matter. Sulphuric acid in the cold gives it a fine red colour, but when heated it chars it.

---

[*] Communicated by the Author.

The following are the results of its analysis:

I. 0·4305 gave 1·369 carbonic acid, and 0·453 water.
II. 0·197 gave 0·625 carbonic acid, and 0·208 water.

	Found.		Calculated.
	I.	II.	
Carbon......	87·93	87·72	88·46 = 5 at.
Hydrogen...	11·69	11·73	11·54 = 8 —
	99·62	99·45	100·00

From this it is evident, that elemi oil has a similar constitution with oil of turpentine, oil of lemons, copaiba balsam oil, and several others, too numerous to be mentioned.

Oil of Olibanum.—Olibanum, the frankincense of the ancients, yields an oil which is prepared in the same way as that of elemi. The quantity I could obtain was 4 per cent. of the resin employed; it is a transparent, colourless and very limpid oil. Its smell resembles that of oil of turpentine, but it is much more agreeable. Its specific gravity is 0·886 at + 24° C., and its boiling point is 162°. It burns with a strong bright flame, and emits much smoke. The action of reagents upon it is almost identical with that of the oil of elemi, so that it is unnecessary to enter into details. The following are the results of its analysis:

I. 0·3255 gave 1·0015 carbonic acid, and 0·330 water.
II. 0·3465 gave 1·061 carbonic acid, and 0·351 water,
III. 0·302 gave 0·931 carbonic acid, and 0·307 water.

	Found.			Calculated.
	I.	II.	III.	
Carbon ...	85·07	84·66	85·23	85·61
Hydrogen	11·26	11·25	11·29	11·18
Oxygen...	3·67	4·09	3·48	3·23
	100·00	100·00	100·00	100·00

This gives the formula

Carbon	35	=	2675·225
Hydrogen	56	=	349·426
Oxygen	1	=	100·000
			3124·651.

It is somewhat singular that this is precisely the composition which Dr. Kane found for the oil of *Mentha viridis.*

XXXVI. *Chemical Examination of Palm Oil and Cacao Butter. By* JOHN STENHOUSE, *Esq.**

Palm Oil.

THIS oil has of late years become rather an important article of commerce, from the great extent to which it is employed in the manufacture of soap. It is chiefly imported from the coast of Africa, and is derived, according to some botanists, from the *Cocus Butyracea*, according to others from the *Avoira Elais*. It is extracted by boiling the bruised fruit in water; the oil collects as a cake on the surface, from which it is easily removed. It has a butyraceous consistence, is of a reddish yellow colour, and has an agreeable aromatic odour. When long kept it becomes readily rancid, and whitens at the same time, especially if exposed to the influence of the light and air. Soap made with the unbleached oil is of a yellow colour, but white soap may be made from it if the oil has been previously bleached. This may be done in various ways; by melting the oil in hot water and treating it with peroxide of manganese and sulphuric acid, or by keeping it melted, spread out in thin layers on iron plates. It has also been proposed to bleach it with hypochlorite of lime and dilute sulphuric acid; and bichromate of potash and muriatic acid have more recently been employed for the same purpose.

It was first observed by Zier, and the observation has been since confirmed by Messrs. Pelouze and Boudet, that palm oil is strongly acid, and that its acidity increases with the age of the oil. The quantity of free acid it contains is very great, often amounting to nearly a third of its weight. It is owing to this circumstance that palm oil is so remarkably easily saponifiable. Indeed, a very tolerable soap may be made from it by boiling it for a short time with the alkaline carbonates. Soap made from palm oil and soda is of a firm consistence, has an aromatic odour, and forms an excellent and agreeable detergent. The greater portion of the common brown soap made in Great Britain contains no tallow at all, but consists of a mixture of palm oil and common resin. The melting point of the specimen of palm oil I examined was 99° Fahr. It appeared to be pretty old. Palm oil was formerly supposed to contain margaric and oleic acids; but Mr. Fremy has lately subjected it to examination, and found it to contain a new acid, to which he has given the name of the palmitic. I am happy in being able to confirm the truth of his observations.

---

* Communicated by the Author.

The mode of proceeding to examine the constituents of palm oil is to saponify it with caustic potash or soda, and then to decompose the soap with muriatic or tartaric acid. The mixture of palmitic and oleic acids thus obtained, is to be dissolved in boiling spirits of wine, in which it is very soluble, and allowed to crystallize. The crystals are to be collected and strongly pressed between folds of blotting paper, and again dissolved in alcohol. These operations are to be repeated eight or nine times till the palmitic acid crystallizes quite free from the oleic acid, which remains dissolved in the mother liquors. Before being subjected to analysis it was once more saponified, and the soap decomposed by muriatic acid. The palmitic acid had then a melting point of 140° Fahr. The following are the results of its analyses.

	gramme.					
I.	0·3025	gave	0·827	carbonic acid, and	0·338	water.
II.	0·3147	„	0·860	„ „	0·3545	„
III.	0·2950	„	0·8076	„ „	0·3305	„
IV.	0·2662	„	0·7262	„ „	0·3007	„

This is per cent.

	I.	II.	III.	IV.
Carbon ...	75·48	75·56	75·69	75·46
Hydrogen	12·41	12·51	12·48	12·51
Oxygen...	12·11	11·93	11·83	11·83
	100·00	100·00	100·00	100·00

In order to determine the atomic weight of the acid the silver salt was prepared by precipitating an alcoholic solution of the soda soap with nitrate of silver.

	gramme.					Oxide of silver.	
I.	0·4992	salt gave	0·1456	silver	=	31·31	p. cent. oxide.
II.	0·8204	„ „	0·2414	„	=	31·59	„
III.	0·6228	„ „	0·183	„	=	31·55	„
IV.	0·5375	„ „	0·1585	„	=	31·43	„
V.	0·5385	„ „	0·1575	„	=	31·41	„

Combustion with oxide of copper gave the following results:

I.	0·3205 salt gave	0·621	carbonic acid and	0·249	water.
II.	0·3305 „ „	0·6395	„ „	0·257	„
III.	0·2885 „ „	0·559	„ „	0·222	„

	I.	II.	III.
Carbon	53·57	53·50	53·58
Hydrogen.........	8·63	8·64	8·54
Oxygen............	6·35	6·41	6·43
Oxide of silver...	31·45	31·45	31·45
	100·00	100·00	100·00

The analyses of the hydrate agree best with the following formula:

			Per cent.
66	atoms of Carbon	5044	75·67
128	„ Hydrogen ...	824	12·33
8	„ Oxygen	800	12·00
		6668	100·00

From the determination of the quantity of oxide of silver in the silver salt, however, I am disposed to prefer Mr. Fremy's formula for the hydrate as the more probable. It is

			Per cent.
32	atoms of Carbon......	2446	75·37
64	„ Hydrogen...	399	12·40
4	„ Oxygen......	400	12·23
		3245	100·00

The formula $C_{32} H_{62} O_3 + Ag O$ gives the following numbers for the silver salt:

			Per cent.
32	atoms of Carbon.........	2446	53·35
62	„ Hydrogen......	387	8·44
3	„ Oxygen.........	300	6·54
1	„ Oxide of silver	1452	31·67
		4585	100·00

The hypothetical composition of the anhydrous acid is consequently as follows:

			Per cent.
32	atoms of Carbon......	2446	78·08
62	„ Hydrogen..	387	12·35
3	„ Oxygen	300	9·57
		3133	100·00

The number 3133 agrees very nearly with the mean of five determinations of the silver salt, which gave 3165 for the atomic weight of the anhydrous acid. The baryta salt was prepared in the usual way by precipitating an alcoholic solution of the soda soap with chloride of barium.

Per cent. baryta.
I. 0·5080 salt gave 0·150 carb. of baryta = 22·90
II. 0·4947 „ „ 0·1462 „ „ = 22·92

The calculated quantity is 23·39 per cent. baryta. The atomic weight 3221, which this gives for the anhydrous acid, does not differ essentially from that found by the silver salt.

The following is the mode of preparing Palmitin, the compound of palmitic acid and glycerine. Palm oil is to be subjected to pressure between folds of linen, to separate the more fluid portion. The solid part is to be treated six or seven times with boiling spirits of wine to remove the palmitic and oleic acids, which are very soluble in that liquid. Palmitin, which, on the contrary, is nearly insoluble in hot spirits of wine, is to be then dissolved in hot æther and filtered, to separate it from the impurities which accompany it, and which remain on the filter. On the cooling of the solution the palmitin is deposited in very small crystals. These are to be pressed between folds of blotting-paper and recrystallized out of æther. These operations are to be repeated six or seven times till the palmitin is free from every trace of olein. It is then to be kept melted on the water-bath till the æther is driven off. Pure palmitin melts at 120° Fahr., and when saponified yields palmitic acid with the ordinary melting point of 140° Fahr. Palmitin is quite neutral; on being melted and not allowed to cool it does not crystallize, but forms a half transparent mass like white wax, but which differs from wax in being easily reducible to powder. It is almost insoluble in ordinary spirits; but absolute alcohol dissolves a little of it at a boiling temperature, which precipitates in white flocks on the cooling of the liquid. It is soluble in æther in every proportion. In its appearance and properties generally it very much resembles stearin; palm oil contains only a few parts per cent. of palmitin.

The following are the results of its analysis.

I. 0·3235 gave 0·896 carbonic acid, and 0·3485 water.
II. 0·3384 „ 0·9397 „ „ 0·3744 „
III. 0·3317 „ 0·9195 „ „ 0·3665 „

or per cent.

	I.	II.	III.
Carbon	76·58	76·78	76·65
Hydrogen	11·99	12·29	12·27
Oxygen	11·43	10·93	11·08
	100·00	100·00	100·00

The calculated numbers are,

35	atoms of	Carbon ……	2675	76·73
66	„	Hydrogen …	412	11·80
4	„	Oxygen ……	400	11·47
			3487	100·00

and consequently palmitin must be regarded as consisting of

1 atom of Palmitic Acid $= C_{32} H_{62} O_3$
1 „ Glycerine $= C_3 H_4 O$

1 atom of Palmitin $= C_{35} H_{66} O_4$

It is evident that this gives a very different formula for glycerine from the ordinary one, $C_6 H_{14} O_5$. This, however, gives a much simpler view of its constitution, proving it to belong to the class of simple organic oxides, like those of ethule and methule. This formula also affords a very simple explanation of the decomposition which glycerine undergoes when treated with peroxide of manganese and sulphuric acid. It is then, as is well known, converted into formic and carbonic acids, as under:

$$C_3 H_4 O + O_5 = C_2 H_2 O_3 + H_2 O + C O_2.$$

Palmitin when distilled gave acrolein, but no sebacic acid; palm oil, on the contrary, when subjected to distillation, yielded sebacic acid in abundance. This shows that the other acid which palm oil contains is the oleic, as it is the only fat acid known to yield sebacic acid by distillation.

Messrs. Pelouze and Boudet discovered that palm oil contains free glycerine, which can be obtained from it by treating the oil with hot water and filtering. I have also succeeded in obtaining it by this process. The acid which accompanied it was neutralized with carbonate of soda, and the glycerine extracted by alcohol. Its quantity was very inconsiderable. The presence of free glycerine in palm oil is what might have been expected from its containing so large a quantity of free palmitic and oleic acids.

Palm oil also contains small quantities of a blueish-green colouring matter. It is heavier than the oil, and forms a thinnish layer, which adheres to the lower part of the cake which palm oil forms when it is melted in water and allowed to cool; I thought at first it was owing to the palm oil having been prepared in copper vessels, but on examination I found that it was wholly of a vegetable nature. Hydrosulphate of ammonia had no effect upon it; caustic alkalies rendered it colourless; muriatic and nitric acid had a similar effect, but sulphuric acid blackened and apparently charred it.

Butter of Cacao.—Cacao butter is obtained from chocolate beans, the fruit of the *Theombra Cacao*; either by subjecting the bruised beans to pressure between hot iron plates, or by boiling them in water and scumming off the oil as it collects on the surface. One pound of beans usually yields from three to four ounces of the oil. When first prepared it has usually a yellowish colour, but this can be easily removed by boiling it with water, or still more completely by treating it with hot alcohol. The taste of cacao butter is mild and agreeable; and its smell, which resembles that of the beans, is owing to the presence of an oily matter, which may be easily removed by one or two digestions with alcohol. With caustic soda cacao butter forms an excellent soap; but it is by no means very easily saponified. The melting point of cacao butter is stated by most chemical authors at 122° Fahr. I found that of the specimen I examined only 86° Fahr. MM. Pelouze and Boudet state it at 85° Fahr. Cacao butter is remarkable for the length of time it may be kept without becoming rancid.

In order to subject the acids which cacao butter contains to examination, it was saponified by caustic potash and the soap decomposed by muriatic acid. The melting point of the mixed acid was 125° Fahr.; they were then dissolved in hot spirits of wine and allowed to crystallize, when the crystals were collected and carefully dried by pressure. These solutions and crystallizations were repeated nine or ten times to free them from the less crystallizable acids, which remained dissolved in the alcoholic mother liquors. The crystals had then a melting point of 157° Fahr., and on analysis they proved to be stearic acid. The following are the results.

I.	0·318	gave	0·8835	carbonic acid,	and	0·3622 water.
II.	0·2595	„	0·719	„	„	0·300 „
III.	0·299	„	0·882	„	„	0·347 „

or per cent.

	I.	II.	III.
Carbon	76·82	76·61	76·85
Hydrogen ...	12·65	12·84	12·86
Oxygen	10·52	10·55	10·29
	100·00	100·00	100·00

This gives the following formula for the hydrous acid:

			Per cent.
68	atoms of Carbon	5197·6	77·04
132	„ Hydrogen...	848·6	12·58
7	„ Oxygen......	700·0	10·38
		6746·2	100·00

In order to determine the atomic weight of the acid the æther was prepared in the usual way, by dissolving a portion of stearic acid in alcohol and saturating it with muriatic acid gas.

The æther is a semi-transparent crystalline body, very like stearin. It was repeatedly washed with hot water to free it from muriatic acid. It was then carefully dried and subjected to analysis; the following are the results.

I. 0·2715 gave 0·7595 carbonic acid, and 0·3155 water.
II. 0·240 „ 0·671 „ „ 0·2805 „
or per cent.

	I.	II.
Carbon	77·35	77·30
Hydrogen.........	12·91	12·94
Oxygen............	9·79	9·76
	100·00	100·00

The formula deducible from these analyses, $C_{72}\ H_{144}\ O_7$, is equivalent to 1 atom acid, 1 atom æther, and 1 atom water.

			Per cent.
72 atoms	of Carbon ...	5503·3	77·49
144 „	Hydrogen	898·5	12·62
7 „	Oxygen...	700·0	9·86
		7101·8	100·00

When one atom of æther and one of water are subtracted from the above,

$$\begin{array}{l} C_{72}\ H_{144}\ O_7 \\ \underline{-C_4\ \ H_{12}\ \ O_2} \\ C_{68}\ H_{132}\ O_5 \end{array}$$

we obtain the numbers $C_{68}\ H_{132}\ O_5$ as the formula of anhydrous stearic acid:

			Per cent.
68 atoms of	Carbon.........	5197·7	79·70
132 „	Hydrogen......	823·6	12·58
5 „	Oxygen	500·0	10·38
			100·00

When cacao butter is distilled it yields no appreciable quantity of sebacic acid, but when the uncrystallizable acid which remains in the alcohol out of which the stearic acid has crystallized is subjected to distillation, traces of sebacic acid are readily obtained. This indicates that the quantity of oleic acid contained in cacao butter is by no means very considerable.

I have every reason to believe that besides the stearic and oleic acids, cacao butter contains a third acid, which has a melting point of about 140° Fahr., and which may probably be the margaric.? Its nature, however, must be the subject of some future investigation.

XXXVII. *Examination of a Fourth Experiment adduced by* Professor Faraday *in support of* M. de la Rive's *Theory, and regarded by* Dr. Fusinieri *to be demonstrative. By Dr.* STEPHEN MARIANINI, *Acting Professor of Particular and Experimental Physics in the University of Modena, &c. &c.*[*]

"......... senza il corredo dell'esperienza non siamo sicuri che la natura sia con noi."—*Romagnosi, Dell'indole e dei fattori dell'incivilmente*, p. 3.

WHEN in the summer of 1835 I undertook the researches which form the subject of the preceding memoir 4th[†] upon the theory of electromotors, I intended to treat of only two of the experiments adduced by Professor Faraday in support of the theory of M. de la Rive; namely, of the electric currents obtained by the immersion of platina and zinc in sulphuric acid, not placed in contact with each other in those parts not immersed in the liquid; and of the spark obtained by bringing the copper wire attached to a large copper plate immersed in acid, in contact with the zinc wire attached to a zinc plate immersed in the same liquid. The third fact brought forward by Professor Faraday is the positive electrization of copper, when, joined with iron, it is immersed in a solution of sulphuret of potassa. I did not wish to speak of this as being at all analogous to the other facts already adduced by M. de la Rive, and which I had already shown in no way favoured the new theory. But having seen that Dr. Fusinieri laid great stress upon this same fact, I began to examine it, and with the assistance of clear and easy experiments, I think I have shown that this also was not more favourable than the others to the theory of the learned Genevan.

I did not write of the fourth experiment pointed out by Professor Faraday, because, not only did it appear to me less conclusive than the others, but I also thought that it must appear so to whoever is acquainted with what I have already published on electromotors. I now see that I partly deceived myself, for Dr. Fusinieri in the discussion into which he enters in some pages of the *Annale delle Scienze del Regno Lombardo-Veneto*, relative to my aforesaid memoir, attaching not a little importance to this experiment, declaring it of itself demonstrative. And as after such a judgement it may be believed by some that I am of the same opinion, and that for this reason I have been silent on the subject, I think it incumbent on me to discuss it in this article, which may therefore be considered as a continuation of the foregoing memoir 4th.

---

[*] From the author's *Memoria de Fisica Sperimentale scritta dopo il* 1836. *Anno secundo*, 1838. Modena, 1838, 8vo.

[†] The memoir here mentioned is inserted in volume 21 of the Memoirs of the Italian Society of Sciences of Modena. Of the other three upon the same subject, the first, read at the Athenæum at Venice on the 22nd of May, 1828, was afterwards published in the 20th volume of those Memoirs; the second was printed in Venice by Alvisopoli in 1830, and in the 45th volume of the *Annales de Chimie et de Physique*; and the third, in the first *bimestre* for 1836 of the Scientific Annals of the Lombardo-Venetian Kingdom.

The experiment now under consideration is that in which, by agitating with a feather the liquid in which are immersed the zinc and the copper of a voltaic pair, the electric current is strengthened. And to explain such increase of strength, Professor Faraday observes that the neutralized acid stratum adhering to the zinc is removed, the acid stratum in contact with it is renewed, and thence the chemical action acquires new force.

By stirring with a feather the liquid in which the voltaic pair is immersed, the neutralized acid stratum adhering to the zinc is removed when the electromotor has been a short time in action; but when the electric current operates for some time, half an hour, for example, the greater part of the neutralized acid stratum remains still adhering to the zinc, notwithstanding the agitation of the liquid. I will suppose, then, that an experiment is here treated of in which the circuit is completed only for a few minutes, in which case, by this agitation, the neutralized acid stratum covering the zinc is in great part removed.

By means of the above-mentioned operation, if it is performed when the electric circuit is interrupted, the current is considerably strengthened when the circuit is again completed; but whenever the circuit is completed many times, and especially when the liquid is water, acidulated or strongly saline, the liquid may be agitated with a feather as much as may be desired, since the electric current does not acquire more strength, as is shown by the constantly decreasing deviation of the galvanometer. And this is very natural, since if on the one hand, the agitation of the liquid removes some of the neutralized acid stratum from the surface of the zinc, the electric current causes it to adhere again, and probably in greater quantity than the agitation of the liquid subtracts. It is otherwise, if, instead of stirring the liquid with a feather, the surface of the zinc is lightly scraped with the angle of a plate of glass; since, in that case, detaching from the metal more neutralized acid than that which the current causes to adhere to it in the same time, the electric current is in a degree visibly strengthened. But here it is said that the current is strengthened by agitating the liquid with a feather; we will therefore suppose that such an operation must be made with the circuit interrupted.

When the force of an electromotor becomes weakened by keeping the circuit complete for some time, we know that by suspending the communication between the poles it recovers by itself its primitive force, either entirely or in part, without the liquid being agitated in the smallest degree*. In order to estimate the true value which in the present question ought to be attached to this experiment, it will be useful to know how much of the strength is owing to the agitation of the liquid, and how much to the repose of the electromotor.

For this purpose I procured forty plates of zinc, and as many of copper; I tried five pairs, one after the other, taken at hazard, and all produced an equal deviation of the galvanometer; all in similar circumstances electrized to an equal tension the electrometer furnished with a condenser. I made these first trials to be certain that the other pairs also would produce the same effects under similar circumstances, and thus to be able to make use of new plates in every experiment, in order the better to obtain the necessary similarity of circumstances.

I have made other preliminary experiments to ascertain what force such pairs would lose when kept in action for a given time, and how much of it they would regain by being left in repose for a like given time. These things being premised, I came to the examination of the experiment.

II. I took therefore a plate of copper and one of zinc connected by means of the wire of the galvanometer, and plunged them two thirds of their height into the acidulated water. The galvanometer deviated quickly full eighty degrees, but after three minutes the deviation was only twenty-one degrees. Having interrupted the circuit, and left it so for twenty minutes without agitating the liquid, the deviation which followed on first re-completing it was thirty-two degrees, and it stopped at last at twenty-one. After three minutes the needle was nearly stationary at nineteen. I interrupted the circuit for twenty seconds, and agitated the liquid, and then completing the circuit, the first deviation was of thirty-eight degrees, and when the needle no longer vibrated it pointed to about twenty-three.

In another experiment I left the circuit closed during an hour and five minutes. The needle of the galvanometer pointed thirteen degrees; I opened the circuit, and after five minutes of repose I closed it; the needle pointed exactly to thirty-five, and stopped at the eleventh degree. The circle being again interrupted, and the liquid stirred with a feather, the needle deviated to thirty-nine, and finally stopped at about thirteen degrees.

---

* See Memoir upon the loss of tension, &c. in the *Annales de Chimie et de Physique*, August, 1828.

From these and other similar experiments I have seen that the agitation of the liquid, after the electromotor has acted for some time, produces an increase of strength in the current independently of the repose. Since in the first, for example, of the above experiments, by the inaction only the pair would have recovered strength to make the needle deviate thirty-two degrees, whilst with the repose and the agitation of the liquid combined the force was such as to make it deviate thirty-eight degrees. Therefore six degrees of deviation above the thirty-two, or the degree of strength expressed by them, was procured by the agitation of the liquid. In the second experiment, then, the force recovered by the agitation of the liquid is expressed by four degrees of deviation above the thirty-five.

III. I convinced myself in another way also of this advantage, which agitating the liquid with a feather confers upon the current, by observing the time required for the needle of the galvanometer to recede a given number of degrees from the point where it had ceased to oscillate; since when the liquid was agitated the time was generally greater than when it was not agitated, and especially if the liquid itself was in any degree a conductor, as for example, spring water, or water rendered slightly saline, which also shows that with the agitation of the liquid we put an obstacle to the cause which tends continually to slacken the current when the circuit is complete.

It is true that this increase of strength caused by agitating the liquid is but trifling when compared with that which is caused by repose. At all events the proposition of Faraday is not the less true, although he may not have remarked that the greater part of the increase of strength is owing to the repose, and the less to the agitation of the liquid: and so much the more founded is the proposition of the learned Englishman, that these two causes of increase of strength resolve themselves into one; since even simple repose causes the voltaic pair to acquire force, probably because the liquid freeing the neutralized acid detaches it from the zinc.

IV. The liquid being agitated, the chemical action acquires new vigour, because a new stratum of acid comes into contact with the zinc. When indeed this experiment is made by immersing the pair in distilled water (in which case the effects are the same, except that they are very slow and small) it is not a new acid stratum that comes to act upon the zinc; yet it will be a stratum of something which will take its place; that is to say, which causes the chemical action exercised by the liquid upon the zinc to take new vigour. I limit myself, therefore, very willingly to the experiment esteemed by Professor Faraday "of itself convincing," in which there is no doubt that when the liquid is agitated in the said circumstances these two facts follow,—the restoration of the chemical action, and the strengthening of the current.

But from both these phænomena arising when the liquid is agitated, does it follow immediately, as a necessary consequence, that the second is occasioned by the first? To admit this is to acknowledge that the chemical theory may be proved independently of this experiment. Then this experiment cannot be proclaimed "of itself convincing," because some other is necessary to prove the truth of the theory of M. de la Rive.

V. If it is not of itself sufficient to demonstrate the truth of the said theory, let us see if at least the aforesaid experiment may serve to confirm it, on the hypothesis that it may already be otherwise proved.

The Delarivian theory being therefore supposed true, that is, it being supposed that the current in the voltaic pair becomes stronger, by the chemical action exercised by the liquid upon the zinc, than upon the copper; it is easily understood, it is true, how by renewing the acid stratum upon the surface of the zinc, the current may be rendered more powerful. But together with the zinc, the copper also is immersed in this liquid, and this metal is attacked itself also by the liquid; and if proofs of this were wanted, the same chemical theory would immediately furnish them; because when copper is joined with gold, platina, charcoal, it becomes more highly electrical. Then by stirring that liquid the chemical action upon the copper is also increased, and this, if the theory of M. de la Rive be just, must be to the injury of the current.

Is it not possible that the trifling increase of power observed on agitation, depends on the chemical action on the copper being also increased in power, in consequence of which the effect that is apparent is only that which is due to the excess of the increase of action on the zinc above that of the increase of action on the copper? Had this been the case it would have been very easy to see it in the experiment I am about to relate.

VI. Instead of placing the two plates of the pair in the same cup, I put the copper into one and the zinc into another;

in each cup was contained the usual acidulated water, and in each was immersed one end of the galvanometrical wire, which was of copper. The circuit being complete, making the two plates to communicate with each other metallically, after the needle was quiet (it pointed to twenty-five degrees) I stirred the liquid in the cup where the zinc was, but I did not perceive any increase in the deviation. I interrupted the circuit, and agitated the liquid, but after it was closed the needle showed no greater deviation, or at least the increase was not perceptible. Then at least the increase of power was less than when the liquid was agitated whilst the two plates were in the same cup; and still, according to the theory of M. de la Rive, in this last case, the increase of power ought, on the contrary, to be greater, because the chemical action upon the zinc was increased, while that on the copper remained unaltered.

But again, if the experiment is repeated, leaving the liquid in which the zinc is immersed always quiet and agitating that in which the copper is immersed, almost the same increase in the deviation is observed which was obtained in the experiments in which the plates were in the same cup; yet according to the new theory, by strengthening the chemical action upon the copper and not upon the zinc, the copper ought to become less negative, and thence the current become weakened instead of strengthened; therefore, according to the results of the examination hitherto made, the experiment treated of contradicts the chemical theory, instead of confirming it.

VII. Shall, then, this little increase in the force of the current which follows upon the agitation of the liquid, or upon such a cleaning (*pulimento*, Fr. *décapé*) of the plates, remain without explanation? I believe it will, so long as no other effect is required to take place from that agitation than the increase of the chemical action of the liquid on the metal.

Since by this action either electricity is not developed, or even if it is developed it is not that which circulates in the electromotors, at least in ordinary cases, as I have shown, with many experiments in my second memoir upon this subject*: thus the strengthening of this action on either metal causes, of itself, no change in the current, as the preceding experiments may show. But if from this agitation of the liquid, besides the strengthened chemical action of the liquid itself upon the metals, we see result also an increase in the conductibility of the apparatus, an increase observed and noted by M. de la Rive, and by myself many years ago*, the explanation of the phænomenon will become easy and natural; and whoever wishes to see this verified has only to repeat the experiment, which I now proceed to describe.

---

* Memoir upon the chemical theory of Voltaic Electromotors, simple and compound. Venice, 1830.—*Annales de Chimie et de Physique*, September and October, 1830.

Having plunged a voltaic pair into two cups of acidulated water, as in the experiment of the preceding paragraph, I put the liquid of the cup in which the copper was plunged in communication with a third cup, containing some of the same liquid, by means of an arc formed by a plate of copper; and in this cup was plunged another plate of copper, which formed one of the extremities of the galvanometric wire; whilst the other was dipped, by means of a plate of copper, into the cup containing the zinc plate of the pair. All being thus arranged, I closed the circuit, and the needle deviated nearly to the fortieth degree, and then stopped at the eighth; and after a minute, I interrupted the circuit and held it open for half a minute; after which time the needle was stationary at zero, and having completed the circuit again the needle deviated quickly to thirty degrees, and then stopped again at the eighth degree. Hitherto the liquid was not agitated; but a minute after the needle became stationary, I opened the circuit, I stirred the liquid in the third additional cup, that namely, in which was plunged one end of the arc of copper joined to the pair, and one plate of copper attached to one end of the wire of the galvanometer, and after half a minute, the circuit being completed, the needle deviated to the thirty-fifth degree, and stopped afterwards upon the ninth.

If instead of stirring the liquid the plates immersed in it are slightly touched with a feather, the increase of strength is observed without interrupting the circuit. In one of the experiments made in this manner, the needle was stationary at the sixth degree, and after brushing the plate of copper annexed to one end of the galvanometric wire and immersed in the third cup, the needle moved to nine degrees, whence it turned quickly to six. The same thing happened upon brushing gently the plate which served as an arc in the part immersed in the cup containing the zinc plate of the pair.

Now in such experiments those plates of copper served only to convey the electricity put in motion by the voltaic pair; then the agitation of the liquid or that light friction upon the plates of copper, strengthens the current in proportion as the power of conducting or transmitting the electricity is strengthened in the apparatus.

---

* Memoir upon the loss of tension which electromotors undergo, &c. Section 15.—*Annales de Chimie et de Physique*, August, 1828.

VIII. We may conclude from what has been said, that the fourth experiment of Professor Faraday, which, according to Signor Fusinieri, is itself demonstrative of the truth of M. de la Rive's theory, is at least inconclusive.

And here it may be asked, whence then comes that alteration in the conductibility of the apparatus? And M. de la Rive will reply that the agitation of the liquid or the cleansing of the plates increases the transmissibility for the electricity of the same; and I shall add that the alteration in the relative electromotive faculty, or, which is the same thing, in the electrotism of the plates, also concurs in this. By the current, indeed, the electrotism of those plates in which the electricity enters, becomes positive, and those from which it proceeds to pass into the liquid negative, and that alteration proceeds from the matter which the current collects upon the plates themselves. Now by the agitation of the liquid, and brushing the plates gently with a feather, all or a part of this matter which alters the electrotism of them, and hence necessarily strengthens the voltaic current, is removed. I was convinced that this was connected with the phænomenon, by observing that in the second experiment of the preceding paragraph, if any one of the plates in which the electrotism becomes negative is brushed with a feather, there is an increase of the current much greater than when one of those in which the electrotism become positive is brushed; and we know that the changes in electrotism to positive are, *cæteris paribus*, more trifling than the changes to negative*.

IX. The above remarks relative to the experiments in question are those which are most obvious, and which suffice to show the little or no importance which it has relatively to the theory of the electromotors. It may, however, assist those who desire to repeat that experiment and to study it, briefly to bring under their notice some other phænomena which may present themselves, and which do not escape the attentive observer.

Sometimes the feather is scarcely introduced into the liquid when the force of the current is increased, and at other times it is lowered. At times the said force decreases when the feather is scarcely withdrawn from the liquid, at others it increases.

In two cases the force of the current is seen to increase at the moment when the feather is introduced into the trough or cup in which are plunged the plates of the voltaic pair.

1st. When the vessel being small, the level of the liquid increases by the immersion of the feather, and this is not introduced between the two plates. In order that such increase of level might not be sensible, I made use for the experiments above described, of rather large cups, so that the two plates of the pair remained distant from each other four centimetres.

---

* See the Memoir upon the alterations produced by the voltaic currents in the electrotism of the principal metals, 1837, p. 140.

2ndly. In the second place the force of the current is increased on the first insertion of the feather, when this being wet, one of the plates is touched with it; because it is as if of these plates another part was immersed in the liquid besides that which is already immersed; and in this case if it is the plate of copper which is touched by the wet feather, the increase of force is much greater than when the plate of zinc is touched; and this is very intelligible after what we know relative to the voltaic pair with unequal plates*.

The deviation of the galvanometer which indicates the force of the current, decreases in some degree when the feather is introduced between the two plates, and parallel to them, or but little inclined; for in this case the feather intercepts part of the current, or, to speak more correctly, obliges part of the current to run through a greater distance before arriving at the other plate.

Whenever the feather is in the liquid, but not between the plates, or if it is between them, remains there so as not to intercept the current in any great measure; in this case, the feather being withdrawn, the current itself slowly diminishes in some little degree.

Finally, when the feather has been for some minutes immersed between the plates, and so as to cause the deviation of the galvanometer to diminish in some degree, and has been left there without agitating the liquid, upon withdrawing the feather, or even only drawing it aside, the deviation is immediately seen to increase an equal number of degrees, and the needle to stop above that to which it pointed before the feather was placed between the plates.

In one of such experiments, in which the liquid conductor was water very slightly saline, the circuit was scarcely closed, when the needle deviated eighty degrees; after an hour the needle was for several minutes stationary at twenty-eight; the feather being introduced between the plates, the needle itself moved to twenty-five. After a space of three minutes I removed the feather, and the needle oscillated to thirty-three and then stopped at thirty. This is an experiment at first sight very singular, and for which it would be difficult to assign a reason, unless it be remembered that when an electromotor has lost force by the circuit remaining complete for some time, not only does it regain its strength when the circuit is interrupted, but it recovers a part of it also, when instead of interrupting the circuit the current itself is only slackened[*].

---

* Essay before quoted, §§. 129 and following.—*Annalen, &c.*, Oct. 1826.

[To be continued.]

XXXVIII. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. Francis *and* H. Croft.

To Richard Taylor, Esq.

Dear Sir,

We have the honour to forward to you for insertion in your valuable Journal, the accompanying notices of the researches of Continental chemists. We trust that, however brief and imperfect, they may prove acceptable to English readers, as but little of what is done abroad, especially in Germany, seems to find its way into England, or, at least, until after the lapse of some years. As instances of this, we need only refer to some papers by English chemists recently published in the Philosophical Magazine, which in a great measure have been but repetitions of what has been done long since on the Continent. Thus Professor Apjohn would have been spared a great deal of labour had he been earlier acquainted with the works of Liebig and Pelouze, and, still more, with those of Mulder. Professor Johnston might have been enabled to shorten his experiments on the Benzoe resins, and probably would have modified his results, if he had known of the researches of Van der Vloet on the same subject, with whose formulæ some of Professor Johnston's results agree. Our next communication shall furnish some comparison of these two excellent investigations. The double oxalates, or at least several of them, which have been so well described by Professor Graham in his celebrated paper on the constitution of the Melates, &c. had been described already by Professor Mitscherlich. The fact that aldehyd is contained in nitrous æther, as lately stated by Dr. Golding Bird, was known years before in Germany from the labours of Heinrich Rose.

It is, however, needless to multiply instances: our object is to assist in preventing their occurrence, and to save English chemists some little unnecessary trouble. It would, it is true, be better to give full translations of the papers, but our own studies do not allow us time for this:—the extracts, which are made in our few leisure hours, are intended to give an idea, not of what has been done, but merely of what is actually going on in the Continental chemical world.

---

[*] See §. 11 of the Memoir already cited, upon the loss of tension which electromotors undergo when the circle remains closed, and upon the regaining of the primitive tension when the communication between the poles is suspended.

Should our attempt meet your approval and that of your readers, we shall with pleasure continue our communication monthly. We have the honour to remain,

Dear Sir, yours truly,
William Francis.
Berlin, Dec. 13, 1840. Henry Croft.

P.S. The Journals consulted are Poggendorff's *Annalen der Physik und Chemie*; Liebig's *Annalen der Chemie und Pharmacie*; Erdmann's *Journal für Praktische Chemie*; *Archiv der Pharmacie*; *Annales de Chimie et de Physique*; *Journal de Pharmacie*; Berzelius's *Arsberattelse*, and the German translation by Wöhler.

MM. Dumas and Stas have published a second treatise on chemical types *. The principal subject of this treatise is the action of potassa on the various kinds of alcohol, both those which are evidently similar to the wine alcohol, and also on those substances which have been considered as analogous to alcohol, as for instance, glycerin, &c. In many cases it is possible to convert an alcohol into its equivalent acid; but yet chemists have not been able to convert an acid into its corresponding alcohol. The discovery and examination of these alcohols is much more important than that of any acid; for the acid is only interesting in as far as its combinations with inorganic bases are concerned, while an alcohol is the head of a numerous family. A new alcohol may be compared to a new metal. The formula of the alcohol of any acid is found by substituting H^2 for O^2 in the hydrated acid.

In the experiments on the action of hydrate of potassa, it was found convenient to employ a mixture of equal parts of fused potassa and well-burnt lime. This substance affected the glass vessels much less than potassa alone; it may, for shortness' sake, be called lime-potassa.

If this substance be moistened with alcohol and subjected to a gentle heat, a gas is developed which consists chiefly of hydrogen, and a little carburetted hydrogen and carbonic oxide. The residue contained a quantity of acetic acid, proportional to the quantity of alcohol employed. If the heat is increased, carbonate of potassa remains behind, and marsh gas (light carburetted hydrogen) is given off; for under the influence of alkalies, acetic acid is decomposed into carbonic acid and marsh gas, $C^4 H^4 O^4 = C^2 O^4 + C^2 H^4$. In the usual preparation of acetic acid, two atoms of hydrogen are replaced by two atoms of oxygen; by the action of alkalies chloracetic acid $C^4 H Cl^3 O^4$ is decomposed into carbonic acid and chloroform $C^2 O^4 + C^2 H Cl^3$; acetic acid is decomposed into $C^2 O^4 + C^2 H^4$, or $C^2 O^4 + C^2 H^3 H$. Alcohol may therefore be considered as composed of two bodies representing carbonic acid, one the methylic aldehyd, the other marsh gas, $C^4 H^6 O^2 = C^2 H^3 + C^2 H^2 O^2 =^* C^2 O^4 + C^2 O^4$. In the usual method of forming acetic acid $C^2 H^2 O^2$ loses H^2, and takes up O^2, there then remains $C^2 O^2 O^2 + C^2 H^4 = C^4 H^4 O^4$, or acetic acid. By the action of alkalies H^4 is given off; the water of the hydrated alkali is decomposed, H^2 is developed and O^2 assimilated. If the hydruret of potassium be treated with dilute sulphuric acid, the hydrogen of the hydruret is developed, and also that of an atom of water, the oxygen of which combines with the potassium to form potassa. So it is with the body $C^2 H^2 O^2$, $C^2 H^2 O^2 - H^2 + O^2 = C^2 O^4$, we then have $C^2 O^4 + C^2 H^4 =$ acetic acid, as before. With pyroxylic spirit it is the same; two atoms of water are decomposed, $C^2 H^4 O^2 - H^2 + O^2$ (from $H^2 O^2$) gives $C^2 H^2 O^4 + H^4$. In the residue, however, is found a large quantity of oxalic acid. Peligot has shown that by the action of alkalies formic acid is converted into oxalic acid. Hydrogen is thereby developed, $C^2 H^2 O^4 - H = C^2 H O^4$; the oxalic acid in its turn may lose H, and become $C^2 O^4$, or carbonic acid.

---

* *Annalen der Chemie und Pharmacie*, xxxv. p. 129, from *Annales de Chimie et de Physique*, vol. lxvi. p. 113.

One part of ethal treated with 5–6 parts of lime-potassa, gave at 210° to 220° C. a considerable quantity of hydrogen. The residue, treated with hydrochloric acid, gave a flocky precipitate: this was combined by boiling with hydrate of baryta, and the salt thus formed freed from excess of ethal by boiling alcohol, and then decomposed by means of hydrochloric acid. The precipitate is ethalic acid. The mixture for preparing it must not be heated above 220° C., and must be kept at that temperature for five or six hours. Ethalic acid is colourless, tasteless, inodorous, lighter than water, easily fusible, solidifies at 55° C., and forms fine shining needles; insoluble in water, soluble in boiling alcohol and æther; volatile. Formula $C^{32} H^{32} O^4$. The salts, excepting those of the alkalies, are insoluble in water and alcohol. $\dddot{Fe}^2 \overline{Eth}$ is dark yellow, $\dot{Co}\,\overline{Eth}$ is rose-coloured, $\dot{Cu}\,\overline{Eth}$ light green, $\dot{Ag}\,\overline{Eth}$ easily decomposed. The potash salt is formed by fusing ethalic acid with carbonate of potassa, and extraction with alcohol; it is white, with a pearly lustre, decomposed by excess of water;

---

* The sign here should represent *equivalency*, but not at all *equality*.—R. K.

insoluble in æther; formula $C^{32}H^{31}O^3 + \dot{K}$. Soda salt crystallizes in large tabular crystals.

One part of potatoe spirit oil treated at 200° with ten parts of lime-potassa, gave hydrogen and valerianic aldehyd; this is further decomposed into valeric acid, $C^{10}H^{12}O^2 - H^2 = C^{10}H^{10}O^2.\dot{K} - H + O = C^{10}H^9O^3.\dot{K}$. The residue is distilled with sulphuric acid, the vapours condensed in a solution of carbonate of soda; out of this the pure valerianic acid may be procured by distillation with phosphoric acid. Specific gravity of the hydrated acid at 16°·5 C. = 0·937; boils at 175°, is not solid at —15°; decomposed by chlorine, not by iodine and bromine; by anhydrous phosphoric acid is decomposed into inflammable gases and valeron, &c. &c. Formula $C^{10}H^{10}O^4 = C^{10}H^9O^3 + HO$; density of vapour = 3·55; found by experiment = 3·67. There is also a hydrate with three atoms of water. Many salts do not crystallize. The silver salt is anhydrous.

Chlorovalerisic acid is formed when dry chlorine is passed into valerianic acid without the aid of sunshine: the reaction is violent. The acid is semifluid, transparent, heavier than water, inodorous, burning taste; at —18° still fluid; decomposed at 100–120°, with development of hydrochloric acid; combines with water; the impure acid must therefore not be washed. Formula $C^{10}H^7Cl^3O^4$.

Chlorovalerosic acid. Formed by passing chlorine through valerianic acid in the sunshine: the chlorine is driven out by a stream of carbonic acid. Semifluid, inodorous, burning bitter taste, heavier than water; not solid at —18°; decomposed above 150°. With water forms a hydrate. Aqueous solutions not precipitated by acid nitrate of silver, but the solutions in alcohol and æther are after a short time. Formula $C^{10}H^6Cl^4O^4$. Potash salt is similar to the valerianate. The acid is decomposed by an excess of alkali. The silver salt decomposes in the dark; chloride of silver and a peculiar body $C^{10}H^6Cl^3O^4$ are formed. Formula of the silver salt $C^{10}H^5Cl^4O^3.\dot{Ag}$. The oily hydrate contains three atoms of water. The boiling point of valerianic acid varies with the quantity of water it contains; all three acids form hydrates with three atoms of water, and are herein similar to the triacetate and triphocenate of lead. By continued distillation of potatoe-oil with nitric acid the valerianic aldehyd is formed, also by acting on it with sulphuric acid and chromate of potassa, or binoxide of manganese. The aldehyd has the formula $C^{10}H^{10}O^2$.

Glycerin treated with lime-potassa in the same manner gave hydrogen, and in the residue formic and acetic acids; a glycerinic acid could not be obtained.

Aceton is decomposed into carbonic acid and marsh gas, as Persoz has already stated.

By passing the vapours of aldehyd over the heated lime-potassa, hydrogen is developed, and the residue contains a large proportion of acetic acid, $C^4 H^4 O^2$ lose H and takes $O = C^4 H^3 O^3$; aldehyd therefore resembles the oil of bitter almonds.

Oxalic æther treated with potash gives hydrogen and acetic and carbonic acids; oxalate of potash and alcohol are first formed, out of these are produced carbonate of potash, acetic acid, and free hydrogen. Acetic æther gives hydrogen and acetic acid; benzoic æther gives hydrogen, benzoic and acetic acids; iodide of ethyl gives iodide of potassium and olefiant gas, $C^4 H^5 I^2 + \dot{K} = C^4 H^4 + \dot{K} I^2 + H O$*. This æther forms with chlorine; chloride of ethyl and iodine is separated, ${}^{4}_{10}E F$ is exactly similar to R F. Chloride of æthyl gives water, olefiant gas, and chloride of potassium; the vapour treated with ammonia gives chloride of ammonium and olefiant gas, $C^4 H^5 Cl^2 + N H^3 = C^4 H^4 + N H^4 Cl$. Common æther gives carbonic acid, marsh gas, and hydrogen; treated with chromate of potash and sulphuric acid it forms acetic acid.

Acetate of mæthyl-oxide gives hydrogen, formic and acetic acids; chloride of mæthyl gives hydrogen, chloride of potassium and formic acid, which is afterwards decomposed, $C^2 H^3 Cl + K O + H^2 O^4 = C^2 H O^3 + KCl + H^4$. Oxide of mæthyl gives hydrogen and formic acid. When the vapour of alcohol is passed over heated baryta, the following decompositions take place: first, hydrate of baryta and olefiant gas are formed; in the second stage, the produced hydrate decomposes the alcohol into acetic acid and hydrogen; and in the third, the acetate of baryta forms carbonate and marsh gas. The gas given off contains olefiant gas, marsh gas, and hydrogen, and perhaps some carbonic oxide.

It appears, therefore, that several alcohols are capable of producing their own peculiar acid. According to the researches of the late M. Delalande, camphor when treated with potash forms a peculiar acid, = camphor + water. $C^{20} H^{16} O^2$ forms $C^{20} H^{18} O^4$.

---

* I do not know whether this method of writing organic bodies is known in England. Berzelius proposes to write the carbon above, the hydrogen below, the nitrogen in the middle; the oxygen to be expressed by dots, as before. Tartaric acid is ${}^{4}_{4}\overset{\cdot\cdot\cdot\cdot\cdot}{T}$, Benzoic ${}^{14}_{10}\overset{\cdot\cdot\cdot}{Bz}$, &c., &c.—H. C.

A tabular view of several acids, with their hypothetical types, which here follow, we have not thought necessary to add; the above short extract of this very important paper is only an outline, those who wish for more information must refer to the original paper.

The oil obtained by distilling the fresh plants of *Ruta graveolens* is yellowish-green, smells like the plants, tastes bitter and aromatic; sp. gr. = 0·837 at 18° C. If allowed to stand some time over chloride of calcium and then distilled, it boils at 218°, and the boiling point rose to 245°. The sp. gr. of the colourless product varies from 0·831 to 0·838. Rational formula $C^{28} H^{28} + O^3$. Specific gravity of the vapour was found to be 7·892, ought to be 7·690; therefore the atom is equal to four volumes. The oil is soluble in sulphuric acid, and is precipitated by water; hydrochloric acid gas has no action; by chlorine and nitric acid it is decomposed. These experiments have been made by Dr. H. Will in Giessen. (*Annalen der Pharmacie*, xxxv. 235.)

Erdmann has examined the action of chlorine and bromine on indigo. The indigo-blue used for these experiments contained a little indigo-red, which is, however, of no consequence. Dumas's formula for indigo-blue is $C^{16} H^5 N O^2$; Erdmann's is $C^{32} H^{10} N^2 O^3$. When chlorine is passed into indigo-blue suspended in water, the blue colour changes to a grayish-green, and then to yellow. Hydrochloric acid remains in solution. It is best to employ a low temperature. When this mass is distilled with water a volatile substance passes over; it is called chlorindopten, or chloride of indopten; it is formed in very minute quantities; it may be purified by repeated distillations with water and sublimation; it is then obtained in the form of fine needles of a peculiar unpleasant smell; fuses to a colourless oil; is not very volatile; soluble in hot water, but very little in cold; soluble in alcohol. Formula $C^8 H^7 Cl^2 O$. It is, however, a compound of two substances. When heated with caustic potassa or its carbonate the disagreeable smell disappears, a substance passes over similar in appearance to chloride of indopten, but differing from it in not giving a sour solution with water. Erdmann calls it chlorindatmit (!), from ἀτμός. It is formed in very small quantities; its formula is $C^{12} H^4 Cl^3 O^2$. The residue, after distilling off the chlorindatmit, is crystalline, soluble in water, less so in alcohol; it is allowed to stand in the air and then extracted with alcohol. Out of this solution acids precipitate chlorindoptenic acid. This body has a disagreeable smell, behaves similar to chlorindopten, reacts acid, gives with a salt of silver a voluminous yellow precipitate, with lead a white one. Formula of silver salt is $C^{12}H^{2}Cl^{3} . \dot{A}g$; formula of free acid $C^{12}H^{3}Cl^{3}O$.

After the chloride of indopten has been distilled off, the residue is boiled with a large quantity of water, a resinous matter remains behind, and the filtered solution deposits a a red body; on evaporation more is obtained, but less pure. This substance is dissolved in boiling alcohol; on cooling, reddish-yellow or brown crystals are deposited; these are chloride of isatin, with a little bichloride of isatin and resin: by repeated crystallizations it may be obtained pure. It is inodorous, bitter, may be heated to 160° C. without decomposition, above that temperature it is partly decomposed and partly sublimed; 100 parts of alcohol of 0·83 dissolve 0·453 parts of it. It is neutral, not altered by hydrochloric or sulphuric acids; is decomposed by nitric acid. Formula $C^{16}H^{4}NClO^{3}$. Treated with caustic potassa it is dissolved with a yellow colour; it takes up an atom of water and forms chlorisatinic acid. The potash salt crystallizes easily, is purified by solution in alcohol, &c.; the acid cannot be separated when precipitated by an acid out of the potash salt; it decomposes and forms chloride of isatin, which body may in this manner be obtained perfectly pure. Potash salt explodes. Formula $C^{16}H^{5}NClO^{4}.\dot{K}$. Silver salt is $C^{16}H^{5}NClO^{4}.\dot{A}g$. There are two salts with baryta, one with one atom of water, the other with three. The lead salt is at first yellow and gelatinous, but becomes flocculent and of a splendid red colour. The copper salt is at first yellowish-brown, but becomes granular and blood-red, &c., &c., &c. Bichloride of isatin remains in the alcoholic solution out of which the chloride has been obtained; out of this it may be obtained: of course the latter crystallizations are the freest from the protochloride. These two bodies are extremely similar; the bichloride differs from the other in being more soluble in alcohol, and the bichlorisatinate of potash gives with lead salts a yellow precipitate, which does *not* become red like the chlorisatinate. Bichloride of isatin is, when obtained from its aqueous solution, a yellowish-red granular powder; out of alcohol it crystallizes in red shining needles and tables; sometimes four-sided prisms may be recognised; it may be partly sublimed, but the greater part is decomposed; more soluble in water than the protochloride. 100 parts alcohol of 0·83 dissolve at 14° 3·40 parts. Formula

$C^{16} H^{4} N Cl^{2} O^{3}$. Treated with potash it forms a bichlorisatinic acid, which may be precipitated from its solution in potash by means of an acid, in form of a yellow powder; this acid is tolerably soluble in water; it is easily decomposed; its aqueous solution decomposes at 60°, or even in the cold; it contains one atom water of crystallization. (*Journal für Praktische Chemie.* Erdmann and Marchand, xix. 321).

The potash salt crystallizes in needles and leaves. Formula $C^{16} H^{5} N Cl^{2} O^{4} . \dot{K}$; the crystallized salt contains two atoms of water. The baryta salt crystallizes in the same form, and contains two atoms of water. The copper salt is at first of the colour of hydrated oxide of iron, but becomes carmine red and granular; when pressed assumes a golden lustre. The silver salt is partly soluble in water, and may be crystallized; it is anhydrous.

The more of the resinous substance is formed the more chloride of indopten is produced; it is not formed by the action of chlorine on the proto- or bichloride of isatin. The resin examined was not quite pure, for the product formed by the action of chlorine on indigo-red was always mixed with it; it is obtained in small quantities. The formula appears to be $C^{20} H^{8} Cl^{2} N_{\frac{1}{2}} O^{5}$, or $C^{40} H^{16} Cl^{4} N O^{10}$.

It is probable that the ammonia resin, and chloride of indopten are unimportant and variable products.

By the action of bromine similar substances are produced, bromide of indopten; and by potassa, bromindatmit and bromindoptenic acid. They have apparently the same formula as the chlorine compounds; also bromide of isatin and bromisatinic acid, bibromide of isatin and bibromisatinic acid: they have the same composition, and are in other respects very similar to the chlorine compounds.

In a second paper Professor Erdmann promises to make known the action of certain reagents on the bodies described above.

Formic æther may be prepared, according to Wœhler, by distilling a mixture of ten parts of starch, thirty-seven parts finely powdered binoxide of manganese, thirty parts of sulphuric acid, fifteen parts of water, and fifteen of mere alcohol. In the product of the distillation chloride of calcium is dissolved, and the æther distilled off in a water bath; from the solution of chloride of calcium in the æther, small yellow crystals are sometimes deposited: they have not been further examined. (*Annalen der Chemie und Pharmacie*, xxxv. 238.)

According to Wœhler telluret of ethyl may be formed by distilling baryto-sulphate of ethyl oxide (sulphovinate of baryta) with telluret of sodium; the telluret, as obtained by fusing tellurium or telluret of bismuth with carbonate of soda and charcoal, is thrown into the baryta solution in a solid state. (*Poggendorff's Annalen*, l. 404.)

The æther is a yellowish-red fluid, like bromine, heavier than water, in which it is but little soluble; has a strong disagreeable smell, similar to sulphuret of ethyl and to telluretted hydrogen; appears to be poisonous; boils under 100°; inflammable, burns with a white flame with blue margin, in the air changes into tellurous acid. When acted on by the nitric and hydrochloric acids, an oily body is formed: contains 68·75 per cent. tellurium; it should contain 68·53. Formula $C^4 H$ Te.

Mylius has found that *Helix pomatia*, *nemoralis* and *hortensis* (but no species of *Limnæa* or *Planorbis*) contain pure uric acid in a free state; it occurs in a glandular organ situated immediately beneath the shell,—which is therefore without doubt the urinary vesicle,—secreted in a solid form, so that it shines through the covering membrane. To obtain it, it is merely necessary to cut open the organ and squeeze out the white paste into a glass. When a sufficient quantity has been collected, it is shaken frequently with water, by which the slime is suspended and can be poured off, while the uric acid is deposited at the bottom of the vessel. This simple and mechanical operation suffices to obtain a perfectly pure product: it has a pulverescent appearance, and contains no crystalline particles. The quantity obtained from each garden snail amounts to about one and three-quarter grains. (*Journal für praktische Chemie*, xx. p. 509.)

Brucin is considered by Dr. Fuss of Schönebeck to be merely a mixture of strychnia with a resin, from which it may be separated by a peculiar method. Erdmann has made some experiments with the products obtained by Dr. Fuss. (*Journal für praktische Chemie*, xix. 510.)

For preparing the chlorate of barytes, Duflos dissolves thirteen and a half parts of chlorate of soda in double the quantity of water, to which he adds a solution of ten parts of tartaric acid in as much water, and pours the whole into a glass containing a double volume of alcohol. It is then left to stand for twenty-four hours, filtered, the acid liquid neutralized with pure carbonate of barytes, which has been previously mixed with water to a milk, the spirit left to evaporate, then filtrated anew and evaporated to crystallization. The precipitate effected by the alcohol is the bitartrate of soda, which may again be employed for preparing chlorate of soda by adding it to a hot solution of sixteen parts of chlorate of potash in four times the quantity of water, leaving the mixture to cool, filtrating and evaporating the solution to forty parts. (*Arch. der Pharm.*, xxiii. p. 306.)

After continued rain, a heap of coals which lay exposed to the weather was observed to smoke, and to be on fire in the interior; when separated it was found in the middle to be in full combustion. In the neighbourhood of this part the coals were half-roasted, forming an almost compact mass, and between them was found a bright yellow, soft, saltlike substance. This proved on examination to be sulphate of magnesia; the ashes of coals contain magnesia, the burning pyrites afforded the sulphuric acid. Dr. Mohr compares it to the formation of cyanide of potassium at the Clyde iron-works*. How the sulphate of magnesia became collected into masses is not so easily explained. (*Annalen der Pharmacie*, xxxv. 239.)

Kersten has employed with advantage a mixture of hydrochloric acid and finely powdered chlorate of potash, added in small portions, as proposed by Berzelius for oxidizing and dissolving metals and metallic sulphurets, even for the analysis of the native metallic sulphurets, the sulphur being quickly and easily converted into sulphuric acid. M. Kersten, moreover, observed that finely powdered iron pyrites was perfectly decomposed by boiling with chlorate of potash, without any addition of acid, the sulphur being converted into sulphuric acid, and the iron remaining undissolved in the form of peroxide. (*Berzelius's Jahresbericht*, xix. p. 288.)

The difficulty of obtaining the hyposulphite of soda in good crystals, according to the usual methods, has induced C. F. Capaun to make known the following modified plan, by which in the shortest time considerable quantities of the salt can be prepared, and of a kind which leaves nothing further to be desired. A solution of caustic soda is boiled with sulphur so long as any of this is dissolved, the liquid poured from the undissolved sulphur put aside to cool, and then a current of sulphurous acid gas passed through it, until a test filtered from the separated sulphur has still a wine-yellow colour—it contains therefore still undecomposed sulphuret of sodium,—and by no means appears colourless. The liquid is now filtered and evaporated in a porcelain basin over a

---

[* Described by Dr. Clark in L. & E. Phil. Mag. vol. x. p. 329.—Edit.]

quick fire to the consistency of a syrup. Although during evaporation the atmospheric air is not excluded, there is no fear of the hyposulphite of soda being more highly oxidized, as the oxygen of the atmosphere must first exert its oxidizing action on the sulphuret of sodium. The liquid thus evaporated is filtered, if necessary, and mixed with half its volume of alcohol and well shaken; in a few minutes the liquid separates into two layers; the upper or alcoholic one, which has taken up all the sulphuret, is of a gold-yellow colour, the lower one containing the hyposulphite of soda dissolved in water, colourless. The vessel is now placed aside, that the hyposulphite may crystallize under the covering of the alcoholic solution of the sulphuret, which happens in the course of twelve hours. The crystals obtained, when quantities not too small have been operated with, are of considerable size, and without any yellow tinge; and when dissolved in water and treated with an acid, let fall a considerable quantity of sulphur with disengagement of sulphurous acid. (*Journal für praktische Chemie*, xxi. pp. 310–313.)

Marchand prepares the dioxide of copper in the following manner: very thick wires of pure Russian copper were brought in a Gay-Lussac's furnace immediately to a violent white heat, which having continued half an hour, was diminished to a dark red heat: the oxide was taken out after some hours; it formed black crystalline masses, which were in part hollow, and gave a beautiful purple-red powder. In the greater part there was still metallic wire inside, around which the dioxide had arranged itself in a crystalline state. Another time the dark red heat was maintained during eight or nine hours. Both products were reduced by hydrogen; the first portion contained 88·6 copper and 11·4 oxygen, the second 88·65 Cn. and 11·35 O. (*Journal für praktische Chemie*, xx. p. 505.)

[To be continued.]

XXXIX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xvii. p. 542.]

Feb. 26, 1840. (*Continued.*) A PAPER was then read, "On the characters of the fossil trees lately discovered near Manchester, on the line of the Manchester and Bolton railway; and on the formation of Coal by gradual subsidence;" by John Eddowes Bowman, Esq., F.L.S., communicated by the President.

The paper commences with a few preliminary remarks on the theory of repeated subsidences of the land during the carboniferous æra; and on the drift theory, the author being of opinion that the former receives much support from the phænomena presented by the fossil trees found near Manchester, and that it affords in return great assistance in explaining the peculiarities of their position. Mr. Bowman does not deny that plants may have been carried into the water from neighbouring lands, as in the instances of fern-fronds and other remains scattered through the sandstones and shales; but he conceives it is difficult to understand whence the vast masses of vegetables necessary to form thick seams of coal could have been derived, if drifted'; and how they could have been sunk to the bottom, without being intermixed with the earthy sediment which was slowly deposited upon them. He is of opinion also, that without a superincumbent layer of mud or sand, to retain the hydrogen during the process of bituminization, ordinary caking coal could not have been formed. Another difficulty, connected with the drift theory, Mr. Bowman says, is the uniformity of the distribution of the vegetable matter, throughout such great areas as those occupied by the seams of coal, extending in the instance of the lower main seam of the great northern coal field, over at least 200 square miles; and in that of a thin seam below the gannister, or rabbit coal, in a linear direction of thirty-five miles from Whaley Bridge to Blackburn. On the contrary, he believes, that it is much more rational to suppose, that the coal has been formed from plants, which grew on the areas now occupied by the seams,—that each successive race of vegetation was gradually submerged beneath the level of the water, and covered up by sediment, which accumulated till it formed another dry surface for the growth of another series of trees and plants,—and that these submergences and accumulations took place as many times as there are seams of coal. He also explains the thinning out of the seams and other strata of the coal measures, by irregularities in the mode or extent of the depressions.

Mr. Bowman then proceeds to the examination of the phænomena presented by the fossil trees discovered on the line of the Manchester and Bolton railway, and described by Mr. Hawkshaw in his paper read on the 5th of June, 1839, (see L. & E. Phil. Mag. vol. xv. p. 539.) and in the preceding communication (see vol. xvii. p. 541.): it will be necessary to notice therefore only those points which did not claim that gentleman's more particular attention. Mr. Hawkshaw describes generally the markings on the internal casts of the trees; but as it is difficult to convey a correct notion of their waved and anastomosing characters either verbally or by reduced drawings, Mr. Bowman applied paper to the surface of the stems and carefully traced the grooves or furrows by following them exactly with an instrument. The only indications of scars, which he could find after a long and close search, were at one point near the base of the largest tree, and though indistinct, his practised eye recognized them to be those of a Sigillaria. He detected also in some parts, on the ribs of the same tree, the fine wavy lines so often visible on decorticated specimens of that family. In describing the second tree, he alludes to a deep wedge-shaped rift on the south-east side, which had been coated with coal, and is strongly marked with wavy lines, like those on the surface of the alburnum of a gnarled oak. On the fifth tree, he discovered a longitudinal concavity on the north side, and he states that it resembles the impression which would be left in a dicotyledonous tree, by the pressure of a parasitic plant. The characters of the roots are also detailed at considerable length, particularly their mode of bifurcation, and position with respect to the horizon.

From a careful consideration of the phænomena presented by the fossils, Mr. Bowman is convinced that they stand where they originally flourished; that they were not succulent, but dicotyledonous, hard-wooded forest trees; and that their gigantic roots were manifestly adapted for taking firm hold of the soil, and in conjunction with the swollen base of the trunks to support a solid tree of large dimensions with a spreading top.

Towards the close of 1838, in forming the railway tunnel at Claycross, five miles south of Chesterfield, a number of fossil trees were found, standing at right angles to the plane of the strata. The tunnel passes through the middle portion of the Derbyshire coal measures, which there dip about 8° to a little north of east. The bases of the trees rested upon a seam of coal fifteen inches thick. The exterior of the stems consisted of a thin film of bright coal, furrowed and marked like the *Sigillaria reniformis*; and the interior consisted of a fine-grained sandstone. Mr. Conway, who supplied Mr. Bowman with an account of the discovery, infers, from the information which he obtained, that there must have been at least forty trees found, and judging by the area excavated, he is of opinion that they could not have stood more than three or four feet apart. There were no traces of roots, the stems disappearing at the point of contact with the coal. Several specimens of *Stigmaria ficoides* were also noticed by Mr. Conway, lying horizontally, and about three feet in length.

With reference to fossil trees in general, and especially to those near Manchester, Mr. Bowman proceeds to show still further; 1st, that they were solid, hard-wooded, timber trees, in opposition to the common opinion that they were soft or hollow; 2nd, that they originally grew and died where they have been found, and consequently were not drifted from distant lands; and, 3rd, that they became hollow, by the decay of their wood, from natural causes, similar to those still in operation in tropical climates, and were afterwards filled with inorganic matter, precipitated from water.

1. In stating his reasons for believing that the coal measures' casts were solid timber trees, Mr. Bowman alludes to the rifting of the bark of modern forest trees, in consequence of the expansion caused by the annual addition of a layer of wood between the bark and the alburnum; and to the thickening or swelling of the base of the trunk and main roots, and the apparent lifting up of the latter out of the soil, in old trees, by the greater annual increase of the upper part or that nearest to light and heat. These phænomena in vegetation were illustrated by a diagram, which exhibited the form of the base of the stem and the root of a sapling, and of a full-grown tree. The author, in applying these characters to the fossils of the Manchester and Bolton railway, alludes to the irregular, longitudinal and disconti-

nuous or anastomosing furrows on their surface, to the swelling out at the base of their stems, and to the divergence as well as the angle of dip or downward direction of their roots. These characters, he says, are not observable in soft monocotyledonous trees, their stems never expanding laterally, and being as thick when only a few years old and a foot high, as when they attain the height of 60 or 100 feet. Their roots also, instead of being massive and forking, generally present a dense assemblage of straight succulent fibres, like those of an onion or hyacinth. Analogy, therefore, as far as outward shape and habit are concerned, he adds, is strongly in favour of the fossils having been solid timber trees.

Mr. Bowman then combats the view, generally entertained, that fossil stems with perpendicular furrows, as in the Sigillaria, were succulent or hollow plants*. He states, that good specimens of decorticated Sigillariæ exhibit fine straight, and curled or gnarled striæ, similar to those on the alburnum of many modern forest trees; and that this character, in conjunction with others, renders it almost certain, that the fossils had a separate bark,—a feature which is considered in vegetable physiology to be a proof of a woody structure. He also alludes to the existence in many of the decorticated parts of these fossil trees of little prominences like those in barked timber; likewise to the scars left by the disarticulation of leaves; and he accounts for the general absence of the latter on large and old trunks, by their having been obliterated, in consequence of irregular expansion from the deposition of new layers of wood: he notices moreover the absence in small Sigillariæ of the irregular furrows observed on large specimens, and due in his opinion to the unequal expansion by the addition of new layers of wood. In support of these proofs of the original solid nature of the trees, Mr. Bowman exhibited polished slices mounted upon glass of portions of a similar fossil tree discovered in sinking a shaft 300 or 400 yards N.W. of those found on the line of the railway. The slices were made from a portion which exhibited within the carbonized bark, a patch browner, heavier, and more compact than the rest. In these slices, made under Mr. R. Brown's direction, that gentleman discovered in the transverse section, the uniformity of vascularity which is evidence of coniferous structure; and in the longitudinal section parallel to the medullary rays, the existence of these rays. The slices therefore exhibit proofs of dicotyledonous structure, and considerable probability of that structure being coniferous. The important evidence however of coniferous structure deducible from discs in sections parallel to the rays, was not obtained, the vessels having apparently undergone some alteration.

2. With respect to the second point, that the trees grew and died

---

* Specimens of recent dicotyledonous wood from New Zealand, lent to the author by Mr. R. Brown, were exhibited on the table of the Meeting Room. They displayed both upon the bark and the naked wood, longitudinal ribs and intermediate furrows as regular as those on Sigillariæ; and therefore prove that these characters are not incompatible with a dicotyledonous structure.

on the spots where they are now found, and that they were not drifted from distant lands, Mr. Bowman says, the arguments in favour of the formation of beds of coal by a series of subsidences of the surface on which the vegetables that produced the coal grew, naturally lead to the inference that the trees associated with the coal also flourished on the same spots. In opposition to the opinion that trees would naturally float in an upright position in consequence of the greater specific gravity of the base and roots, he asserts, that the trees would maintain that position only as long as they floated, and that they would fall and lie prostrate when grounded on shoals or cast ashore. He agrees with Mr. Hawkshaw in the opinion, that it is more difficult to account for a number of great trunks being deposited in the position of the fossils in the Manchester railway, than to imagine that they grew on the surface of the bed on which they now stand. Their position on a bed of coal is another proof, Mr. Bowman conceives, that the trees were not drifted, for if they had been transported by currents of water they might equally have been imbedded in the alternating shales or sandstones. If beds of coal are the accumulated remains of many generations of a luxuriant vegetation, the rich compost thus formed, Mr. Bowman argues, would be well suited for the growth of trees. Again, the angle at which the roots of the fossil trees, particularly of that distinguished by him as No. 2, dip towards the bed of coal, is considered by the author evidence of the trees being in their original position, because, had they been drifted, the roots would have been bent upwards, by the downward pressure of the trunk, when the water had left them. The appearance of the roots being cut off, where in contact with the coal, he is of opinion, may be explained by the fermentative process having dissolved the vegetable texture below the surface. The stems and upper portions of the roots standing above the coal, he explains by reference to similar phænomena in peat marshes, in which the bases of the trunks of ancient forest trees stand with the roots exposed, owing to the shrinking of the surrounding peat.

3. In discussing the third point, that the trees became hollow from the decay of their wood, and were filled with sedimentary matter after their immersion, Mr. Bowman refers to the facts recorded in the preceding paper by Mr. Hawkshaw (see vol. xvii. p. 541); and in confirmation of them states, that Mr. Schomburgk during his four years' travels in Surinam repeatedly observed similar phænomena. Mr. Bowman then proceeds to explain the processes by which he conceives the fossil trees were gradually submerged—their upper branches torn off—their interior removed by natural decay,—their bark converted into coal,—their central cavities filled with sediment; and the whole buried beneath the stratum of shale or sandstone in which the trees were discovered. He afterwards applies the phænomena which he believes these processes produced to the condition and position of the trees and the arrangement of the surrounding sedimentary matter. The author then enters into the inquiries, 1st, the time which the trees may have required to attain their dimensions; and consequently the minimum of years requisite for the accu-

mulation of the vegetable matter; and, 2ndly, what thickness of vegetable matter was necessary to form the stratum of coal nine inches thick, over which the trees stand. Mr. Schomburgk is of opinion that a dicotyledonous tree which would require in temperate climates one hundred years to attain a certain diameter, would arrive at the same dimensions within the tropics in sixty or eighty years. The largest of the fossil trees forming the immediate subject of the paper is equal in circumference to an oak of 130 years growth in this climate, or about 100 for a climate equal in temperature to that of the tropics. Allowing therefore that some time elapsed after the commencement of vegetation on the surface of the then dry land before the trees began to grow, Mr. Bowman infers, that 100 years must be the minimum of time which would be required for the production of the vegetable matter out of which the nine inches of coal were produced. With respect to the depth of the stratum of vegetable matter from which it was formed, Mr. Bowman takes for his data, the thickness of the bed of coal, nine inches; the distance between the top of the seam and the bottom of the trunk under the arch formed by the roots, fifteen inches; and for the distance to the surface of the ground, four inches, or in all twenty-eight inches; whereby he infers that the thickness of the solid coal is equal to about one-third that of the vegetable matter out of which it was produced.

A paper was lastly read, "On the character of the beds of clay lying immediately below the coal seams of South Wales; and on the occurrence of coal-boulders in the Pennant grit of that district;" by William Edmond Logan, Esq., F.G.S.

Immediately below every regular seam of coal, in South Wales, (and nearly 100 are known to exist) is constantly found a bed of clay, varying in thickness from six inches to more than ten feet, and called the underclay, undercliff, understone, pouncer, or bottom stone. It is so well known to the collier, that he considers it an essential accompaniment of the coal; and only where it ceases, does he give up his expectation of finding coal. Seams which have thinned out in one portion of a work, have been recovered in another by following this bed.

The underclay is always more or less argillaceous, but it is never without a considerable admixture of sand; and in most cases it yields a very good fire-clay; which, though generally tough when freshly cut, yet crumbles on exposure into a mass of a grey colour. Occasionally it is quite black, in consequence of the presence of carbonaceous matter, and it then sometimes resists the effects of the weather. Under a part of the lowest seams of coal between Swansea and the Bury river, it is a hard, durable, finely grained, siliceous stone. It is however by containing innumerable specimens of *Stigmaria ficoides*, that these beds are most strongly marked, other portions of the coal measures presenting the same mineral composition. The stems of the Stigmaria, which are usually of considerable length, always lie parallel to the plane of the bed, and nearer to the top than the bottom; and they are occasionally compressed, their diameter varying from two to six inches. Their long slender processes, covered with a pellicle of carbonaceous matter, form an entangled mass, and traverse the beds in every direction, vertically, horizontally, and obliquely; but Mr. Logan has never been able to trace them to their termination, though he has followed single processes for considerable distances. Portions of the stem of the Stigmaria are found in other parts of the coal measures, but it is only in the underclay that the fibrous processes are attached to the stem or associated with it. Mr. Logan, however, states, that if such specimens exist in other strata, they are not so likely to be exposed, as those beds are less worked than the underclay.

In some instances, the Stigmaria, with its processes, is found equally abundant in the roof as in the floor of a coal pit, but in such cases the roof has been ascertained to be the underclay of an immediately overlying bed of coal.

Mr. Logan then quotes at length, Steinhauer's account of the Stigmaria, as it gives the best explanation he has seen of the external botanical character of the plant, as well as of its position in the beds in which it occurs; the only point in which his experience induces him to differ from Steinhauer, being the vertical extent to which the fibres range. Mr. Logan has never traced them in that direction more than seven or eight feet from the stem, though he admits they may have an horizontal range of twenty or more feet. (American Phil. Trans. New Series, vol. i. p. 265, 1818.)

When it is considered, that over so considerable an area as the coal field of South Wales, not a seam has been discovered without an underclay, abounding in Stigmaria, Mr. Logan says, it is impossible to avoid the inference, that there is some essential and necessary connexion between the existence of the Stigmaria and the production of the coal. To account for their unfailing combination by drift, seems to him unsatisfactory; but whatever may be the mutual dependence of the phænomena, he is of opinion, that it affords reasonable grounds to suppose, that the *Stigmaria ficoides* is the plant to which may be mainly ascribed the vast stores of fossil fuel.

In the second part of the paper, Mr. Logan gives an account of boulders or rounded fragments of coal, contained in the coal measures themselves.

The thickness of the coal deposit of South Wales, he says is equal in the deepest part to 12,000 feet, and that consequently a long period must have been required for the accumulation of the materials, and that any fact which may assist in ascertaining its length, cannot fail to possess some interest. The occurrence of these boulders he is of opinion bears upon the subject.

From a layer of indurated clay, two inches thick, lying on the top of a seam of common bituminous coal, and covered by hard sandstone, at Penclawdd on the Bury river, he obtained, in the spring of 1839, a worn, rounded mass of cannel coal, six inches long, four inches wide, and two inches thick. The discovery of this singular specimen having excited attention to the subject, it was ascertained that in the quarries of the enormous mass of sandstone forming

Cilfay hill and the Town-hill range from Swansea to the Bury river, there occur many irregular conglomerate beds, formed of innumerable pebbles and small boulders of coal, sometimes four inches in diameter, mingled with sand and pebbles of ironstone; and there have been also found in them small boulders of granite and mica-slate. Many impressions, coated with coal, of Sigillariæ and other plants, occur in the mass; and the difference of age between this coal and that of the pebbles, he says, is beautifully illustrated in numerous cases, where the softer coal of the plants has been pressed down upon the harder coal of a layer of the pebbles, by the cleavage of the former, however distorted the plant may be, presenting an uniform parallelism, while the cleavage of the coal forming the pebbles is parallel with the sides of the pebbles, which are inclined in all possible directions.

The pebbles consist principally of the common bituminous coal of the neighbourhood, but two have been found composed of cannel coal, the only seams of which, existing in the lower measures, occur about 2000 feet below the conglomerate bed.

The Cilfay sandstones and the measures at Penclawdd, in which the first-mentioned pebble was found, form part of the Pennant grit; and there is reason to believe, that throughout the whole of this great mass of sandstone, about 3000 feet thick, occasional beds of coal pebbles are to be met with: but Mr. Logan has not seen any associated with the lower measures.

March 11, 1840.—A paper was first read, "On the Rocks which form the West shore of the Bay of Loch Ryan in Wigtonshire, N.B.," by John Carrick Moore, Esq. F.G.S.

The peninsula of the Ryans extends about thirty miles from N. to S., and is about seven miles across at its greatest breadth, or from Stranraer to Port Patrick. In the geological maps of M. Necker, Dr. Macculloch, and Mr. John Phillips, it is coloured as part of the great graywacke chain, stretching from the Irish sea to St. Abb's Head; and the chief part of the rocks composing the peninsula, Mr. Moore says, undoubtedly belongs to that epoch; but he has ascertained from an examination of the district during the summer of 1839, that others of a more recent date also exist.

The portion of the peninsula particularly described by the author, extends about eleven miles from north to south, and about five from east to west; and is bounded on the W. and N. by the Irish sea, and on the E. by the Bay of Loch Ryan. The formations of which it consists are—1. Graywacke, 2. Trap rocks, 3. Coal measures, and, 4. a red breccia.

1. The graywacke constitutes the greater part of the district, the beds being nearly vertical and the prevailing strike E. by N. At the northern extremity, near the Corsewall Lighthouse, are beds of conglomerate composed of rounded masses of granite, with pebbles of serpentine and other rocks. In the little bay of Sloughnagarry, at the most southern point, where the graywacke shows itself, Mr. Moore found in a slaty rock alternating with compact beds, an abundance of fossils, determined by Mr. Lyell to be graptolites.

2. The trap rocks occur at two points, one near the northern extremity of the peninsula, on the farm of Balscallock, constituting a dyke of amygdaloid greenstone which cuts through the graywacke and is lost in the sea; and the other is near Loch Connell, where a mass of greenstone extends in a westerly direction for nearly two miles. At both localities, the trap intersects the graywacke; but at neither point could the author find it in contact with the coal or overlying breccia.

3. Coal Measures. A deposit consisting of beds of red and white sandstones, clays and micaceous shale, similar to those of the coal-field of Ayr, has been long known to exist in the district, and has led to several fruitless researches for coal. The deposit may be traced for about nine miles, forming a narrow band parallel to Loch Ryan. The beds are in general moderately inclined to the E. or S.E. In a quarry on the farm of Clachan, Mr. Moore found remains of *Stigmaria ficoides*, and in another, on the farm of Challock, an abundance of Calamites.

4. Red Breccia. This rock extends from the bay of Sloughnagarry to the farm of Dumlae, a distance of eight miles, forming a ridge from 200 to 300 feet high, between the coal measures and the shore of Loch Ryan. It consists entirely of irregular fragments of graywacke cemented by a red clayey sand, but in some places it passes into laminæ of red sandstone. The beds are nearly horizontal or dip slightly to the S.E., and rest on the coal measures. As Mr. Moore did not detect any organic remains in the breccia, nor find any rock overlying it, he does not offer an opinion respecting the period of its formation.

A paper was afterwards read, "On the Siliceous Bodies of the Chalk, Greensand and Oolites;" by Mr. Bowerbank, F.G.S.

The author commences by stating, that naturalists and geologists have long considered the form of tuberous masses of flint found in the upper chalk to be due to alcyonia or sponges, but that he is not aware of this opinion having been proved to be correct. It was Professor Ehrenberg's observations on siliceous bodies which first induced him to obtain thin slices of flint with the intention of procuring specimens of Xanthidium. In the examination of these slices, he was struck with the frequent occurrence of patches of brown, reticulated tissue, spicula and Foraminifera, and he was induced to infer, that the patches of tissue were the remains of the organized body, possibly a sponge, to which the flint owed its form. With this belief, he commenced his inquiries by examining thin slices of flints obtained from various localities, and he found in all of them, a perfect accordance in the structure and proportion of reticulated tissue, in the number of spicula, and in the occurrence of Xanthidia and Foraminifera. The following are the general appearances which the slices of flint exhibit when mounted upon glass.

With a power of about 120 linear, the slice presents the appearance of a stratum of a turbid solution of decomposed vegetable or animal matter containing Foraminifera, spicula, Xanthidia, and frequently fragments of the brown tissue. In a specimen from North-fleet the mass of the spongeous portion exhibited numerous cylindrical contorted canals, which from their uniformity and minuteness of diameter, Mr. Bowerbank considered to be the incurrent canals of the sponge; and other orifices of greater diameter, to be the excurrent. Very frequently, when little of the reticulated substance of the sponge remains, its former presence, the author says, is indicated by the siliceous matter resembling a congeries of gelatinous globules, moulded by the tissue amid which it was deposited; and the globules, when traced to the edges of the patches of spongeous texture, were found to agree in size and form with the orifices of the supposed incurrent canals. In cases where no traces of the sponge can be detected, Mr. Bowerbank thinks, that the mode in which the spicula, Foraminifera and other extraneous matters are dispersed equally in all parts, and not precipitated to one portion of the flint, indicates that the organized tissue in which they were entangled, retained its form and texture sufficiently long to allow of the fossilization of these remains in their original places; and that the nature and position of these bodies strongly indicates the former spongeous nature of the flint.

When the chalk is carefully washed from the exterior of a flint, and a portion examined as an opake object with a power of about fifty linear, it exhibits a peculiar saccharine appearance, with deep circular excavations, having fragments of extraneous matters partly imbedded or adhering to them. If the surface be further cleansed by immersion in diluted muriatic acid, till effervescence ceases, spicula may be detected on the sides of the deep circular cavities; and if, again, a piece a quarter of an inch in diameter, presenting the roughest aspect, be examined under a power of 120 linear, illuminated by a Lieberkuhn, the surface, under favourable circumstances, will present a complex mass of small, contorted tubuli, occasionally furnished at the apex with a minute perforation.

The structure and other characters of the tabular flints are stated to accord perfectly with those of the nodular masses, except that the under surface has a still more marked spongeous aspect, and that spicula and Foraminifera are more abundant. The absence of any apparent base or point of attachment in the great mass of nodular chalk flints, the author says (considering them undoubtedly of spongeous origin), may be accounted for by supposing that the gemmule was originally attached to some minute fragment of a shell or other substance, and that its further development took place while recumbent on the mud or silt.

The perpendicular and oblique veins of flint between Brighton and Rottingdean, are reported to present exactly the same internal characters as the tabular and nodular flints, and to agree externally with the former. The occasional existence of a fissure filled with chalk, in the centre of the vertical layers, Mr. Bowerbank conceives, may indicate that the sponge had grown from the two sides of the crevices, but had not in all places been able to unite. The sides of these flint veins are not studded with Foraminifera in a manner similar to that of the tabular horizontal layers.

Mr. Bowerbank next examined the flint with which Echinites and shells of the chalk are often entirely or partially filled and enveloped, and he states, that the results were the same, both with reference to the exterior and the interior of the flint. In those cases in which the Echinite is only partially filled, he infers that the portion so occupied was originally a sponge, because its surface is uneven; for had the flint been deposited in an empty shell or Echinite, it would present an uniformly flat surface. Again, he states, that the projecting of the flint through the two openings of the Echinite, with an extension to a greater or less distance, is owing to the sponge having grown outwards through these orifices; and the envelopment of an organic body by a tubular mass of flint, he explains by reference to the habit of recent sponges to invest testacea or other marine bodies. In some cases, he has found minute but deep depressions on the surface of flints filling Galerites, and immediately opposite to the ambulacral pores; and he ascribes the origin of the depressions to streams of water drawn in through the orifices to supply the wants of the living sponge.

Mr. Bowerbank was afterwards induced to extend his examination to the flints which invest the zoophytic bodies of the Wiltshire chalk. By carefully cleaning the interior of some of these flints, he discovered spicula projecting from all parts, however different the character of the inclosed body; and the spicula appeared to have no reference to it, none of them being found on its surface. Under the microscope, the investing flint presented in every respect the same appearance as that exhibited on the lower surface of the tabular flints, having fragments of minute corals and small shells attached to the inner surface. A thin slice exhibited the usual organic contents of the common flint. He, therefore, infers that the tubular flint which incloses the zoophytes, owed its origin also to a sponge which invested the organic nucleus.

A comparison of the characters presented by the spongeous remains of the flint, with a collection of recent sponges, has induced Mr. Bowerbank to conclude that the fossils cannot be referred to any of the established divisions of existing sponges.

On examining the cherts of the greensand of Fovant in Wiltshire in the same manner, he found that the only differences between them and chalk flints, existed in the coarser texture of the spongeous fibre, the greater size of the interstices of the network, and the larger dimensions of the imbedded extraneous bodies. The cherty nodules of the upper greensand of Shaftesbury afforded similar appearances. A black, semi-transparent nodule, with an outer coat resembling agglutinated sand, was found under the microscope to contain numerous contorted canals of various sizes, and a considerable number of beautiful green spicula. Two chert casts of Spatangi from Shaftesbury afforded results analogous to those obtained from chalk Echinites.

Slices from a great variety of the greensand cherts of Lyme Regis presented characters which agreed with the cherts of Fovant. A specimen of flint from the Portland stone of Tisbury, and another from Portland, gave a greater quantity of cellular structure than any of the previously noticed cases, and the texture bore a greater affinity to that of the freshwater sponge, than is exhibited in the flints of the chalk or the cherts of the green sand.

With respect to the causes of the deposition of the flint, Mr. Bowerbank objects to the supposition, that it was influenced by the siliceous spicula of the sponges, because the flint is in no case limited or determined by their immediate presence, but is, in all instances, bounded by the extent of the animal matter of the sponge. He has frequently observed that the large excurrent canals in the chalk-flint spongites are not filled with silex, and that the spicula projecting into them have not the slightest incrustation of siliceous matter upon their surface; while on the contrary, wherever a single tube or a thin layer of tubes has been projected from the mass into the chalk, the silex has been attracted to it. He conceives also, that the retention of the spicula and extraneous matters in all parts of the flint, may be accounted for, by supposing that the animal matter was the attractive agent, acting equally throughout the whole body of the sponge. In support of his argument he adduces the siliceous shells of Blackdown, and the siliceous corals of the Tisbury oolite and the mountain limestone, which contain no spicula, and in which it cannot be supposed that previously existing siliceous matter was the attractive agent. Lastly, the pyritous fossils of the London, Kimmeridge, Oxford and other clays, are also mentioned as examples of animal and vegetable substances having exercised an attractive influence.

March 25, 1840.—A paper was first read "On the Age of the Limestones of South Devon;" by W. Lonsdale, F.G.S.

The object of this communication is to show the nature and limits of the author's claim to having been the first to infer from zoological evidence that the limestones of South Devon would prove to be of the age of the old red sandstone; and it was drawn up at the request of Mr. Murchison, in consequence of the subsequent adoption and extension of the proposed classification by Professor Sedgwick and that gentleman; and at the request likewise of Dr. Fitton, in consequence of the same views having been applied to some of the infra-carboniferous formations of Belgium and the Boulonnais. The paper commences with a summary of the opinions previously entertained respecting the age of the limestones. The authors quoted are, Woodward, 1722; Da Costa, Maton, Playfair, Berger, L. A. Necker, De Luc, T. Thomson, Kidd, W. Smith, Brande, W. Phillips, Hennah, Greenough, Sedgwick, W. Conybeare, J. J. Conybeare, Buckland, Dufrénoy, Elie de Beaumont, De la Beche, Prideaux, Boase, J. Phillips, Austen, Murchison, Bakewell and J. de Carle Sowerby.

By these geologists the limestones are placed in the primary, transition or graywacke and carboniferous series; Mr. Prideaux being the only author who ascribes them in part, and on mineral characters, to the old red sandstone; and Mr. J. Phillips, in his article on geology in the Encyclopædia Metropolitana, hesitating to place them in a definite position, in consequence of the resemblance of many of the shells to species found in the mountain limestone. Mr. De la Beche, in his memoir on Tor and Babacomb Bays, also states that the limestones of that district rest on old red sandstone; and in his Report on Cornwall and Devonshire (1839), he says, "that those who rely very exclusively on the character of organic remains would probably feel disposed to consider the Torbay and Plymouth beds as equivalent to some such rock as the old red sandstone." The author of the paper refrains from all reference to the memoirs of the Rev. David Williams and Mr. Weaver, because his attention is more particularly confined to the limestones of South Devon. In allusion to the diversity of opinions which have been entertained respecting these rocks, even on some occasions by the same geologist, he is of opinion that it must be ascribed to the want, at the time the memoirs were written, of that preponderating weight of evidence which enables the mind to rest steadily on its own decisions; and that if a better result be now attainable, it must be ascribed to the mass of evidence, which has been recently accumulated in various parts of the kingdom. Until the organic remains of the mountain limestone and Silurian system had been determined, the former overlying and the latter underlying the old red sandstone, and shown by Mr. Murchison to graduate regularly into that formation, and to contain perfectly distinct suites of fossils, it was impossible to determine the age of a series of beds, the fossils of which were in part new, and in part closely allied to carboniferous shells; and procured from a region but partially examined, without a base line, beset with faults, and traversed by igneous rocks.

Mr. Lonsdale then proceeds to show, what was the zoological evidence on which he ventured in December, 1837, to conclude that the South Devon limestones would prove to be of the age of the old red sandstone. Previously, he had examined in part the corals of the Silurian region and South Devon, and ascertained that some of the species are common to both; he had also examined with Mr. J. Sowerby, Mr. Hennah's valuable collection of fossils from the neighbourhood of Plymouth, and had become aware, from the decisions of Mr. Sowerby, that certain of the shells could with difficulty, if at all, be distinguished from mountain-limestone species; and that some were distinct. In December, 1837, he examined with Mr. Austen a portion of that gentleman's collection of Newton Bushell fossils, and though he ventured to differ from some of the identifications with mountain-limestone species pointed out to him, yet these shells agreed so much in aspect with testacea of the carboniferous fauna, that he could not doubt there was a connexion between the beds from which they had been obtained and the mountain limestone system: the same collection also proved that, associated with these shells were corals of Silurian species. He had also been informed by Mr. Austen, that in beds connected with the limestone, the *Calceola sandalina* had been found. It was therefore by combining the amount of the above evidence, the presence in the same strata of shells, identical, or nearly identical, with mountain-limestone species, of Silurian corals, the *Calceola sandalina*, and a numerous distinct testacea, that he suggested the South Devon limestones would prove to be of an age intermediate between the carboniferous and Silurian systems, and consequently of that of the old red sandstone. In alluding to Professor Sedgwick and Mr. Murchison's adoption of the suggestion in 1839, and their bold application of it to all the older sedimentary rocks of Devon and Cornwall, the author states, that the fullest testimony is borne in the papers, containing their present views of the structure of those counties, of the source whence the suggestion was derived.

Appended to the notice was a list of fossils, necessarily very incomplete, from the limited nature of the materials at the author's command. It consisted of sixty-three species; twelve considered common to the Carboniferous and Devonian limestones, forty-two peculiar to the Devonian strata; and nine, seven of which are corals, common to the Devonian and Silurian formations; doubts were, however, entertained respecting the identification of the two species of shells. The author then observes,—should it be urged that it was unjustifiable to assume, from organic remains alone, the age of the Devonshire limestone, it may be replied, that in a district of which little in 1837 was positively known, which is cut off by the granite of Dartmoor from the only base line of the country, the culm measures of central Devon, proved in 1836 by Prof. Sedgwick and Mr. Murchison to be the representative of the true coal measures, organic remains were the only test by which the age of strata so situated could be determined; and in support of his argument, he advanced the recent establishment in Cutch and the Desert to the east of it, from the examination of suites of fossils brought to England by Capt. Smee and Capt. Grant, and others procured by Colonel Pottinger at the request of Colonel Sykes, of a series of beds unquestionably of the age of the oolites of England, the fossils agreeing in their general characters with those of that geological epoch in this country, and being in many instances specifically undistinguishable. In this case, mineral characters and order of superposition would have been valueless guides, for the rocks are totally different in character from those of the same age in England; and there was no predetermined series of beds from which an order of superposition could be derived. Another instance was noticed of the value of organic remains, if rightly applied, in determining the relative age of a distant region, and in this case of one inaccessible to Europeans, in the Ammonites obtained from the Tartar side of the Himalayan mountains. These fossils prove the existence in that unexplored country, of rocks of the secondary epoch, by possessing that peculiar character in the sutures, which is not found in Ammonites of any other epoch; they are moreover accompanied by Belemnites.

In advocating the value of fossils, the author, however, begs it may be clearly understood, that he would not expunge from the geologist's consideration, the aid to be derived from order of superposition, and under a right control, from the use of mineral composition and lithological structure; and he would advise the observer not to depend upon his own limited sources of knowledge, but to seek the aid of the philosophical zoologist, who can teach him to reason justly on the distribution of animal life,—the accidents to which it is liable,—the changes which such accidents may produce, or the means provided by nature to resist them,—and on the effects which a permanent alteration in the inhabiting medium may work in the form and size of a shell or coral.

Of the importance of organic remains in identifying districts less widely separated, the two following instances were noticed. In M. Dumont's work on the geology of the province of Liège, published in 1832*, and justly valued for unravelling the structure of a most intricate country, the strata immediately beneath the mountain limestone are divided into three systems, but without any definite comparison with the formations which underlie that deposit in England. At the meeting of the Geological Society of France, at Mezières, in September, 1835, Dr. Buckland proposed the following first comparison between the systems of M. Dumont and the subdivisions of the Silurian system established by Mr. Murchison:—

Système calcareux supérieur......	Mountain limestone. (Old red sandstone wanting.)
Syst. quartzo-schisteux supérieur..	The Ludlow rocks.
Syst. calcareux inférieur........	The Dudley and Plymouth limestone.
Syst. quartzo-schisteux inférieur..	The Caradoc sandstone. (Builth and Llandeilo flags wanting.)
Terrain Ardoisier.	

This comparison was principally founded on the resemblance of the corals with those obtained at Dudley and Wenlock. M. Constant Prevost pointed out the resemblance of the *calcaire bleu* of the *système calcareux inférieur* of M. Dumont with the Plymouth limestone, and of the marble of Heer, subordinate to the *système quartzo-schisteux supérieur*, with the limestones of Babacombe. Mr. Greenough, however, doubted the identity of the Plymouth and Dudley limestones, and he stated that he had remarked the total absence of the Dudley Trilobites in the *système calcareux inférieur*. During the Mezières meeting, Dr. Buckland identified certain beds beneath the mountain limestone near Namur, Dinant, and Huy, and at Engis, with the old red sandstone†; and at an ordinary meeting of the Geological Society of France, in December, 1837, M. Rozet repeated his belief, that the old red sandstone is well developed between Dinant and Namur; and M. Constant Prevost stated, that he had also during the Mezières meeting, determined its existence in those districts. In 1838, M. Dumont visited England for the purpose of examining the

---

[* An Abstract of M. Dumont's memoir appeared in L. & E. Phil. Mag. vol. xv.—Edit.]

† In the "Outlines of England and Wales" (1822), the Rev. W. D. Conybeare places all the Belgian beds between the carboniferous limestone and the transition slates in the old red sandstone.—Note, 468.

Silurian region; and on his return to Belgium, he laid before the Royal Academy of Bruxelles a table, differing from that of Dr. Buckland only in drawing more closely the terms of comparison, and in identifying the two upper divisions of the Terrain Ardoisier with the Cambrian system. He stated also, in a report which accompanied the table, that the old red sandstone was most probably wanting in Belgium, or, if it exist, that it must be considered as a great development of the superior part of the Upper Ludlow Rock. In M. Dumont's work, before mentioned, lists are given of the fossils from each system; and on examining them, for the purpose of determining how far the comparison of the Belgian and Silurian systems could be established by organic remains, the author of this notice ascertained, that out of twenty-two species, only four could be considered as peculiar to the Silurian system; and of these he believes two may be erroneous identifications; that five species are common to the Belgian beds and the mountain limestone, and thirteen to the Belgian and Devonian systems. These lists, Mr. Lonsdale states, are small, but, he adds, they bear internal evidence of having been carefully drawn up without any preconceived theory; and he conceives that they afford sufficient proof that the beds from which they were obtained do not belong to the Silurian system, but partake of the same intermediate character as the Devonian limestones. The other case, alluded to in the paper, refers to the older beds of the Bas Boulonnais. These strata were identified by M. de Verneuil in 1838, with the Silurian series of England, particularly a bed of limestone containing corals and other fossils with the Wenlock limestone; and M. Dumont, who examined the country with M. de Verneuil, states in his report to the Bruxelles Academy, that his four systems occur in the Boulonnais. The above bed of limestone, M. Rozet had also, in 1828, placed below the old red sandstone; but in a subsequent memoir, published in the *Annales des Sciences Naturelles* (xix. p. 145. 1830), he assigns it to the old red sandstone. At the Meeting of the Geological Society of France at Boulogne, in Sept. 1839, and at which some of the Fellows of the Geological Society of London assisted, the identification of the Boulonnais beds with the Silurian system was fully admitted. When, however, doubts were recently thrown out respecting the age of the formations in the Liège districts on account of the nature of their fossils, Mr. Murchison, who was present at the Boulogne Meeting, stated to the author of this notice, that if the Liège country had been wrongly identified, the older beds of the Boulonnais had been wrongly identified also. To determine the question, as far as fossils would assist, Mr. Murchison procured, by the kind assistance of M. Dutertre Yvart, a collection of specimens in the Museum at Boulogne. An examination of these specimens with published lists, proved that the inference was just, and that there exists in the Bas Boulonnais, the same mixed assemblage of mountain limestone, Silurian and Devonian, or peculiar fossils, as in the province of Liège and in Devonshire.

A note "On the Bone Caves of Devonshire," by R. A. C. Austen, Esq., F.G.S., was then read.

Mr. Austen commences by noticing the two theories which have been proposed to account for the introduction of the bones of animals into caves—one, which accounts for their presence on the belief that they were dragged in by hyænas or bears inhabiting the caves; the other, which supposes that the bones were drifted in by diluvial waters. He then proceeds to give his own explanation of the phænomena presented by Kent's Cave and Yealmpton Cavern; but he says it is not his intention, by doing so, to propose a general theory for ossiferous caves.

In the Devonshire caverns, mentioned above, remains of the Elephant, Hog, Rhinoceros, Horse, Ox, Bear, Hyæna, and Cat, generally bearing marks of teeth, are intermingled. With reference to the means by which they were collected, Mr. Austen observes, the habits of the Hyæna are now better known than formerly, and there is little in them to warrant the conclusion that the fossil bones were collected by that animal. He says, on the authority of Cuvier, that hyænas "se tiennent solitaires dans les parties montagneuses," (last Edit. Oss. Foss.) least of all do they inhabit caves; that they have not the courage to attack any formidable animal, preferring the putrid flesh and bones, which they find in their nightly prowlings: that they never drag away their prey, but devour it greedily on the spot: and he adds, on the authority of M. Marcel de Serres, who has observed the habits of the Hyæna in Africa, "that its gluttony is equalled only by its cowardice."

The Lion, on the other hand, seeks solely for living prey, which it prostrates at one spring, and then conveys to its lair. The African lion has been known to carry off a bullock, and its constant abode is in chasms, caves, or on overhanging ledges of rock.

Mr. Austen is therefore induced to believe that the cavern bones were in the first instance the prey of the larger feline animals, and that during their absence the hyænas visited the caves to feed upon the fragments of the partially consumed prey; and in support of this view he quotes the passage from Johnson's Field Sports, given in the *Reliquiæ Diluvianæ* (p. 22): "they feed on small animals and carrion, and often come in for the prey left by tigers and leopards after their appetites have been satiated."

What the large feline animals were, Mr. Austen says, is not important, as they resemble each other in their habits. The remains of a Felis as large or larger than any now known, have been found in the Plymouth and Hutton caves, and the canine and molar figured by Dr. Buckland from Kirkdale, are said by Cuvier to differ in no respect from those of a lion. (Oss. Foss. IV. 151.)

The remains of a fossil lion have been also found in the caves of Gailenreuth, the province of Liège, Mialet and Jobertas (Dép. du Gard), Lunel-Viel, Joyeuse, Ardèche, Fouvent, Fausan (Dép. de l'Herault), and in Kent's Cavern.

It is known that the Lions of the present day will attack every one of the animals, the remains of which are found in Kent's Hole, and other caves; and if it should be urged that the most powerful lion could not carry off the bodies of the great Pachyderms, Mr. Austen says, that an examination of a very large proportion of the remains taken from Kent's Hole has proved that the bones and teeth of the Elephant belonged to young animals; and he quotes Dr. Buckland's statement, that the ten elephants' teeth discovered in Kirkdale Cave belonged to extremely young animals. (Rel. Dil. p. 18.)

The conclusions, therefore, which Mr. Austen wishes to draw are, 1st, that the carcases were dragged into the bone caves by powerful feline animals; and 2ndly, that hyænas picked and gnawed the bones after those animals had satisfied their hunger, and while they were absent. He also objects to the belief that some of the German caves are filled with the animal matter of countless generations of bears, as the decomposition of one carcase, he says, would have driven the living bears from the cave; but he believes the prevailing fossil remains in each locality indicate only what animals were most abundant in the district, and consequently most frequently fell a prey to the powerful Felidæ: thus in the low grounds about Yealmpton, Kent's Hole or Kirkdale, herbivora may have been most abundant, and bears in the region of the Hartz[*].

April 8th.—A paper was first read, "On the Great Fault, called the Horse, in the Forest of Dean Coal Field;" by John Buddle, Esq., F.G.S.

The term fault is used in this paper in the miner's signification, or for any interruption in the regular deposition or range of a bed. The Horse Fault, therefore, is not a displacement of one part of the stratum by a dislocation, but a local thinning out of a bed of coal, and a substitution of sandstone for it.

The Horse has been traced in the Coleford High Delf seam, the 23rd in the descending series, or the 3rd from the bottom, and the only one in which it is clearly developed, for about two miles; and its known breadth varies from 170 to 340 yards. The only point at which it has been tunneled through in a transverse direction, is under Barn Hill enclosure, between Brixslade and Howler's Slade valleys, and its width is there about 200 yards. The upper surface of the seam of coal, to a considerable distance on each side of the Horse, undulates considerably, producing depressions called "lows," and great varieties in the thickness of the bed; but the pavement composed of the ordinary argillaceous deposits, which accompany the seam throughout the basin, preserves nearly its ordinary regularity.

The roof of the seam consists of the strong sandstone which usually reposes upon the Coleford High Delf, but a layer of black slaty substance is sometimes interposed between it and the coal. This sandstone extends to the surface, varying in thickness according to the undulations of the ground, but at one point over the Horse, the thickness is 94 yards. The sandstone sometimes passes into a conglomerate, containing fragments of coal, ironstone, and vege-

[[*] See Phil. Mag., First Series, vol. lxvi. p. 307.—Edit.]

table remains; also quartz pebbles, similar to those which abound in "the pudding-stone," a deposit between the carboniferous limestone and the old red sandstone, and which attains a considerable elevation in the adjoining hills. The sandstone also encloses concretions of highly indurated, ferruginous sandstone, scattered irregularly throughout its mass; and angular fragments of obliterated casts of vegetable remains, formed likewise of highly indurated sandstone.

The coal under the lows is generally mixed with particles of the sandstone of the roof; but it contains no boulders, angular fragments, or pebbles, as asserted by some observers; the supposed boulders and fragments being, Mr. Buddle observes, the concretions and vegetable remains of the roof, alluded to in the preceding paragraph.

The fall of the Horse conforms to that of the strata or S. 31° E., but whether the "fault" rises with the seam of coal to the outcrop on the S.E. side of the basin, remains to be proved. In the transverse section, the bed of the Horse is nearly horizontal.

There are no indications on the surface by which the Horse can be traced beyond the limits explored under ground; and whether it produces any change in the overlying seams, can be determined only by future works. Mr. Buddle infers, that it does not descend any lower than the Coleford High Delf seam, in consequence of the evenness of the floor, and the entire absence in it of sandstone.

In its underground characters, the Horse is similar to the "washes" or aqueous deposits in many coal-fields, but it differs in not underlying a river bed, or being in the bottom of a valley, and in not extending to the surface. In the Newcastle coal-field all the "washes" cut through the whole of the strata, from the surface to that on which the wash reposes.

In the workings of the Park End Colliery in Park End High Delf seam, which is situated 50 fathoms higher in the series than the Coleford High Delf, and two miles to the S.E. of the point to which the Horse has been traced, a great succession of "lows" has been found in crossing the line of the Horse, but no fault corresponding with the Horse. The coal is deteriorated in the same manner as in the Coleford seam. This colliery is situated beyond the centre of the basin, and where the strata rise in the opposite direction. Future workings alone can determine if there be any connexion between the Horse and these "lows."

In the direction of the Horse there is also an extraordinary oval depression of the Coleford High Delf, the centre of the seam being 20 feet below the ordinary level; and it remains to be proved if the Horse presents the same characters under the depression as elsewhere.

From the phænomena exhibited by the Horse and the adjacent coal-seam, Mr. Buddle is of opinion, that the fault and seam occupy the site of a lake, which existed during the deposition of the latter, and that the carbonaceous matter, which forms the seam, was accumulated while the water was deep and tranquil; that the undulations on the surface of the coal were occasioned by the action of the water when the lake was discharged; and that the Horse occupies the bed of the stream by which the complete drainage of the lake was effected. The sandstone of the roof, and that which fills the lows, he conceives, on account of the fineness of the grain, were tranquilly deposited.

A paper was then read, entitled, "Remarks on the Structure of the Royal George, and on the Condition of the Timber, Iron, Copper, &c., recovered during the operations of Col. Pasley, in the Summer of 1839;" by Mr. Creuze, of Her Majesty's Dock Yard, Portsmouth, and communicated by Captain Basil Hall, R.N., F.G.S.

The Royal George was accidentally sunk at Spithead on the 29th of August, 1782, and as the specimens described in the paper were recovered during the summer of 1839, they had consequently been immersed in a tide-way of salt water fifty-seven years. She was the first ship of war built on the principles recommended by the committee appointed to inquire into the superiority of the vessels in the Spanish and French navies; and was commenced at Woolwich in 1746, and launched the 1st of February, 1756. The Royal George was consequently, when sunk (1782), twenty-six years old.

The great agent in the work of destruction of the timbers had been "the worm." This insect had gradually, by its innumerable perforations on every exposed portion of the wreck, destroyed the fibrous tenacity of the wood, and reduced it to such a state as to permit the wash of the tide to remove the surface layer by layer. The quantity which had been thus destroyed, Mr. Creuze considers, from the parts recovered, to have been almost the whole of the upper portion of the ship, including the topsides above the line of the middle deck ports; and he is of opinion, that in another half century the same agents might have destroyed every part of the hull above the surface of the mud, if Col. Pasley's operations had not been undertaken.

The timbers which had been protected by the mud, were found to be solid and firm; but the only exposed wood which has escaped the ravages of "the worm" is the ash of which the dead-eyes were made. A portion of one of these timbers, which accompanied the paper, and had formed a part of the exterior surface covered with mud, bore no marks of "the worm." The copper sheathing appears to have suffered very slightly, several whole sheets having been found to be of the average weight per square foot of that now used. This state of preservation Mr. Creuze assigns to galvanic action. The copper nails are also nearly perfect. The cast-iron guns which have been recovered, were so soft when first brought to the surface, that they were easily abraded by the finger nail to the depth of at least $\frac{1}{16}$th, and in some parts of $\frac{1}{8}$th of an inch; but they have gradually hardened on exposure to the atmosphere. The brass guns were apparently as sharp in their ornamental castings, and as sound, as at the period of their immersion. A fragment of tarred rope-yarn exhibited a remarkable instance of durability. It is supposed to have formed part of the sea-store of the Royal George, or of one of the cables used by Mr. Tracey in an attempt to raise the ship soon after she was lost. A piece of 2½-inch cable-laid cordage, made from the yarns of this junk, bore 21 cwt. 3 qrs. 7 lbs.; a piece of similar cable, from yarn spun in 1830, bore only 20 cwt. 1 qr. 7 lbs.; but another manufactured from yarn spun in 1838, bore 23 cwt. 1 qr. 7 lbs.

The paper contained various details respecting the construction and measurement of the Royal George, and of the different descriptions of timber used in building the ship. Appended to the Memoir was also a catalogue of twenty-three specimens sent for exhibition, a portion of each of which was presented to the Society's Museum.

A letter, dated November, 1839, was afterwards read, addressed to Dr. Mantell by Mr. C. Hullmandel, "On the Subsidence of the Coast near Puzzuoli."

In 1813, Mr. Hullmandel resided during four months in the Capuchin Convent, which is situated at the entrance of Puzzuoli, and on the seaward side of the road towards Naples. The oldest friar, styled *il molto reverende*, then ninety-three years of age, informed Mr. Hullmandel, that when he was a young man, the road towards Naples passed between the convent and the sea, but that from the gradual subsiding of the soil it had been obliged to be changed to its present course. During Mr. Hullmandel's residence, the refectory and the entrance-gate were from six to twelve inches under water, whenever strong westerly winds prevailed. Thirty years previously such an occurrence never took place. The small wharf at Puzzuoli was also constantly under water during westerly winds. Mr. Hullmandel therefore infers, that as it is not probable the architect of the convent would have so placed the ground-floor as to expose it to inundations, or the builder of the wharf would have so constructed a landing-place as to render it liable to be overflowed; —a gradual subsidence of the soil has been going on for many years, and that this change tends to corroborate the opinion respecting the differences of relative level which have taken place in the Temple of Jupiter Serapis.

A notice was next read, "On part of Borneo Proper;" by G. Tradescant Lay, Esq. Communicated by the President.

The country visible in the background, on approaching the estuary into which the river of Borneo flows, is of variable, though nowhere of considerable elevation. Towards the east, however, is a remarkable range of mountainous ridges, rising one above another like steps, and trending, the author supposes, towards Kinibalu, the most lofty point in the island.

Borneo Proper consists, as far as Mr. Lay's observations extended, of sandstone; but near the mouth of the river is a little island on which coal is found, and called by the natives Pulu-cheomin, or Mirror island, in allusion, it is supposed, to the brightness of the coal.

Mr. Lay says, if he understood his informant rightly, a large supply of fuel might be obtained from the island. Lignite is also found by the natives in sandstone in a deep valley or ravine, not far from Borneo city, and believed by the author to be that called Kianggi. The bed extends obliquely from one side of the ravine to the other, forming an angle of about 45°, with the direction of a rivulet, which flows through the valley; and it is stated to be more than two yards in breadth. The valley is accessible by a path called Jalan-subrek, and conspicuous from the palace of the Sultan, but it is steep, rugged, and narrow. The distance from the water-edge is less than two miles. The whole of the peninsula lying on one side of the river is formed of very steep hills, which gradually become more lofty towards the south-west. Upon the main land, or opposite side of the river, the ridges are supposed to range at right angles to the mountains. They are composed, generally, of a soft sandstone, alternating with clay; but on the summit of one of the hills, Mr. Lay noticed the outcrop of a hard red sandstone, formed of round and angular masses of quartz, particles of black mica, and a ferruginous cement.

A paper was last read, "On some Geological Specimens from Syria;" by Mr. W. C. Williamson.

The specimens were sent to England by Mr. Heugh, to whom the author states, he is indebted for a few notes respecting the localities, whence they were obtained. The chief districts are the vicinity of Beyroot, especially Mount Gebeel Suneen, which forms the part of the Lebanon range immediately above Beyroot. The triangular tongue of land, on which that town is built, is about four miles in extent from the mountains to the coast, and it presents an undulating surface, some of the higher points attaining 500 feet above the level of the sea. The formation of which it is composed, is a hard cream-coloured limestone, which exhibits in the cliffs along the sea-shore numerous veins of flint; and it is in one part of the coast overlaid by a soft calcareous rock, occasionally 100 feet thick. The latter stone is easily wrought, and is employed as a building material, being better able to resist the effects of the earthquakes than the harder and more compact rock.

On ascending Gebeel Suneen from the flat plain, which extends along its foot, and is 400 feet above the level of the sea, the following rocks are passed over:—

Compact limestone............................	1200 to 1500 feet.
Coarse siliceous conglomerate, containing thin seams of lignite, and fragments of siliceous wood............................	800
Compact limestone............................	2000
A very ferruginous rock, composed of minute grains of sand, thickly coated with hydrated oxide of iron	50
A seam of oysters, which may be traced completely around the mountain	

Compact limestone, forming the summit of
the mountain, about .... 100 feet.

In a break in the side of Gebeel Suneen, and extending for some distance along the upper part of the lower conglomerate, is a basaltic dyke, which shoots upwards into the compact limestone. It is about 100 yards wide, and begins, as well as terminates, very abruptly. Except a small hillock near the sea of Tiberias, this is stated to be the only trap seen by Mr. Heugh or his friends throughout the whole of Syria.

The fossils from the middle bed of limestone are generally casts, but are assigned by the author to the genera *Dolium*, *Buccinum*, *Nerinæa*, *Turritella*, *Venus*, *Crassatella ?*, *Hippurites*, *Trigonia*, *Cardium*, *Lucina*, *Nucula*, and *Spatangus*.

In a soft limestone at the village of Ba-abda, and also on the banks of the Zamies, are found large drusy geodes of quartz, and sometimes of chalcedony.

Among the other fossils contained in the collection, are specimens of *Clupea brevissima* (Agassiz, Tab. LXI., f. 6–9.). They occur in great numbers a little above Tripoli, on the way to the Cedars, and about thirty miles north of Beyroot.

None of the fossils, except the fishes, having been identified with described species, Mr. Williamson does not venture to determine the precise age of the beds from which they were obtained; but he is of opinion that the fossils are more nearly allied to the organic remains of the cretaceous series than to any other. The *Dolium*, he says, bears a strong resemblance to the *D. nodosum* of the English chalk, and a species of *Venus* to the *V. angulata* of the green sand. *Nerinæa*, he states, on the authority of Mr. Daniel Sharpe, are found near Lisbon associated with *Hippurites*.

LONDON INSTITUTION.—CONVERSAZIONI.

Jan. 20.—Mr. Grove on a powerful Voltaic Combination.

In reviewing the history of the voltaic battery from its first discovery to the present day, Mr. Grove showed how the chemical theory had led to all the improvements of that instrument. The original pile was defective, as, from the small intervening stratum of liquid, chemical action was soon exhausted; hence the troughs of Cruickshank and Wollaston. A second defect was the waste of local action, now remedied by amalgamated zinc, the inactivity of which is explained by the chemical theory. A third defect was the reaction occasioned by the precipitation of cations upon the negative metal, remedied by Mr. Daniell's introduction of sulphate of copper. However, as the chemical theory supposes that the power of this voltaic combination is as the affinity of oxygen or chlorine for zinc, minus its affinity for copper, a wide field was still open for the increase of power; thus solutions of silver, gold or platina, would be efficient did not their expense preclude their practical employment; the nitric, iodic, chloric and bromic acids, whose elements are united by a very feeble affinity, are still more effectual than any metallic solution, and fortunately the nitric acid, being commercially manufactured, is sufficiently cheap for the purpose.

The battery excited on this occasion was probably the most powerful ever seen, although of very small size; it consisted of 100 pairs of zinc and platina plates two inches by four, with interposed burned pipe clay diaphragms, and was charged with concentrated nitric and dilute sulphuric acid; its superficial relation to the great battery of Davy was as 1 : 40.

The arc of light was from three to four inches long and of great volume; its prismatic spectrum was of extreme brilliancy, and it was polarized and dipolarized by Mr. Grove, showing its identity with solar light.

[We understand that Mr. Grove has received the appointment of Professor of Experimental Philosophy in the London Institution.—Edit.]

XL. *Intelligence and Miscellaneous Articles.*

ON THE FOSSIL WAX OF GALLICIA. BY M. P. WALTER.

SEVERAL years since a fossil wax was discovered at Truskawietz, in Gallicia, or Austrian Poland, in beds of grit and bituminous clay, at a depth of two or three metres. I procured this substance, but want of leisure has prevented me from examining it: it is with regret that I feel it necessary to publish the little that I know respecting it; and I have decided to do so only in the hope that I shall hereafter be enabled to examine it completely.

This fossil is of a brownish black colour; its smell is penetrating and bituminous; it is slightly soluble in alcohol and æther; it fuses at 156° Fahr. When heated in a retort by means of an oil-bath, it first fuses, and at 212° it loses a little water; at 572° nothing whatever distils; above this, ebullition begins to occur, and continues up to 662°: oils first appear, afterwards a yellow-coloured substance, which forms the greater portion of the distilled product. This matter, freed from the empyreumatic oils by pressure through cloth, and dissolved in boiling æther, precipitates on cooling in the state of a beautiful white pearly substance.

It yielded by analysis,

Carbon	85·85
Hydrogen	14·28

This therefore is the composition of bicarburetted hydrogen and paraffin; and what induces me to believe that it is actually paraffin is, that sulphuric acid appears not to exert any action upon it. The examination of oils, formed by the distillation of fossil wax, may throw great light on the formation of naphtha and analogous compounds, which are probably derived from the decomposition of this kind of bodies.—*Ann. de Chim. et de Phys.*, 75, 212.

PHOSPHATE OF COPPER FROM HIRSCHBERG, ON THE SAALE, IN RUSSIAN VOIGTLAND. ANALYSED BY KÜHN.

This mineral is of a nodular appearance; the internal structure is concentrically fibrous, with conchoidal fracture; in colour it resembles malachite. Its specific gravity cannot be determined on account of the impurity of the material. The matrix is a brown iron stone, interspersed with veins of quartz, and containing here and there groups of quartz crystals.

The analysis was gone through in the following manner: some of the purest pieces, amounting to 0·407 gramme, gave 0·031 loss by ignition. After its solution in dilute nitric acid, there remained 0·030. The whole loss from ignition amounted, therefore, to 7·62 per cent.; and the foreign matter to 7·37. Now since 1·147 of pure matrix, by ignition, gave 1·031 of residue, so must 7·37 of matrix, brought to a red heat, have been obtained from 8·20, which was unignited. Thus, in the total loss from ignition, 0·83, which resulted from the matrix, are reckoned. The salt of copper had, consequently, from 91·80 of its first condition, lost 6·79 of water = 7·40 per cent. The oxide of copper, precipitated by boiling caustic potash, amounted to 0·268, or 71·73 per cent.; and since no other acid, besides phosphoric acid, could be found, this last, including what was lost, must have amounted to 20·87. Another portion, which weighed 1·946, was dissolved in dilute nitric acid, and precipitated by sulphuretted hydrogen; the sulphuret of copper was then converted into oxide of copper, which weighed 1·354 = 69·58 per cent.; the liquor, filtered from the sulphuret of copper, remained, on the addition of caustic ammonia, colourless and clear.

From a third unweighed portion, oxide of copper weighing 0·374, and lead precipitate weighing 0·567, were obtained. If, in the last, 0·100 of phosphoric acid must be assumed, so, in the first experiment, where 71·73 per cent. of oxide of copper were obtained, 19·21 of phosphoric acid were contained. The water, besides loss, amounted to 9·06.

The result was, consequently,

Oxide of copper	71·73	69·58	71·73
Phosphoric acid	(20·87)		19·21
Water...............	7·40		(9·06)

The simplest proportion of the formula is $3\,CuO + PO_{2\frac{1}{2}} + 1\frac{1}{2}\,HO$, and of the quantity of oxygen 6 : 5 : 3. From experiment the following formula may be calculated:

$$1\tfrac{1}{2}\,(CuO + HO) + (1\tfrac{1}{2}\,CuO + PO_{2\frac{1}{2}}):$$

or, after Berzelius's method and numbers,

$$3\,(CuO + HO) + (3\,CuO + P_2O_5).$$

Some additional grounds for this formula, as well as some analogues, shall be published hereafter.—*Annalen der Pharmacie*, Vol. xxxiv. No. 2.

ON MINIUM. BY M. LEVOL.

Although minium has been an object of research by several chemists, opinions are still divided, not only as to its analysis, but also as to the manner in which its true constitution ought to be represented.

M. Levol states that, having had occasion to examine several samples with respect to the commercial value, his experiments tend to confirm the views generally entertained with respect to this substance, namely, that it is not a peculiar oxide, but a definite compound of protoxide and peroxide.

M. Levol agrees with M. Dumas, and it may be added also, with the previous determination of Dalton, that the composition of minium is represented by $PbO^2 + 2PbO$, and he finds that this is also its composition obtained by the two following new processes:

The first consists in calcining, in a platina or silver crucible, a mixture of 100 parts of protoxide of lead, prepared by calcining the carbonate, 25 of chlorate of potash, and 200 of nitrate of potash; this last salt is employed for the purpose of rendering the mixture fluid, without occasioning loss of chlorate of potash.

In operating in this way, the action of the oxygen upon the oxide of lead is so effectual, that it is converted into peroxide; and this oxide may be thus procured with the greatest readiness. If the operation be carried on till the mixture becomes nearly red-hot, the swelling diminishes, the mass thickens, and minium is formed. It is sufficient to boil the residue in a solution of potash or soda, and to wash it well, in order to obtain pure minium of the composition above stated. The product is in a state of minute division, of a fine red colour, with a shade of orange, like the finest minium obtained in commerce.

Minium may also be obtained in the moist way, by boiling for an hour or two a solution of an alkaline plombate with binoxide of lead in fine powder; the colour of the binoxide becomes gradually lighter, and eventually an ochre-red powder is obtained; this is merely minium, mixed with a little binoxide, which has escaped the action of the plombate; it is easily got rid of by digestion in a solution of oxalic acid, which decomposes the peroxide without acting upon the compound, and the oxalate of lead is afterwards separated by potash. The product thus obtained has always a deeper red tint than that of the minium prepared in the dry way; but it becomes brighter, and more nearly resembles the latter when it is triturated with water; their composition is similar, and the difference of colour appears to be entirely owing to texture; there is, in fact, some appearance of crystallization in the minium obtained in the moist way.

M. Levol's analyses were performed by putting the minium into excess of nitric acid of sp. gr. 1·114, and agitating the mixture frequently during twenty-four hours; the temperature must not be raised by the action, or otherwise a portion of the binoxide is decomposed, and a small quantity is even dissolved, which gives the solution a violet colour. It remains after this operation merely to weigh the residual binoxide, and to determine that it was totally soluble in protonitrate of mercury, which does not act upon minium.

The following considerations induced M. Levol to believe that minium is a compound of two oxides. In supposing that minium is a peculiar oxide intermediate between these oxides, it is inexplicable how it happens that minium cannot be converted into binoxide by calcination with chlorate of potash, an effect which it readily produces with the protoxide of lead.

Oxalic acid constantly converts binoxide of lead into protoxide, but does not alter minium; and this is at once a method of purifying, and a good characteristic of minium. As this acid, as well as protonitrate of mercury and sulphurous acid, reduce the binoxide of lead to protoxide, and has no action on minium, it may be inferred, not only that minium is a compound of the two oxides, but that it is a compound of remarkable stability.—*Ann. de Chim. et de Phys.*, 75, 108.

ACTION OF ANHYDROUS PHOSPHORIC ACID UPON ANHYDROUS CAMPHORIC ACID. BY M. P. WALTER.

The action excited by anhydrous phosphoric acid upon anhydrous camphoric acid, in nowise resembles that of Nordhausen sulphuric acid upon anhydrous camphoric acid: the anhydrous sulphuric acid acts, so to speak, in a less destructive manner; it re-acts according to the rules of substitutions. Instead of taking carbon from the camphoric acid, it adds the elements of sulphurous acid, and from this re-action a new acid arises; whereas, in causing anhydrous phosphoric acid to act upon camphoric acid, it attacks it in all its molecules and gives rise to several compounds. If several layers of anhydrous phosphoric acid, and anhydrous camphoric acid, be arranged in a tubulated retort, with a tubulated receiver, having a bent tube adapted to it, and immersed in mercury, on the cautious application of heat to the retort, a considerable and continuous disengagement of gas takes place; when this ceases, a liquid substance runs down the neck of the retort into the receiver; this has a slight yellow colour, and a penetrating, but not disagreeable odour; when repeatedly rectified from anhydrous phosphoric acid it is rendered perfectly colourless. In the bottom of the retort there remains a black matter, which is strongly acid.

The gas which is formed in this re-action is of a compound nature; it is a mixture of carbonic acid and oxide of carbon, but they are in indefinite proportions. Several experiments proved that, for one volume of carbonic acid, there were four of oxide of carbon. The liquid is a carburetted hydrogen: two analyses, performed with two different products, gave the same quantity of carbon, namely, 88·4 and 88·2 per cent.; but the quantity of hydrogen varied half per cent.; in one analysis 11·6, and in the other 11·07 were obtained. If the first analysis be correct, the carburetted hydrogen has the same centesimal composition as oil of turpentine; but I do not think that it is oil of turpentine, though its formation may be explained by means of the formula of anhydrous camphoric acid; I am more inclined to believe that the [carburetted] hydrogen in question is a species of naphtha which contains 89 per cent. of carbon, and that the smaller quantity of carbon, found in my analyses, is due to the presence of a little phosphuretted hydrogen, which it is difficult to separate from the carburetted hydrogen. In the latter case, in order to explain the formation of this carburetted hydrogen by means of the formula for anhydrous camphoric acid, the formation of water must be admitted in the re-action. I should have decided this question long since, if the quantity of carburetted hydrogen, obtained during the re-action, had been more considerable than it actually was.—*Ann. de Chim. et de Phys.*, 75, 212.

METEOROLOGICAL OBSERVATIONS FOR JAN. 1841.

Chiswick.—Jan. 1. Hazy: fine with clouds. 2. Rain: clear and fine: hurricanes at night. 3. Thunder-storm about 7 A.M., accompanied with large and vivid flashes of lightning, rain, hail and sleet, and high wind, which soon after subsided into a perfect calm. 4. Sharp frost: slight fall of snow: clear at night. 5. Densely overcast: snow: large lunar halo in the evening. 6. Hazy. 7. Intense frost. 8. Dense fog: severe frost. 9. Intense frost. 10. Overcast: slight haze: rain at night. 11. Overcast. 12. Cloudy: clear. 13. Foggy: rain: fall of snow. 14. Cold haze: rain: sleet and snow. 15. Rain. 16. Thawing rapidly: occasioning inundations, the frozen crust preventing the water from sinking into the earth. 17. Continued thaw. 18. Rain. 19. Overcast. 20. Cloudy and cold: sharp frost at night. 21. Frosty: fine. 22. Frosty: rain at night. 23. Clear. 24. Boisterous: cold and dry. 25. Clear and frosty. 26. Overcast and fine. 27. Very fine. 28. Cloudy. 29. Fine. 30. Hazy. 31. Foggy: rain.

Previously to the thaw, in the beginning of the month, the frost had penetrated in some soils to a depth of 12 inches.

Boston.—Jan. 1. Cloudy. 2. Fine. 3. Cloudy: stormy with lightning and rain early A.M. 4. Cloudy: snow early A.M.: stormy with rain P.M. 5. Stormy. 6, 7. Fine. 8. Fine: thermometer 17°·0 three o'clock P.M. 9. Fine: thermometer 28°·0 three o'clock P.M. 10. Cloudy: large fall of snow early A.M. 11. Cloudy: snow early A.M. 12. Cloudy. 13. Fine: rain P.M. 14, 15. Cloudy. 16. Cloudy: snow early A.M.: rain P.M. 17. Fine. 18. Cloudy. 19. Cloudy: rain early A.M. 20. Snow: snow P.M. 21. Cloudy: snow early A.M. 22, 23. Fine. 24. Stormy: heavy snow-storm P.M. 25, 26. Fine. 27. Fine: beautiful morning. 28. Cloudy. 29. Fine. 30. Cloudy. 31. Rain: rain early A.M.: snow-storm P.M. N.B. The 8th of this month was the coldest day since Jan. 1, 1820.

Applegarth Manse, Dumfries-shire.—Jan. 1. Slight showers. 2. Slight showers: frost in the morning. 3. Snow-storm. 4. Snow-storm and frost. 5. Snow-storm. 6. Fair: snow lying. 7. Snow-fall: frost very keen. 8. Snow-fall slightly: frost keen. 9. Thaw, with slight snow. 10. Snow and frost again. 11. Fair: snow lying: thaw P.M. 12. Fair: but freezing hard. 13. Fair: freezing. 14, 15. Fair. 16. Storm of snow, sleet and rain. 17. Thaw: heavy rain P.M. 18. Frost again: clear. 19. Frost again. 20. Frost again: Aurora borealis. 21. Thaw: drizzling rain. 22. Wet and boisterous. 23. Wet and boisterous: slight snow-fall. 24. Fair: frosty: slight snow-fall. 25. Frost A.M.: drizzle P.M. 26. Thaw and thick fog. 27. Shower in afternoon. 28. Fair and fine: snow melting. 29. Drizzling. 30. Thick fog all day. 31. Clear and cold: moist P.M.

Sun shone out 25 days. Rain fell 10 days. Snow 8 days. Frost 16 days. Fog 2.

Wind north 2 days. North-east 5½ days. East 2 days. East-south-east 3½ days. South-east 1½ day. South-west 4 days. West-south-west 1 day. West 4 days. West-north-west 2½ days. North-west 3 days. North-north-west 2 days.

Calm 8 days. Moderate 8 days. Brisk 3 days. Strong breeze 7 days. Boisterous 4 days. Stormy 1 day.

Mean temperature of the month...........	31°·45
Mean temperature of January, 1840......	37 ·80
Mean temperature of spring-water	42 ·00
Mean temperature of spring-water, 1840..	43 ·30

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. Jan.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·962	29·942	29·882	29·50	29·72	29·76	40·3	44·2	37·5	45	34	39	47	38	W.	W.	calm	W.	·016	...	...	...	34
2.	30·096	30·086	29·816	29·70	29·95	29·61	39·7	45·5	40·0	45	32	35	43	33	W.	W.	calm	W.	...	·15	...	...	35
3.	29·304	29·302	29·084	28·86	29·01	28·74	35·8	44·8	36·0	39	21	37	44	30	S.	NW.	W.	WNW.	·166	...	·13	...	35
4.	29·002	29.185	28·978	28·75	29·20	29·46	30·4	38·7	29·8	35	29	28·5	35	29	S.	NW.	calm	NE.	·061	...	...	...	32
5.	29·308	29·376	29·316	29·10	29·53	29·60	31·8	36·3	30·0	33	22	33	35	27½	NW.	N.	NW.	NE.	·036	...	·47	...	29
6.	29·528	22·631	29·518	29·28	29·55	29·61	27·7	34·4	28·3	30	12	27	29	19	N.	SE.	calm	E.	...	...	...	...	27
○ 7.	29·696	29·760	29·698	29·45	29·58	29·58	19·7	29·8	20·7	27	6	13·5	19	1	NW.	NE.	calm	NE.	...	...	...	0·00	21
8.	29·884	29·893	29·866	29·70	29·60	29·67	19·7	27·3	19·5	20	6	9·5	30½	8½	SSW.	NW.	calm	SE.	...	...	...	...	24
9.	29·706	29·738	29·441	29·47	29·37	29·07	21·2	21·4	14·9	33	27	15·5	34	18	SW.	S.	calm	ESE.	...	...	...	...	18
10.	29·230	29·247	29·089	29·04	29·00	28·94	32·4	32·8	22·2	39	31	27	33½	27	SE.	SE.	calm	ESE.	...	·40	...	...	25
11.	28·864	29·064	28·864	28·66	28·90	29·05	34·3	37·7	32·8	39	33	32·5	35½	23½	S.	SW.	calm	ESE.	·205	·02	...	...	28
12.	29·366	29·578	29·364	29·02	29·19	29·38	35·2	37·2	33·0	39	22	34	34½	28	W.	SW.	calm	NE.	...	...	...	...	30
13.	29·550	29·563	29·324	29·27	29·38	29·38	35·2	38·0	32·8	38	32	27	33½	10	S.	E.	calm	NW.	...	·29	...	...	32
☾ 14.	29·370	29·398	29·176	29·16	29·40	29·46	35·2	39·2	34·0	36	32	35	29½	14	NE.	NE.	calm	NNW.	·352	·80	·11	0·00	32
15.	29·578	29·687	29·559	29·29	29·55	29·57	34·8	37·2	33·3	39	31	36	31	15½	NW.	SE.	calm	NE.	·700	·15	...	...	33
16.	29·654	29·631	29·365	29·39	29·40	28·93	37·4	38·2	33·6	52	40	33·5	37½	30	E.	SW.	calm	E.	·088	·16	...	...	35
17.	29·632	29·729	29·605	29·20	29·18	29·34	47·4	51·8	37·0	52	45	42	40½	35½	SE.	SW.	calm	WSW.	·166	·02	·38	...	41
18.	29·736	29·725	29·685	29·29	29·52	29·70	46·8	52·3	46·7	47	33	43·5	35	31½	S.	W.	calm	N.	...	·24	...	...	42
19.	29·812	29·918	29·778	29·45	29·80	29·91	35·5	48·7	35·0	36	28	35	34	24	NNW.	N.	calm	NNW.	·227	·02	·05	...	37
20.	30·050	30·182	30·019	29·73	30·03	30·14	34·3	37·8	32·6	34	22	30	32½	23	NW.	N.	N.	N.	·022	...	...	...	32
21.	30·410	30·505	30·374	30·00	30·19	30·20	29·8	34·7	28·4	38	25	29	39	23½	W.	NW.	calm	NW.	...	...	...	...	29
● 22.	30·396	30·303	30·157	29·96	29·97	29·70	34·5	39·4	30·2	44	32	36	42	35½	S.	SW.	calm	SW.	...	·10	...	2·35	34
23.	30·084	30·108	30·001	29·65	29·85	29·75	39·3	44·7	34·3	43	32	36	38	31	NW.	NW.	N.	WNW.	·100	...	...	...	37
24.	29·796	30·161	29·796	29·41	29·77	30·13	36·7	42·0	36·0	38	26	34	34	26½	NW.	N.	N.	NW.	...	·02	...	...	33
25.	30·350	30·336	30·195	30·00	30·10	29·88	31·5	38·0	31·0	40	32	28·5	35	21½	SW.	W.	calm	SW.	·033	·01	·15	...	31
26.	30·000	30·000	29·988	29·59	29·70	29·70	42·7	43·6	31·0	49	43	38	42	36	WSW.	SW.	calm	SW.	...	...	...	...	35
27.	30·050	30·225	30·015	29·58	29·77	29·97	47·3	48·6	43·0	53	34	47	48	38	SSW.	W.	calm	W.	...	...	...	...	40
28.	30·322	30·300	30·250	29·83	30·12	30·14	37·8	52·8	36·0	44	28	41	45½	37	SW.	W.	calm	WNW.	...	...	...	...	36
29.	30·202	30·208	30·176	29·80	30·10	30·11	37·7	44·8	36·0	46	32	36·5	42½	41	NW.	N.	calm	W.	...	·02	...	0·55	37
☽ 30.	30·282	30·251	30·212	29·90	30·08	30·13	40·2	44·2	37·9	40	37	37	43½	38½	S.	W.	calm	S.	...	·08	...	...	37
31.	30·210	30·190	30·165	29·82	30·20	30·35	40·8	41·7	40·2	42	28	39	42	34	S.	E.	calm	NE.SE.	·052	·12	·26	...	38
Mean.	29·788	29·813	29·701	29·44	29·635	29·647	35·3	40·3	3[illegible]	[illegible]9·90	28·61	32·7	37·0	26·7					Sum. 2·224	2·60	1·55	0·92	Mean. 33

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

APRIL 1841.

XLI. *On the Voltaic Decomposition of Aqueous and Alcoholic Solutions.* By ARTHUR CONNEL, *Esq.*, *F.R.S.Ed.*, *Professor of Chemistry in the United College of St. Salvator's and St. Leonard's, St. Andrew's*[*].

HAVING been engaged at intervals for several years in researches on the nature of the changes which take place in solutions in the principal solvents under voltaic agency, and having, I conceive, attained some general results, I propose at present to give a condensed view of these experiments and conclusions, the more detailed account of them being contained in two papers already published in the Transactions of the Royal Society of Edinburgh[†], and a third in the course of publication. The principal objects which I have in view at present, are to suggest one or two general rules relating to the secondary voltaic decomposition of dissolved combinations of elementary bodies; to illustrate the state in which the haloid salts are dissolved; and to offer some elucidations of the conducting power of solutions.

I. *Aqueous Solutions.*—It is well known that by the employment of the voltameter Mr. Faraday was enabled to show that in many instances of voltaic action on aqueous solutions, the changes produced on the dissolved substance were the results not of the direct action of the electric current, but of the secondary agency of the products of the direct decomposition of water[‡]. This method, however, is not capable of resolving all cases, many of its results being explicable equally on the idea of a primary as of a secondary decomposition of the dissolved body; for the dissolved substance is often equally capable, with water, in so far as respects composition and atomic constitution, of yielding a definite proportion, whether of oxygen or of hydrogen; and the non-appearance of one or other of these elements at either pole is often equally explicable on the idea of the dissolved substance not containing such element, and on that of its being derived from water, and subsequently entering into union with the elements of the dissolved substance. Many cases, however, occur, in which, from peculiarity of atomic constitution, taken in conjunction with the proportion of gases evolved at the poles, we can have little hesitation in pronouncing in favour of a secondary action. Such cases, I conceive, are those of nitric, sulphurous and the organic acids, and of ammonia, as exemplified in Mr. Faraday's experiments.

---

[*] Communicated by Sir David Brewster.

[†] Vol. xiii. and xiv.

[‡] Experimental Researches, Seventh Series. [L. and E. Phil. Mag., vol. v.]

Accordingly, although in such instances as these Mr. Faraday decided with reason in favour of such an action, yet he was so far from laying down as a general rule, that in all cases of solution of acids, alkalies and other primary combinations of elements, the dissolved body was never directly decomposed by galvanic agency, that he distinctly gave his opinion hat the hydracids are directly decomposed *, and inclined to the view also that the haloid salts were in the same situation.

By adopting other methods, with the occasional aid of the principle of the voltameter, I conceive that I have been enabled to extend the rule of secondary action to all cases of aqueous solution of primary combinations of elementary bodies.

The usual method followed, was to endeavour to separate some one of the constituents of the dissolved body, on the assumption that that body was directly decomposed, and to exhibit it in a distinct shape. The solution, contained in a small tube of the capacity of about one and a half drachm, was connected with another tube of the same capacity filled with distilled water, the connexion being made by a short and thick bundle of asbestus previously well washed with dilute acid and then with distilled water, with which latter fluid it was still moist when the connexion was made, and the level of the solution being a fraction of a line lower than that of the distilled water.

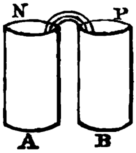
The water was then connected with the side of the battery, to which the constituent of the dissolved substance would naturally pass if that substance suffered direct decomposition,

---

* *Ubi sup.* §. 763–4.

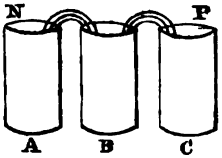
and the solution itself with the other side, as in fig. 1. In this way the constituent must necessarily be exhibited in the water, either separately or in a state of combination with one or other of the elements of the water, which new combination would in its turn suffer decomposition, whether primary or secondary, if admitting it. The readiest and simplest illustration of the efficacy of this method, is to connect the solution of any ordinary salt, such as sulphate of soda, with two tubes containing distilled water, and one of the latter with the positive and the other with the negative side of the battery, as in fig. 2, where B contains the saline solution, and A and C distilled water; when by the passing of acid into C and of alkali into A, the constituents will be exhibited in a separate state.

Fig. 1.



The first case which shall be adverted to, is that of a simple binary combination having neither acid nor alkaline qualities. If an aqueous solution of bromide of iodine is mixed with a solution of starch and acted on voltaically, iodine immediately separates at the negative pole, forming the usual blue combination with starch. Apparently, therefore, the bromide is resolved by the action of the current into its constituents, iodine going to the negative, and bromine to the positive. It is easy, however, to show that this effect is entirely a secondary one: the mixed solution of the bromide of iodine and of starch was placed in the tube B, fig. 1, and the starch solution in A, the asbestus being moistened with the starch solution, B being made positive, and A negative by fifty pairs of two-inch plates*. Effervescence speedily ensued from both poles, but after forty minutes' action not a trace of any blue colour was observed in either tube. The battery was then reversed, the platinum foil in B being now connected with the negative side, and that in A with the positive side. Within two minutes the blue combination appeared round the negative foil; the effervescence ceasing at that pole, but continuing at the positive. Had the bromide been directly decomposed, iodine

Fig. 2.



---

[*] All the batteries employed in these experiments were on Cruickshank's construction.

ought to have been liberated, and the blue colour produced in one or other of the tubes before reversed; but as this change did not occur till after reversal, the effect was due to nascent hydrogen at the negative pole. This hydrogen must have combined with bromine if the bromide is dissolved as such, as is usually held, and is most probable; or with oxygen if the whole or a part of it decomposes water, forming hydrobromic and iodic acids *. The appearances of effervescence lead to the same conclusion. The oxygen liberated at the positive pole in B before reversal must have come from water, and implies an equivalent quantity of hydrogen passing into A, and uniting with the oxygen of water in the course of decomposition in A. Now as this hydrogen did not appear on reversal of the battery, it must have been employed in liberating iodine.

The next case to be considered is that of the oxyacids. When we observe the radical of such an acid solution separating at the negative pole and oxygen at the positive, we are at first sight inclined to suppose that the acid itself has been the subject of direct voltaic decomposition; and accordingly it has been often thought that sulphur and phosphorus, when they are observed in such circumstances, are the result of direct decomposition of the corresponding acids. In like manner we might conclude that the iodine which instantly separates at the negative pole when a solution of iodic acid is acted on, is the result of primary decomposition; but a simple experiment at once shows the action to be secondary. A mixed solution of iodic acid and of starch was placed in the tube B, fig. 1, and a solution of starch in A, the connexion being by asbestus moistened with the latter solution, and B made positive and A negative by fifty pairs of two-inch plates. Effervescence ensued from both poles, but after half an hour's action no formation of blue was observed anywhere in either tube. The battery was then reversed in the manner above described. In two minutes blue matter was formed on the now negative foil in the acid solution with little or no effervescence at that pole, but effervescence from the positive foil. Thus in the first position of the battery, as there was no direct decomposition of the acid, iodine did not pass towards the positive pole; whilst after reversal, iodine immediately appeared by the reducing action of hydrogen derived from directly decomposed water. The analogy of this ex-

---

* When both poles are plunged into the solution, and the battery is in fresh action, there is effervescence at both poles, a part of the hydrogen only being engaged in producing the secondary action.

periment may evidently be extended to sulphuric, phosphoric and other oxyacids.

This result may be further illustrated on the principle of the voltameter. A current of the same power as above was passed through a solution of iodic acid, and one of sulphuric acid diluted with twelve parts of water. Iodine without any elastic fluid separated from the negative pole of the iodic solution, and from the other poles the following quantities of gas were collected.

Negative of sulphuric solution	·33	cubic inch.
Positive of do. do.	·12	„
Positive of iodic solution	·14	„

This result can scarcely be explained except on the idea of the direct decomposition of water only; for similar quantities of oxygen were liberated from both solutions, notwithstanding the difference of atomic constitution of the two acids; and whilst a corresponding quantity of hydrogen was evolved from the sulphuric solution, that from the iodic did not appear, and was plainly occupied in reducing iodine.

In like manner when the same current was passed through dilute sulphuric acid and a solution of boracic acid, it was found that very nearly the same relative quantities of oxygen and hydrogen were evolved from both solutions in the same time, notwithstanding the difference in the atomic constitution of the two acids. There was therefore little doubt that in both solutions the water and not the acid had suffered decomposition.

We turn now to the hydracids. When a solution of muriatic or hydriodic acid, not too weak, is acted on under ordinary circumstances, there is an immediate liberation of chlorine or iodine without oxygen at the positive pole, and of hydrogen at the negative. The first view which strikes one, is that the hydracid is directly resolved into its elements; and this is the view which Mr. Faraday has adopted. It is not, however, difficult to show that the decomposition is a secondary one. A moderately strong solution of muriatic acid was placed in the tube A, fig. 1, and distilled water in B, the connecting asbestus being moistened with the latter fluid, A being made negative, and B positive by a power of fifty pairs of two-inch plates. There was speedy effervescence from both poles, and in about a quarter of an hour a trace of acid was found in the positive tube, which went on increasing; but it was not until after nine hours' action that a feeble and doubtful odour of chlorine could be detected in that tube. When the conducting power of the water in B was improved by adding a few drops of sulphuric acid, a brisker effervescence than before was observed at the poles, but still in ten minutes no trace of chlorine was observed in either tube by the smell or by test paper. The battery was then reversed, when in less than two minutes a decided smell of chlorine was observed in B, and in ten minutes test paper was bleached when plunged into it. It was also ascertained that no gas but chlorine was liberated from the positive pole subsequent to reversal. When the power was seventy pairs of four-inch plates and the liquid in B distilled water, acid was detected at the positive pole in three or four minutes; but after half an hour's action no bleaching could be observed in either tube, although a feeble odour of chlorine was latterly observed. The battery was then reversed, when instantly a pungent odour of chlorine arose and test paper was bleached.

When a moderately strong solution of hydriodic acid was placed in A and distilled water in B, A being negative and B positive, and the power fifty pairs of two-inch plates, a slight effervescence ensued from both poles, and during the first ten minutes there was no discoloration of the liquid in either tube. A slight yellow tint then appeared in the positive liquid and no change in the negative, and shortly after a just perceptible acid reaction was observed in the positive liquid. In a quarter of an hour the battery was reversed. Instantly, notwithstanding the diminished action of the battery, the liquid around the positive foil became brown from liberated iodine, without the least effervescence from that pole. When the level of the hydriodic solution was about a line lower than that of the water, no discoloration took place in either tube in a quarter of an hour, but it was instantly observed on reversal. When the power was seventy pairs of four-inch plates, the discoloration began in B in about five minutes, with acid reaction. On reversal there was instant discoloration in A, and no gas.

In these experiments, it seems obvious that the non-appearance of chlorine for so long a period, and its speedy appearance after reversal with cessation of oxygen, show its secondary origin from the reducing action of oxygen on muriatic acid. If before reversal a trace of it ultimately appeared, its origin was plainly due to the acid which had been some time before drawn over to the positive side. In the case of hydriodic acid the iodine was in like manner due to the reducing action of oxygen; and if it appeared sooner before reversal than chlorine, that circumstance was due to the circumstance of hydriodic acid being a much weaker combination, and more readily reducible than muriatic acid. Even if we suppose that chlorine or iodine in passing by direct voltaic agency towards the positive side reacted with the hydrogen of the water in B, it is plain that the hydracid so formed should have been immediately decomposed again, and the chlorine or iodine should have speedily appeared at the pole in a separate form, and *that* before acid appeared; whereas in the case of muriatic acid, the chlorine did not appear till considerably after acid had passed into B, and much later than iodine appeared in the case of hydriodic acid, although the constituents of the latter are less decidedly opposed to one another in electric nature than those of muriatic acid, and therefore ought reasonably to be of more difficult voltaic decomposition. The quantity of chlorine or iodine which appears is not in proportion to the absolute quantity of acid which passes over, but to the facility with which that acid is reduced. The fair and natural interpretation of the whole phænomena, is evidently afforded on the view of a secondary action on the hydracid.

The experiment with hydriodic acid was varied by connecting two tubes, B and C, fig. 2, each filled with distilled water, with the tube A, containing the hydriodic solution, the power being seventy pairs of four-inch plates. Slight effervescence ensued at both poles, but during forty minutes' action no discoloration appeared in either tube. Soon after a brown tint appeared in C, with acid reaction at the positive pole. In ten minutes more the battery was reversed, when iodine without any gas instantly appeared in A. All these appearances are quite in conformity with the above views.

In regard to metallic oxides it is not easy to obtain such direct experimental evidence, because the metals of such as are soluble in water react on the solvent; but we may take the decomposition of metallic oxides as contained in soluble salts, during which the metal frequently appears at the negative pole, as illustrating the action on solutions of such oxides as are dissolved by pure water. It is now pretty generally admitted, that when metal appears in solutions of metallic salts at the negative pole, it is due to the reducing action of nascent hydrogen; and this opinion I have verified directly by finding, that when solutions of sulphate of copper, or muriate of zinc or nitrate of silver, were made positive by fifty pairs of two-inch plates, and connected by asbestus with distilled water which was made negative, neither metal nor oxide was carried towards the negative pole during half an hour's action; whilst on reversal of the battery, the non-negative foil in a quarter of an hour was found to be more or less covered with reduced metal. Thus the circumstance that in the one position nascent hydrogen was produced in the metallic solution, and in the other in the distilled water, evidently was the cause of the difference of the result. The experiment with muriate of zinc and nitrate of silver was varied by placing the solution in a bent tube and pouring distilled water over it, the negative wire being alternately plunged in the water and in the solution. The results before and subsequent to reversal of the battery were the same as in the experiments with asbestus; the only difference being, that in the experiment with zinc solution, a little oxide of zinc appeared to be deposited at the confines between the water and the solution.

I am aware of the circumstance mentioned by Davy, that the connecting asbestus in an experiment with nitrate of silver acquired a film of silver, water being on the negative side; but without doubting for a moment the accuracy of the statement, I do not hesitate to conclude, from a consideration of my own results, that the silver had resulted in the single instance mentioned from the reduction of silver by the secondary action either of light, or of the hydrogen evolved, by the powerful battery employed, in the negative liquid.

I thought that a negative pole of tellurium in the metallic solution might, by combining with hydrogen, have prevented the formation of metal at that pole: but I found that copper and zinc were deposited even in those circumstances; showing that nascent hydrogen, where the metal held in solution is easily reducible, rather unites with its oxygen than with the tellurium.

In regard to haloid salts, I shall afterwards endeavour to show that they are not dissolved by water as such, but as hydracid salts. In the mean time I shall mention, that proof of exactly the same description as that which has been detailed in regard to the hydracids, was obtained in regard to solutions of chlorides and iodides of potassium, showing that the chlorine and iodine which appear when they are acted on voltaically has a secondary origin.

There is also a very simple experiment of a different description which leads to the same conclusion. When an aqueous solution of iodide of potassium was acted on, using a positive pole of zinc, by fifty pairs of two-inch plates, instead of iodine separating at that pole, as is the case when one of platinum is employed, there was a speedy and copious deposition of oxide of zinc from the *positive* pole, with only a bubble or two of elastic fluid, but with brisk effervescence from the negative; appearances which can only be explained on the idea of the secondary origin of the iodine, and are conformable to the view of the direct decomposition of water, the oxygen of which combines with the zinc when the pole is of that metal, or with hydrogen of the acid of the salt when platinum is used. With chlorides of potassium and calcium, there was also a deposit of oxide of zinc at the positive pole, and sometimes a part of the oxide of zinc was taken up by the acid of the hydracid salt and carried to the negative side.

I conceive that a sufficient number of cases has been investigated to warrant the general conclusion, "that when aqueous solutions of primary combinations of elementary bodies are submitted to voltaic agency, the dissolved substance is not decomposed by the current, but only the solvent." The rule of course does not embrace combinations of the second order, such as oxysalts, which as every one knows are resolved into their constituent acid and alkali under voltaic agency.

[To be continued.]

XLII. *A new and more general Theory of Multiple Roots. By* J. J. SYLVESTER, *F.R.S. &c., Professor of Natural Philosophy in University College, London**.

I SHALL begin with developing the theory of polynomials containing perfect *square factors*, one or more.

First, let us proceed to determine the relations which must exist between the coefficients of such polynomials, and afterwards show how they may be broken up into others of an inferior degree.

A parallelogram filled with letters standing in *one* row is intended to express the product of the squared difference of the quantities contained. Thus $(\overline{\underline{a\ b}})$ indicates $(a-b)^2$ $(\overline{\underline{a\ b\ c}})$ is used to indicate $(a-b^2)\,.\,(a-c)^2\,.\,(b-c)^2$, and so forth.

Suppose now that two of the roots $e_1\ e_2 \dots e_n$ belonging to the $=^n fx = 0$ are equal to one another, it is clear that

$(\overline{\underline{e_1\ e_2 \dots e_n}}) = 0$; and moreover is a symmetric function, and can be calculated in terms of the coefficients of fx.

Next let us suppose that we have two couples of equals, (as for instance a and b, two of the roots equal, as also c and d two others) it is clear, that on leaving any one of the roots out, the $(n-1)$ that are left will still contain one equality, and therefore we have

$$(\overline{\underline{e_2\,e_3 \dots e_n}}) = 0 \quad (\overline{\underline{e_1\,e_3 \dots e_n}}) = 0 \dots (\overline{\underline{e_1\,e_2 \dots e_{n-1}}}) = 0.$$

---

* Communicated by the Author.

None of the parallelogrammatic functions above taken *singly*, are symmetric functions of the coefficients, but their sum is; so also is the sum of the product of each into the quantity left out.

Now in general, suppose that the polynomial fx contains r perfect square factors, so that we have r couples of equal roots belonging to the equation $fx = 0$, it is clear that

$\left(\overline{\underline{e_r\, e_{r+1} \ldots e_n}}\right)$ and all the other $\frac{n(n-1)\ldots(n-r+2)}{1\,.\,2 \ldots (r-1)}$ functions of which it is the type are severally zero. Moreover, the sum of these or the sum of the products of each by *any* symmetrical function of the $(r-1)$ letters left out will be a symmetrical function of the coefficients of the powers of x in fx. To express now the *affirmative** conditions corresponding to the case of there being r pairs of equal roots, we *might* employ the r equations,

$$\left(\overline{\underline{e_1\, e_2 \ldots e_n}}\right) = 0$$

$$\Sigma \left(\overline{\underline{e_2\, e_1 \ldots e_n}}\right) = 0$$

$$\Sigma \left(\overline{\underline{e_3 \ldots e_n}}\right) = 0$$

$$\cdots\cdots\cdots\cdots\cdots\cdots$$

$$\Sigma \left(\overline{\underline{e_r\, r_{r+1} \ldots e_n}}\right) = 0.$$

But these, except the last, are not the *simplest* that can be employed; that is to say, we can write down r others, the terms of which shall be of lower dimensions in respect to the roots.

Let f_μ denote that any rational symmetrical function of the μth degree is to be taken of the quantities which it precedes.

Then the r equations in question are all contained in the general equation

$$\Sigma \left\{ f_\mu \left(e_1\, e_2 \ldots e_{r-1}\right) \times \left(\overline{\underline{e_r\, e_{r+1} \ldots e_n}}\right) \right\} = 0;$$

μ being taken from 0 up to $(r-1)$ we obtain r equations, which in respect to the roots are respectively of all degrees between $\frac{n\,.\,(n-1) \ldots (n-r+2)}{1\,.\,2 \ldots (r-1)}$ and $\frac{n\,.\,(n-1) \ldots (n-r+2)}{1\,.\,2 \ldots (r-1)} + (r-1)$ reckoned inclusively.

Now at this stage it is important to remark that the above r equations, although *necessary*, are not *sufficient*; and indeed, no more affirmations of equality can be sufficient to ensure there being r pairs of equal roots.

---

* The importance of the restriction hinted at by the use of the word affirmative will appear hereafter.

To make this manifest, suppose $r = 2$. Then in order that an equation *may* have two pairs of equal roots, we must have by the above formula

$$\Sigma\left(\overline{e_2 e_3 \ldots e_n}\right) = 0 \quad \Sigma\left\{e_1\left(\overline{e_2 e_3 \ldots e_n}\right)\right\} = 0.$$

But if instead of there being two perfect square factors there be one perfect *cube* factor in fx, it may be shown by the same reasoning as above, that the very same two equations apply. In fact, it may be shown in general that no such equations as those given above can be *affirmed* in consequence of there being an amount r of multiplicity consisting of unit parts which may not be affirmed with equal truth as necessary consequences of the same amount distributed *in any other manner* whatever. How to obtain affirmative equations sufficient as well as necessary (under certain limitations) will appear at the close of this present paper.

It is worthy of being remarked, that if we make f_μ denote the sum of the products of the quantities to which it is prefixed, taken μ and μ together, the equations of affirmation become identical with those obtained by eliminating between fx and $\frac{fx}{d\,nd}$[*].

It can scarcely be doubted that the illustrious Lagrange, had he chosen to perfect the incomplete theory of equal roots given in the *Résolution Numérique*, by applying to it his own favourite engine of symmetric functions, could scarcely have failed of stumbling by a back passage upon Sturm's memorable theorem.

Let us now proceed to show how a polynomical known to contain one or more perfect square factors may be decomposed.

Let us begin with supposing that it contains but one such factor; so that $fx = \phi x \cdot (x-a)^2$.

I shall show how to obtain the equations $C(x-a) = 0$, $D\phi x \cdot (x-a) = 0$, $E(x-a)^2 = 0$, $F \cdot (\phi x) = 0$, each in its lowest terms.

1. To form the equation $Lx + M = 0$, where $x = a$, it is easy to see that if we write down in general the expression $(x-e_1)\left(\overline{e_2 e_3 \ldots e_n}\right)$ this will become zero whenever the root e_1 left out is not one of the equal roots (a): so that in fact (calling the two equal roots e_1 e_2 respectively)

---

[*] See my Note on Sturm's Theorem, Phil. Mag., December, 1839. [L. and E. Phil. Mag. vol. xv. p. 434.]

$$\Sigma \left\{(x-e_1) \times \left(\overline{\underline{e_2\, e_3 \dots e_n}}\right)\right\} = (x-e_1) \times \left(\overline{\underline{e_2\, e_3 \dots e_n}}\right)$$
$$+ (x-e_2) \times \left(\overline{\underline{e_1\, e_3 \dots e_n}}\right).$$

or simply $= 2\,(x-a) \,.\, \left(\overline{\underline{e_2\, e_3 \dots e_n}}\right)$

Hence by making

$$x\, \Sigma \left(\overline{\underline{e_2\, e_3 \dots e_n}}\right) - \Sigma \left\{e_1 \times \left(\overline{\underline{e_2\, e_3 \dots e_n}}\right)\right\} = 0,$$

we have an equation for finding the equal roots $e_1\, e_2$.

Again, it is easily seen upon the same hypothesis, that

$$\Sigma \left\{(x-e_2)\,(x-e_3)\,(x-e_4) \dots (x-e_n) \times \left(\overline{\underline{e_2\, e_3 \dots e_n}}\right)\right\}$$
$$= 2\,(x-e_2)\,(x-e_3) \dots (x-e_n) \times \left(\overline{\underline{e_2\, e_3 \dots e_n}}\right).$$

Hence, to form the equation having the same roots as $(x-a)\, \phi\, x$, we have only to make

$$x^{n-1} \Sigma \left(\overline{\underline{e_2\, e_3 \dots e_n}}\right) - x^{n-2}\, \Sigma \left\{\overline{e_2 + e_3 + \dots e_n} \times \left(\overline{\underline{e_2\, e_3 \dots e_n}}\right)\right\}$$
$$\dots\dots \pm \Sigma \left\{\overline{e_2\, e_3 \dots e_n} \times \left(\overline{\underline{e_2\, e_3 \dots e_n}}\right)\right\} = 0.$$

Suppose now in general that we have (r) perfect square factors, so that $f x = \phi\, x \,.\, (x-a_1)^2\, (x-a_2)^2 \dots (x-a_r)^2$.

To form the equation $C\,(x-a_1)(x-a_2) \dots (x-a^r) = 0$, we have only to make

$$\Sigma \left\{\overline{x-e_1} \,.\, \overline{x-e_2} \dots \overline{x-e_r} \times \left(\overline{\underline{e_{r+1}\, e_{r+2} \dots e_n}}\right)\right\} = 0$$

And to obtain $D \phi\, x \times (x-a_1)\,(x-a_2) \dots (x-a_r) = 0$ we must make

$$\Sigma \left\{(x-e_{r+1})\,(x-e_{r+2}) \dots (x-e_n) \times \left(\overline{\underline{e_{r+1}\, e_{r+2} \dots e_n}}\right)\right\} = 0.$$

The theory of perfect square factors is not yet complete until it has been shown how to obtain constructively $\phi\, x$, and as analogy suggests the complementary part $D'\,(x-a_1)^2 \,.\, (x-a_2)^2 \dots (x-a_r)^2$ each in its lowest terms. To effect the latter it might be said that it is only necessary to take the square of $C\,(x-a_1)\,(x-a_2) \dots (x-a_r)$. It is true the polynomial so formed would contain every pair of equal factors, but not in the lowest terms as regards the coefficients (as we shall presently show).

To solve this last part of the problem, let it be agreed that two rows of letters inclosed in a parenthesis shall indicate the product of the squares of the differences got by subtracting each in the row from each in the other, so that $\left(\begin{smallmatrix} a \\ b \end{smallmatrix}\right) = (a-b)^2$ $\left(\begin{smallmatrix} a & \\ b & c \end{smallmatrix}\right) = (a-b)^2\,(a-c)^2$ $\left(\begin{smallmatrix} a & b \\ c & d \end{smallmatrix}\right) = (a-c)^2 . (a-d)^2\, (b-c)^2 \,.\, (b-d)^2$.

Let us begin with supposing that fx has one pair only of equal roots; to form the simplest quadratic equation containing this pair, write down

$$(x-e_1)(x-e_2)\times\left(\overline{\underline{e_3 e_4 \ldots e_n}}\right)\times\left(\begin{matrix} e_1 \ e_2 \\ e_3 \ e_4 \ldots e_n \end{matrix}\right).$$

Now if e_1 and e_2 are the two equal roots in question the multipliers of $(x-e_1)$ $(x-e_2)$, neither of them vanish.

If e_1 and e_2 are neither of them equal roots $\left(\overline{\underline{e_3 e_4 \ldots e_n}}\right)=0$.

If one of the two only belong to the pair of equal roots

$$\left(\begin{matrix} e_1 \ e_2 \\ e_3 \ e_4 \ldots e_n \end{matrix}\right)=0.$$

Hence it is clear that

$$\Sigma\left((x-e_1)(x-e_2)\times\left(\overline{\underline{e_3 e_4 \ldots e_n}}\right)\times\left(\begin{matrix} e_1 \ e_2 \\ e_3 \ e_4 \ldots e_n \end{matrix}\right)\right)=0$$

is the equation desired.

In like manner if there be (r) pairs of equal roots the equation of the $(2r)$th degree which contains them all may be written

$$\Sigma\left\{(x-e_1)(x-e_2)\ldots(x-e_{2r})\times\left(\overline{\underline{e_{2r+1}\ldots e_n}}\right)\times\left(\begin{matrix} e_1 \ e_2 \ldots e_{2r} \\ e_{2r+1}\ldots e_n \end{matrix}\right)\right\}=0.$$

The coefficient of x^{2r} in this equation is clearly of $(n-2r)$ $(n-2r-1)+4r(n-2r)$ *i.e.* of $(n+2r-1)(n-2r)$ dimensions. The coefficient of x^r in the equation which contains the r equal roots unyoked together is of $(n-r)(n-r-1)$ dimensions, and consequently the coefficient of x^{2r} in the square of this equation would be of $2(n-r)(n-r-1)$ dimensions, *i. e.* would be $n^2+6r^2-(4r+1)n$ dimensions higher than needful.

Finally, to obtain an equation clear of *simple* as well as double appearances of the equal roots, we have only to write the complementary form

$$\Sigma\left\{(x-e_{2r+1})(x-e_{2r+2})\ldots\ldots(x-e_n)\times\left(\overline{\underline{e_{2r+1}\ldots e_n}}\right)\right.$$
$$\left.\times\left(\begin{matrix} e_1 \ e_2 \ldots e_{2r} \\ e_{2+1}\ldots e_n \end{matrix}\right)\right\}=0.$$

Let us, now that we are more familiarized with the notation essential to this method, revert to the question with which we set out, and endeavour to obtain r such equations as shall imply *unambiguously* the existence of r pairs of equal roots.

The existence of r such pairs enables us to assert the following disjunctive proposition, which cannot be asserted when the *same amount* of multiplicity is distributed in any other way.

To wit, on selecting any r roots out of the entire number, either these r will all be found again in those that are left, *or* those that are left will contain *inter se*, one repetition at least; so that except on the latter supposition any $(r-1)$ may be absolutely sunk out of those that are left, and there will still be *one* root common to the $(n-2r+1)$ remaining, and to the r originally selected to be left out.

Wherefore calling the roots $e_1\, e_2 \ldots e_n$ and giving μ any value whatever, we have

$$\Sigma\left\{\int_{\mu}(e_1 e_2 \ldots e_r)\times\left(\overline{\underline{e_{r+1}\, e_{r+2} \ldots e^{n}}}\right)\times\Sigma\begin{pmatrix} e_1 & e_2 & \ldots & e_r \\ e_{2r} & e_{3r+1} & \ldots & e_n \end{pmatrix}\right\}=0.$$

Hence the simplest distinctive equations indicative of the existence of r pairs of equal roots are to be found by putting μ equal in succession to all values from 0 up to $(r-1)$.

For instance, if we require that an equation of the seventh degree shall have three pairs of equal roots, we need only to call the seven roots respectively $a\ b\ c\ d\ e\ f\ g$, and then our type equation becomes

$$\Sigma\left\{\int_{\mu}(a\,b\,c)\times\left(\overline{\underline{d\,e\,f\,g}}\right)\times\left\{\begin{matrix}\binom{d\,e}{a\,b\,c}+\binom{d\,f}{a\,b\,c}+\binom{d\,g}{a\,b\,c}\\ +\binom{e\,f}{a\,b\,c}+\binom{e\,g}{a\,b\,c}+\binom{f\,g}{a\,b\,c}\end{matrix}\right\}\right\}=0.$$

From this it appears that the r distinctive equations for r pairs of equal roots are of different dimensions from the r general or overlying ones corresponding to the multiples r, anyhow distributed; the lowest of the latter being of $(n-r+1)\,(n-r)$, the lowest of the former of $(n-r).(n-r-1) + 2r\,(n-2r+1)$ *i. e.* of $n.(n-1)-3r\,(n-1)$ dimensions. In general we shall find that the more unequally distributed the multiplicity may be the lower are the dimensions of the distinctive equations, and are accordingly lowest when the multiplicity is absolutely undistributed*.

22, Doughty Street, Mecklenburgh Square.

---

* It must not, however, be overlooked, that the equations above given, although decisive as to the existence of r pairs of equal roots *when* the multiplicity is known to be not greater than r, do not enable us to affirm with certainty their existence when this limitation is absent: for should the multiplicity exceed r, then inevitably (no matter how it may be distributed) $\left(\overline{\underline{e_{r+1}\, e_{r+2} \ldots e_n}}\right)$ is always zero, and consequently nullifies each term of every one of the equations in question. In fact (repugnant as it may appear to be to the ordinary assumptions of analytical reasoning), it is not possible to express with *absolute* unambiguity the conditions of there being a multiplicity (r) distributed in any assigned manner by means of r *affirmative equations alone.*

[To be continued.]

XLIII. *On the Atomic Volume and Crystalline Condition of Bodies, and on the Change of Crystalline Form by means of Heat.* *By* Dr. HERMANN KOPP[*].

IN a former paper (Poggendorff, *Ann.* xlvii.) I have endeavoured to show that a relation subsists between the volumes in which bodies unite in forming chemical compounds, in addition to that which is known to exist with respect to their weights. Gay-Lussac had already shown that gaseous compounds combine with one another according to volumes; but this observation had not been extended to solid or liquid compounds. Hence the object of my inquiries was to examine the *atomic volumes* in which the latter combine, in contradistinction to their *atomic weights*; to which attention had hitherto been exclusively directed in examining their chemical composition.

The numbers representing the atomic volumes of bodies are proportional, just as the atomic weights are; and the atomic volume of any body may be deduced from its atomic weight, merely by dividing the latter by its specific gravity. But it is evident that these numbers must vary according to the system of atomic weights adopted; hence in the following paper, that scale of equivalents in which oxygen is equal to 100 is adopted, and not what is still retained by a few chemists, in which hydrogen is regarded as unity. The formulas of the compounds examined are generally those of Berzelius.

It is not sufficient to affirm that bodies unite according to their respective atomic weights and volumes: we must also comprehend clearly what is meant by the application of these terms. Nor is this difficult; for the idea of *mass* is represented by atomic *weight*; whilst a regular and definite *volume* is evidently represented by a *crystal*.

The doctrine of isomorphism shows us that there are many bodies which possess an analogous constitution and the same crystalline form. Our idea of the *volume* (or in other words of the crystalline form) of these bodies must, therefore, be the same. From this it follows that their specific weight is dependent upon our idea of *mass* (that is of atomic weight), whilst our idea of specific weight is connected with the mass contained in the same volume. From these considerations the following law may be deduced:

The specific weight of isomorphous bodies is proportional to their atomic weight; or, isomorphous bodies possess the same atomic volume.

---

[*] Communicated by the Author.

This law may be proved from facts already known. T following presents a tabular view of all the isomorph groups, the specific gravities of which have been determin with accuracy. In the first column the chemical formulas the compounds are stated; in the second their specific g vities, with the authorities from which they are taken; in t third the atomic weights adopted by Berzelius; and in t fourth the atomic volumes deduced from the observations.

Gold and Silver.

Au	19·258	Brisson	1243·0	64·54
Ag	10·428	Karsten.	1351·6	129·61
			675·8	64·80

Berzelius has given the number 1351 as the atomic weig for silver, but Regnault has lately proved that 675 is mo correct.

Potassium and Sodium.

K	0·865	Gay-Lussac and Thénard.	489·92	566·39
Na	0·972	" " "	290·90	299·27
			581·80	598·54

Dr. Clarke, influenced by several theoretical consideratio was induced a few years since to propose that the atom weight of sodium should be raised twice as high as it is co sidered by Berzelius to be; and it is remarkable, that we o tain an equal atomic volume between potassium and sodiu if we adopt the view of Dr. Clarke.

Oxide of Tin and Titanic Acid.

SnO_2	6·960	Mohs	935·20	134·38
TiO_2	4·202	Breithaupt	503·69	119·87
	4·254			118·40
	4·249	Mohs		118·54
	3·759	Breithaupt		134·00
	3·826	Mohs		131·65

The first three numbers for the specific gravity of titan acid are deduced from the mineral *rutile*, which is alwa rendered impure by substances specifically heavier; its atom volume must, therefore, be smaller than it really is. The t last are drawn for *anatase*.

Alumina, Peroxide of Iron, Oxide of Chromium and Ilmenit

Al_2O_3	3·909	Mohs	642·33	164·32
	3·979			161·43
	3·995	Breithaupt		160·78
	4·023			159·67
	3·562	Musschenbroek		180·33
	3·531	Brisson		181·93
Fe_2O_3	5·225	Boullay	978·43	187·26
	5·24	Leonhard		186·72
	5·30			184·61
	5·251	Mohs		186·33

$Cr_2 O_3$	5·21	Wöhler	1003·6	192·63
$(Fe+Ti) O_3$	4·729	Breithaupt	942·90	199·39
	4·793			196·72
	4·75	Kupffer		198·51
	4·78			197·26

Spinel, Gahnite, Chrome-iron ore, Franklinite and Magnetic-iron-ore.

$Mg O, Al_2 O_3$	3·48	Breithaupt	900·68	258·82
	3.62			248·81
½ $\begin{pmatrix} Zn O, Al_2 O. \\ Fe O, Al_2 O_3 \end{pmatrix}$	4·232	Mohs	1113·6	263·12
½ $\begin{pmatrix} Mg O, Al_2 O_3 \\ Fe O, Cr_2 O_3 \end{pmatrix}$	4·410	Abich	1171·8	265·71
	4·439			263·97
½ $\begin{pmatrix} Zn O, Fe_2 O_3 \\ Mn O, Fe_2 O_3 \end{pmatrix}$	5·091	Mohs	1453·0	285·41
$Fe O, Fe_2 O_3$	5·094	Mohs	1417·6	278·28

Copper-glance and Silver-copper-glance.

$Cu_2 S$	5·695	Mohs	992·56	174·28
	5·735			173·07
$(Cu+Ag) S$	6·255	Stromeyer	1272·7	203·47

The atomic weight of silver is supposed to be half that adopted by Berzelius.

Antimony-glance and Orpiment.

$Sb_2 S_3$	4·620	Mohs	2216·4	479·75
	4·626	Breithaupt		479·11
	4·850	Musschenbroek		456·99
$As_2 S_3$	3·313	Musschenbroek	1543·6	465·91
	3·480	Mohs		443·55
	3·459	Karsten		446·24

Cobalt-glance and Nickel-glance.

$Co S_2+Co As_2$	6·298	Mohs	2080·4	330·32
$Ni S_2+Ni As_2$	6·238	Breithaupt	2081·8	333·72
	6·331			328·83

Arsenic- and Antimony-ruby-blende.

$Ag_3 S_3+As_2 S_3$	5·531	Breithaupt	6201·9	1121·3
	5·592			1109·1
	5·524	Mohs		1122·7
$Ag_3 S_3+Sb_2 S_3$	5·787	Breithaupt	6874·7	1187·9
	5·844			1176·4
	5·831	Mohs		1179·0

Tennantite and black Copper.

$Cu_6 S_3 As_2 S_3$ $+2 (Cu_6 S_3 As_2 S_3)$	4·375	Phillips	13563·7	3100·3
$Cu_6 S_3, Sb_2 S_3$ $+2 (Pb_3 S_3, Sb_2 S_3)$	5·763	Mohs	18600·9	3227·6

Lead-glance and Seleniuret of Lead.

Pb S……	7·220	Musschenbroek	1495·7	207·16
	7·587	Brisson		197·13
Pb Se……	8·20	Leonhard	1789·1	218·18
	8·80			203·30

The Carbonates of Zinc and Magnesia; Mesitine; the C bonates of Iron and Manganese; Dolomite and Calcareous s

$Zn O, C O_2$	4·442	Mohs	779·67	175·52
	4·4	Naumann		177·20
	4·5			173·26
$Mg O, C O_2$	2·208	Breithaupt	534·79	190·45
	3·001	Mohs		178·20
	3·112			171·85
	2·88	Naumann		185·69
	2·97			180·06
½ $\{Mg O, CO_2$	3·350	Mohs	625·22	186·62
$Fe O, C O_2\}$	3·363			185·90
$Fe O, C O_2$	3·829	Mohs	715·65	186·90
	3·872	Naumann		184·82
	3·6	Naumann		198·79
	3·9			183·50
$Mn O, C O_2$	3·550	Mohs	722·34	203·48
	3·592			201·10
½ $(Mg O, CO_2$ / $Ca O, C O_2)$	2·884	Mohs	583·62	202·36
$Ca O, C O_2$	2·721	Mohs	632·46	232·43
	2·750	Naumann		229·98

Arragonite, Strontianite, Witherite and Carbonate of Le

$Ca O, C O_2$	2·931	Mohs	632·46	215·78
	2·995	Breithaupt		211·17
$Sr O, C O_2$	3·605	Mohs	923·73	256·24
	3·625	Karsten		254·82
$Ba O, C O_2$	4·302	Karsten	1233·3	286·68
	4·301	Mohs		286·75
$Pb O, C O_2$	6·465	Mohs	1670·9	258·46
	6·428	Karsten		259·94

The Sulphates of Barya, Strontia and Lead.

$Ba O, S O_3$	4·200	Karsten	1458·1	347·15
	4·446	Mohs		327·95
$Sr O, S O_3$	3·588	Karsten	1148·5	320·08
	3·953	Breithaupt		290·52
$Pb O, S O_3$	6·169	Karsten	1895·7	307·29
	6·298	Mohs		301·00

The Nitrates of the same Oxides.

$Ba O, N_2 O_5$	2·915	Hassenfratz	1633·9	560·51
	3·185	Karsten		513·00

$Sr\,O, N_2O_5$	2·890	Karsten	1324·3	458·25
	3·006	Hassenfratz		440·57
$Pb\,O, N_2\,O_5$	4·400	Karsten	2071·5	470·80
	4·769	Breithaupt		434·37

The Chlorides of Barium, Strontium and Lead.

$Ba\,Cl_2$	3·860	Boullay	1299·5	336·66
	3·704	Karsten		350·84
$Sr\,Cl_2$	2·803	Karsten	989·9	353·18
$Pb\,Cl_2$	5·802	Karsten	1737·2	299·40
	5·682			305·72
	5·238	Monro		331·60

The Nitrates of Potash and Ammonia.

$K\,O, N_2O_5$	1·933	Watson	1267·0	655·47
	2·101	Karsten		603·04
	2·058	Kopp		615·64
N_2H_8O, N_2O_5	1·707	Kopp	1004·0	588·16
	1·579	Hassenfratz		635·84

The Chlorides of Potassium and Ammonium.

$K\,Cl_2$	1·915	Karsten	932·57	486·99
	1·945	Kopp		479·47
$N_2\,H_8\,Cl_2$	1·450	Watson	669·61	461·81
	1·528	Mohs		438·24
	1·500	Kopp		446·41

The Carbonates of Soda and Silver.

$Na\,O, C\,O_2$	2·466	Karsten	667·34	276·86
$Ag\,O, C\,O_2$	6·077	Karsten	172·81	284·36

The Sulphates of the same.

$Na\,O, S\,O_3$	2·631	Karsten	892·06	339·05
	2·462	Mohs		362·33
$Ag\,O, S\,O_3$	5·341	Karsten	1952·8	365·65

The Nitrates of the same.

$Na\,O, N_2O_5$	2·256	Karsten	1067·9	473·35
	2·188	Marx		488·07
	2·096	Klaproth		509·50
	2·200	Kopp		485·43
$Ag\,O, N_2O_5$	4·355	Karsten	2128·7	488·79

The Chlorides of Sodium and Silver.

$Na\,Cl_2$	2·078	Karsten	733·55	353·01
	2·150	Kopp		341·18
$Ag\,Cl_2$	5·501	Karsten	1794·3	326·18
	5·458			328·75
	5·130	Herapath		349·50

The Molybdate of Lead, the Tungstates of Lead and L

PbO, MoO_3	6·7	Gmelin	2293·0	342·25
	6·698	Leonhard		342·35
	6·760	Mohs		339·20
PbO, WO_3	8·10	Leonhard	2877·7	355·27
	8·0	Gmelin		359·72
CaO, WO_3	6·040	Karsten	1839·2	304·50
	5·800	Meissner		317·10
	6·028			305·11
	5·576			329·85

Olivenite and Libethenite.

$Cu_4O_4, As_2O_5+2H_2O$	4·281	Bournon	3647·8	852·08
$Cu_4O_4, P_2O_5+2H_2O$	3·6	Mohs	3100·1	861·14
	3·8			815·82

The crystallized Sulphates of Zinc, Magnesia and Nick

ZnO, SO_3+7H_2O	2·036	Mohs	1791·8	880·06
MgO, SO_3+7H_2O	1·751	Mohs	1546·9	883·42
	1·674	Kopp		924·06
NiO, SO_3+7H_2O	2·037	Kopp	1758·2	863·14

The crystallized Sulphates of Copper and Manganese.

CuO, SO_3+5H_2O	2·274	Kopp	1569·3	690·10
	2·2	Gmelin		713·32
MnO, SO_3+5H_2O	2·095	Kopp	1509·5	720·53
	2·087			723·28
	1·834	Gmelin		823·06

The Sulphate and Chromate of Potash.

KO, SO_3	2·623	Karsten	1091·1	415·97
	2·636	Watson		413·91
	2·662	Kopp		409·87
KO, CrO_3	2·612	Thomson	1241·7	475·39
	2·640	Karsten		470·57
	2·705	Kopp		459·04

Diopside, Hypersthene and Hedenbergite.

$\begin{pmatrix}3CaO, Si_2O_3\\3MgO, Si_2O_3\end{pmatrix}$	3·006	Mohs	4408·0	1466·7
	3·127			1409·7
$\begin{pmatrix}3MgO, Si_2O_3\\3FeO, Si_2O_3\end{pmatrix}$	3·389	Mohs	4657·6	1374·3
$\begin{pmatrix}3FeO, Si_2O_3\\3CaO, Si_2O_3\end{pmatrix}$	3·582	Breithaupt	4950·6	1382·1

The crystallized double Sulphates of Potash or Ammonia Alumina, Peroxide of Iron or Oxide of Chromium.

$\begin{pmatrix}KO, SO_3\\Al_2O_3, 3SO_3\end{pmatrix}+24H_2O$	1·724	Kopp	5936·5	3443·4

$N_2 H_8 O, S O_3$ $Al_2 O_3, 3 S O_3$	$+ 24 H_2 O$	1·626	Kopp	5673·6	3489·2
		1·625			3491·4
$N_2 H\ O, S O_3$ $Fe_2 O_3, 3 S O_3$	$+ 24 H_2 O$	1·712	Kopp	6009·7	3510·2
$K O, S O_3$ $Cr_2 O_3, 3 S O_3$	$+ 24 H_2 O$	1·848	Kopp	6295·8	3406·8

The crystallized double Sulphates of Potash or Ammonia and Magnesia, Copper, Iron, Manganese, Cobalt, Zinc, Cadmium or Nickel.

$Mg O, S O_3$ $N_2 H_8 O, S O_3$	$+ 6 H_2 O$	1·721	Thomson	2262·5	1314·7
		1·696	Gmelin		1334·0
$Cu O, S O_3$ $K O, S O_3$	$+ 6 H_2 O$	2·137	Kopp	2762·8	1292·8
$Cu O, S O_3$ $N_2 H_8 O, S O_3$	$+ 6 H_2 O$	1·757	Kopp	2499·9	1422·8
		1·756			1423·7
$Mn O, S O_3$ $N_2 H_3 O, S O_3$	$+ 6 H_2 O$	1·930	Thomson	2450·1	1269·2
$Zn O, S O_3$ $K O, S O_3$	$+ 6 H_2 O$	2·153	Kopp	2770·4	1286·8
$Ni O, S O_3$ $K O, S O_3$	$+ 6 H_2 O$	2·111	Kopp	2736·8	1296·4
		2·136			1281·3
$Ni O, S O_3$ $N_2 H_3 O, S O_3$	$+ 6 H_2 O$	1·801	Thomson	2473·9	1373·6
		1·783	Kopp		1387·4
		1·915			1291·8
		1·921			1287·8

Apatite, Green and Brown Lead Ore.

$CaCl_2 + 3(Ca_3 O_3, P_2 O_5)$	3·15	Naumann	6879·8	2184·1
	3·25			2116·9
	3·225	Mohs		2133·3
$Pb Cl_2 + 3(Pb_3 O_3, P_2 O_5)$	7·0	Naumann	16964·6	2423·5
	7·050	Karsten		2406·3
$Pb Cl_2 + 3(Pb_3 O_3, As_2 O_5)$	7·2	Naumann	18607·9	2584·5
	7·208	Mohs		2581·6

By reference to the table now given, it will be seen that our law has received ample confirmation; but it still remains to be explained why the atomic volume in several of the isomorphous groups is not absolutely equal.

The law can obviously hold only for those bodies which are perfectly isomorphous. But this is seldom the case, for those bodies termed isomorphous are generally only approximately so, for we find the angles of their crystals deviating several degrees from one another; neither are the relations between the axes of bodies thus denominated isomorphous perfectly equal. But it is evident, that the more nearly the crystalline forms of isomorphous substances are identical, the more nearly will their atomic volumes be the same. The crystalline condition and atomic volume stand, however, in a certain dependence on one another, which I will endeavour to prove by several groups which have been more closely examined. It may be proper to remark, that the crystallographic notation employed in the following remarks is that of M. Naumann.

It is said that witherite, strontianite, carbonate of lead, and arragonite are isomorphous. They all crystallize in rhomboids, but the dimensions of their angles respectively are not perfectly equal. If we consider the correct atomic volume from each of these bodies as that deduced from the mean of the views already stated, and compare these atomic volumes with the proportions between the axes, the following is found to be the case:—

		Atomic volume.
BaO, CO_2 : a : b : c =	0·7413 : 1 : 0·5950	286·72
SrO, CO_2	0·7237 : 1 : 0·6096	255·53
PbO, CO_2	0·7236 : 1 : 0·6100	259·50
CaO, CO_2	0·7205 : 1 : 0·6215	213·48

Or we may also compare the atomic volumes with the individual angles, for example with the declinations ∞P and $\breve{P}\infty$:

	$\infty P =$	$\breve{P}\infty =$	Atomic volume.
BaO, CO_2 :	118° 30′		286·72
SrO, CO_2 :	117 16	108° 12′	255·53
PbO, CO_2 :	117 14	108 13	259·50
CaO, CO_2 :	116 16	108 27	213·48

From these comparisons it is shown that the carbonates of lead and strontia are perfectly isomorphous: their atomic volumes also approximate very closely. Again, it is seen that the two other bodies are neither isomorphous with one another nor to the former minerals: hence we find that their atomic volumes also differ.

But it is also apparent that an increase of atomic volume for this group must be followed by an increase of the axis *a*, and a decrease of the axis *c*; or according to the augmentation in the atomic volume the declination ∞P must become more, and the declination $\breve{P}\infty$ less obtuse.

If one of these crystals (of arragonite for example) be heated, its density becomes less, and in consequence of this its atomic volume greater. The consequence of this must be that the declination ∞P must be more and that of $\breve{P}\infty$ less obtuse; and this has been long since observed to be really the case.

But the relation between the crystalline form and the atomic volume may be more correctly estimated in another class of carbonates.

The carbonates of zinc and magnesia, mesitine, the carbonates of iron and manganese, dolomite and calcareous spar, are a group of bodies which possess both an analogous constitution and an equivalent crystalline form. The crystalline form of all of them is that of a rhombohedron, but the axes a in them are unequal and the angle R different. If we adopt the mean of the observations already given as the atomic volume of each of these bodies, we find that the axis a of the rhombohedron increases, that the angle R diminishes, whilst the atomic volume increases.

	Axis a.	Angle R.	Atomic volume.
Carbonate of zinc ……	0·80708	107° 40′	175·33
Carbonate of magnesia	0·81165	107 25	181·25
Mesitine………………	0·81498	107 14	186·26
Carbonate of iron ……	0·81962	107 0	188·50
Carbon. of manganese	0·82182	106 51	202·29
Dolomite ……………	0·83312	106 15	202·36
Calcareous spar………	0·85440	105 15	231·20

We wish to establish a connexion between the length of the axis a; and the atomic volume of a body; for this purpose it is natural to assume the atomic volume as the volume or the cubic capacity of its fundamental form. Accordingly, in the group now considered, the atomic volume (V) must be proportional to the length of the axis a; hence we have

$$a = y\mathrm{V}.$$

But another number is found for y in every body, and the cause of this is obvious. It is supposed in this formula, that the filling up of the space in the rhombohedral crystals is equal on all sides; but it is known by the optical characters of these crystals that this is not the case. In order to discover a relation between a and V, let us suppose

$$a^x = y\mathrm{V}.$$

We seek x and y according to the method of the least squares from the estimations of a and V of the seven different substances, and we find

$$x = 4{\cdot}739 \qquad y = 0{\cdot}0020417.$$

The relation between a and V is therefore

$$a^{4\cdot739} = 0{\cdot}0020417\ \mathrm{V}.$$

This formula coincides very exactly with the observations. And as the greater part of the substances in their natural condition are accompanied by impurities, the formula may be employed to calculate their specific weight from their cryst line form. If we suppose that the axes of the bodies alrea examined are given, we deduce the atomic volume, and fr it the specific gravity.

	Atomic volume.	Spec. gravity.
Carbonate of zinc.........	177·37	4·3956
Carbonate of magnesia...	182·18	2·9355
Mesitine	185·75	3·3658
Carbonate of iron.........	190·41	3·7585
Carbonate of manganese	193·26	3·7377
Dolomite	210·28	2·7755
Calcareous spar	232·36	2·7220

From the differences occurring between the specific weig observed and calculated, we can conclude whether the su stances found in their natural condition are rendered impu by substances specifically heavier or lighter; but with tho minerals which occur in a state of purity, the specific gravit ascertained by observation and calculation approximate ve closely. In the case of dolomite the specific weight obtain by experiment is always greater than it is found to be by c culation; but analysis shows that it is always rendered impu by the oxides of iron and manganese. The carbonate manganese is, on the other hand, less than the calculated sult, but it is always mixed with carbonate of lime.

From what has now been brought forward, it must be e dent that an increase of atomic volume is dependent upon increase of the axis *a*. The application of heat to one of the crystals must decrease its density, and the axis *a* must be e larged, whilst the angle R will be rendered less obtuse.

This has long since been discovered by Mitscherlich. Th chemist has accurately determined the diminution of dens on the application of heat to calcareous spar. He found th by a heat of 100° C. (180° F.) its specific gravity was decreas in the proportion of $1 : \frac{1}{1\cdot001961}$. We find above the sp cific weight of calcareous spar, when its axis $a = 0\cdot854$ and its angle $R = 105° 5'$ is 2·7220. By heating it 100° C., therefore, it will be equal to 2·71675, or its aton volume passes from 232·36 to 232·80. If we determine t length of the axis *a* by means of the formula already give we find it = 0·85672, corresponding to an angle R of 10 57′ 22″. According to this calculation the change in the ang R by 100° C = 7′ 37″, a result which coincides sufficien with that found directly by Mitscherlich (8′ 34″), when consider the difficulties which necessarily accompany the rect measurement of the dilatation and change of the angle

XLIV. *Further Remarks on some of the Circumstances under which Steam developes Electricity.* *By Dr.* Charles Schafhaeutl*.

BY repeating the experiments described in the February Number of the Philosophical Magazine, p. 95, of holding the glass bell against the jet of wet steam, it frequently happened that the water dropping down from the edges of the bell flowed in a continuous stream, and served as a conductor for the electricity developed within it.

In order to obviate this, I substituted for the short metallic jet (the opening of which must never be smaller than one-sixteenth of an inch in diameter) a flexible leaden tube about ten inches long, with its upper end bent downwards at an angle of about 80°. Now in consequence of this arrangement I was enabled to place the glass bell, with its mouth upwards, simply upon a small ring on the table under the opening of the jet, and the bell served not only as a receiver for the steam, but likewise for the separating water.

By this arrangement an objection is removed, which might have arisen in respect to my former experiments, viz. *Whether the observed electricity did not arise from the friction of the escaping minutely divided water during its striking against the glass bell?*

Now in this experiment the development of electricity is the same, whether the issuing steam jet strikes against the glass, or touches only the surface of the water, which is collecting in it during repeated experiments; neither the distance of the mouth of the jet from the glass or the water surface made the slightest difference in the quantity of the liberated electricity, a proof not only that the cause of the electricity is not to be ascribed to friction, but likewise that a sheet of water exerts the same condensing power in respect to wet steam as the solid glass.

The fact already mentioned, that the inside of the glass bell, when discharged, begins slightly, and often repeatedly, to recharge itself again after the steam has ceased to flow, excludes all possibility of explaining the phænomenon by means of friction, but the electricity observed in this way might be ascribed possibly simply to evaporation still going on in the bell in some degree after the steam has been stopped. I therefore frequently poured boiling water into the glass bell, and brought it into connexion with the electroscope; but neither the water nor the steam ascending in clouds from the surface of the water showed the slightest traces of electricity. It appears, therefore, that steam, the caloric of which is derived from the water from which it separates, developes no electricity.

---

* Communicated by the Author.

The temperature, for example, of the laboratory during one experiment was 45° Fahr. The temperature of the issuing wet steam at a distance of one-eighth of an inch from the mouth of the jet was 150°; at three inches further the temperature of the steam sunk to 120°; in the bell itself it was 110°; and the collected and condensed water in the bell measured 100°, the steam escaping from this water showing only between 60° and 70°.

The temperature of the steam and the water in the boiler was under a pressure of thirty-two inches, of course about 253°. After opening the stop-cock, the water released partially from its pressure was converted, till the stop-cock was shut again at a pressure of about twenty-three inches, 62·3 of its bulk further into steam, converting the mass of water into an agglomeration of air-bubbles*, which, mixed with the steam of the upper part of the boiler, escaped through the metallic tube. During its expansion in the air the free caloric of the steam became latent, which was immediately replaced by the free caloric of the minutely divided water, mixed with the steam, so that the temperature of this jet of steam must be of course, notwithstanding its expansion, far higher than that of pure steam escaping under an equal pressure; and this higher degree of heat retained in wet steam, may be one cause of its retaining the admixed water in a state in which it is easily and at once separable by striking against a resisting or condensing medium, as glass, water, or even air, at which moment electricity begins to become developed.

The steam separating afterwards from the condensed water, has at the same time its own quantity of caloric derived from the heat of the boiler; and instead of having it absorbed from the hot water, which must be the case when boiling water is poured into the glass bell, it has rather a tendency to impart some of its caloric to the water.

The greatest difficulty is to explain in which state or form the water exists mixed with the issuing steam; whether it is suspended in it as minute *solid* water globules, or as minute *hollow* ones, as Saussure considered them to exist in clouds, perhaps similar to a soap bubble? I am inclined to adopt Saussure's opinion, particularly as all liquids when agitated with gases assume finally a form resembling bubbles filled with gas. When water-gas is viewed through a microscope

* A similar effect may be witnessed daily when the cork of a soda-water bottle is drawn.

the minute vesicles may be very easily distinguished from water globules passing at the same time through the field of the microscope, the former disappearing when coming in contact with each other, the latter uniting in one larger globule.

M. Fresnel considered the theory of Saussure as inadmissible, because the air surrounding the small vesicles must be in a state near saturation, and therefore he thought it was scarcely possible that the envelope of these little globules could be of an equal or lesser density than the air in which the clouds are suspended. Further, he considered the swiftness of their movement entirely irreconcilable with their vesicular state, as the friction caused by such movement through the air would soon set them at rest. Finally, he explains, as the air in the vesicles must be in a state of condensation, as it has to resist the tendency of the water molecules to unite themselves in a drop, the globule would finally disappear, as a soap bubble will swiftly diminish and disappear when the tube on which it is suspended is removed from the mouth. But this latter case only takes place when the film forming the soap bubble is very thick, viz. in the beginning of its formation, and can *possibly* only occur when the bubble is joined to a *tube*, which with the vesicles in the cloud is certainly never the case; but when the bubble is once formed, every child knows that it will stand till evaporation or concussion causes it to burst.

The rapid movement of the small vesicles of water is for the greatest part attributable to currents of air; and as therefore their velocity is equal to that of the molecules of the air, no possible friction and loss of velocity can arise from this circumstance.

In further considering the equilibrium of the cloud, we must not only bear in mind the single vesicles as such, but the single vesicle surrounded of course by an atmosphere of air and water-gas united to one substantial body, viz. the *cloud*, and forming itself by exerting forces within, as molecules of water when at liberty form and arrange themselves into a shape where their mutual attractive forces are in a state of perfect equilibrium. Besides, the surrounding air, as long as the cloud swims in it, can only be in a state of saturation on one part and in contact with the cloud. When the entire air between the earth and the clouds is near saturation, the clouds begin to decompose and discharge water.

Fresnel assumes that clouds consist of minute solid globules of water, and as the air, in their intervals and therefore in contact with the globules, is easier to be heated by radiation than air not in contact with solid or liquid bodies, he explains, that this air between the water globules is of a much higher temperature than the surrounding air, and that this heated and rarefied air, together with the water globules, constitutes a body of equal or less density than an equal mass of the surrounding air. But if we take into consideration the slight expansion of gaseous bodies for every degree of the thermometer, and compare the very great difference in density between air and water, and therefore the mass of heat required to keep only a small quantity of water in suspension for such a considerable time; further, if we ask, why by such an elevation of the temperature of the air in contact with the water globules the capacity for saturation of this air may not increase with the temperature, and the minute water globules be not converted into water gas again;—all these circumstances present difficulties almost insurmountable in adopting this theory.

M. Saigey finally declares, that the equilibrium of the clouds is only *apparent*; that the minute globules of water in the cloud were indeed *constantly falling*, but immediately as they left the cloud were again converted into water-gas by touching the surrounding non-saturated air, which rising and cooled down again, restored again its water to the cloud, so that all these clouds were constantly losing water on one and receiving it on the other side. This explanation can only be correct under certain circumstances, that is when the cloud is near its decomposition. The sharp definite outlines and the circumscribed forms of the clouds swimming in a clear and dry sky, prove that they are not further connected with the cause of their origin; besides, their swimming so long in the air unaltered in form during very widely different degrees of temperature, and their movement often in different directions coming in contact with each other, and separating again unaltered in shape, all proves too well their substantiality, or rather their *individuality*.

XLV. *A Suggestion relative to* Barrett's *Method of computing the Values of Life Contingencies. By* A. De Morgan, *Esq. Professor of Mathematics in University College*[*].

THE method of calculating the value of life contingencies, which is called after Mr. Barrett, its inventor, has been rapidly coming into use of late years, and is well known by those who are used to it, to save much the greater part of the time and labour required by the common methods. The use of this method will be much extended by the copious tables published by Mr. Jones in the "Library of Useful Know-

[*] Communicated by the Author.

ledge," where, for the first time, we see tables for the problems which involve two lives.

An improvement in the method of calculating tables for two lives has suggested itself to me, which I have no doubt will be adopted as soon as proposed, and will be carried into effect in future tables.

If a_m be the number living at the age m, and v the value of 1*l.* due in a year, the construction of Barrett's tables (in the improved form suggested by Mr. Griffith Davies) is as follows:—

$$D_m = a_m v^m, \quad N_m = D_{m+1} + D_{m+2} + \ldots\ldots$$

with other results, not here necessary to be specified. When we come to construct tables for two lives it is necessary that the column answering to D_m, say $D_{m,n}$, should be of the form $a_m a_n v^k$, where k is a function of m and n, which increases by a unit when m and n are both increased by a unit. In Mr. Jones's tables, k is the greater of the two, m and n; I propose that it should be the half sum of the two, thus:

$$D_{m,n} = a_m a_n v^{\frac{1}{2}(m+n)}, \quad N_{m,n} = D_{m+1,n+1} + D_{m+2,n+2} + \ldots$$

In problems in which the order of survivorship does not enter, I do not see any particular advantage in this change; but in all such problems there is a decided and obvious gain. At present two formulæ must be used, accordingly as the life which is to survive the other is elder or younger of the two; while with tables calculated in the manner proposed by me, one only would be necessary. And even supposing that no new tables were calculated, it would still be better to teach the method in the manner I propose, while two very simple transformations would reduce any result to those which the present tables require.

For example, the present value of 1*l.* payable on the decease of a life aged y, on condition that a life aged x is then subsisting, according to the method I propose, is in all cases

$$\frac{N_{x-1,y-1}\,v - N_{x,y} + (N_{x,y-1} - N_{x-1,y})\sqrt{v}}{D_{x,y}}.$$

To convert this into the common method (if it can yet be called common, as far as *two* lives are concerned) the rule is, divide every term of the form $N_{x,y}$ or $D_{x,y}$ by $v^{\frac{1}{2}(x-y)}$ or $v^{\frac{1}{2}(y-x)}$, according as x or y is the greater. If then $x-1$ be greater than, or equal to y, the preceding result, so treated and reduced, becomes

$$\frac{(N_{x-1,\,y-1} - N_{x-1,\,y})\,v + N_{x,\,y-1} - N_{x,\,y}}{D_{x,\,y}}$$

while if $y - 1$ be greater than, or equal to x, it becomes

$$\frac{(N_{x-1,\,y-1} + N_{x,\,y-1})\,v - (N_{x-1,\,y} + N_{x,\,y})}{D_{x,\,y}}.$$

These results agree with those of Mr. Jones (p. 176), if in the formulæ cited $N_{x,\,y}$ be substituted for its equivalent $N_{x-1,\,y-1} - D_{x,\,y}$. If ever the time should arrive when tables of this sort are calculated for three lives, it should be by the formula

$$D_{m,\,n,\,p} = a_m\, a_n\, a_p\, v^{\frac{1}{3}(m+n+p)}$$

and so on. In such a case, the method now in use would become intolerable, from the large number of cases into which different orders of survivorship would require formulæ to be subdivided.

XLVI. *Note to a Paper "On certain points in the Undulatory Theory, &c." in the last Number of the Philosophical Magazine. By the Rev.* BADEN POWELL, *M.A., F.R.S., &c., Savilian Professor of Geometry, Oxford**.

IN referring to the establishment of the principle of a relation between the symmetrical or unsymmetrical arrangement of the ætherial molecules, and the rectilinear or elliptic nature of the vibrations, I was led at the suggestion of a friend to add a note (p. 161) while the paper was in the press, with respect to the share which M. Fresnel might be supposed to have in disclosing that principle.

Having since looked more closely into the matter, I think it right to take this opportunity of stating the result.

Fresnel devotes a section of his admirable Memoir on double refraction to the consideration of the mechanical theory of ætherial vibrations. But this, though characterized by all the profound ability of the author, is discussed almost entirely in general terms, and the results are not connected with any mathematical development of equations of motion.

---

* Communicated by the Author.

He has considered the conditions presented by different cases of *elasticity* of the æther within crystallized and uncrystallized media, and, in particular, traces the consequences of a supposition of this kind in modifying the formula for waves so as to give rise to elliptic or circular polarization in *rock crystal*, and refers to a peculiar arrangement of the vibrating molecules *in that substance* as influencing the right or left-handed direction of the rotation*. But I cannot find that he took up such an idea upon any more general grounds: in other words, I think he cannot be said to have shown *generally* a connexion between the arrangement of the molecules of æther in space,—the forms assumed by the differential equations accordingly,—and the consequent rectilinear or elliptic character of the vibrations. I am still of opinion, therefore, that the credit of first establishing such a general relation is due entirely to Mr. Tovey.

Oxford, March 10, 1841.

XLVII. *Report on the Memoir on Electric Currents in Warm-blooded Animals, by* Prof. Zantedeschi *and* Dr. Favio, *presented to the Royal Academy of Sciences and Belles Lettres of Brussels, on the 4th of April,* 1840. *By* M. Cantraine†.

THE memoir which is addressed to us relates to electro-physiological doctrines, and the object of the experiments which these gentlemen have made, is to ascertain whether electric currents exist in warm-blooded animals; to endeavour to find their connexion with life; their intensity, their direction, their number, and in what way they may be discovered. Electricity, whether considered as the cause of the phænomena of life, or as the special and intimate effect of life, is in some measure a new subject: few persons have attended to it; the difficulties also which surround such researches are numerous, and we must acknowledge with the authors, that in this case the concurrence of natural philosophy and the medical sciences is indispensable, as much on account of the apparatus which should be used, as for the perspicacity necessary to penetrate into the mysteries of the vital organism.

These gentlemen set out with the principle enunciated by Pucinotti and Pacinotti, the learned Professors of Pisa, which is based upon the experiments and the observations also made by those Professors. It maintains that *in living animals there exists an electro-vital or neuro-electric current, the character of*

---

* *Mém. Instit.*, vii. p. 74.

† From the *Bulletin de l'Academie Royale des Sciences et Belles Lettres* of Brussels, 1840, No. 8. p. 43; *Séance du* 1^{er} *Août*.

which is that it is the effect of a vital reaction, whether anatomical or voluntary, and that it is, if not the cause of life, at least that immediate and special effect which life alone can produce and maintain; that *this current may be discovered* (sondé) *and obtained in a manner more or less evident, according to the nature of the metals and the form of the instruments*; that *it proceeds in the direction of from nerve to muscle*; that *it is intimately connected with the energy and with the physiological changes of life*; that *it has nothing in common with the electro-chemical and thermo-electrical currents*; that *the convulsive movements of animals augment it, whilst, on the contrary, pain weakens it*; that *the same difference exists with it as between animal life and organic life, following thus, in the first case, the phases of animal life and dying with it; and in the second case, still existing so long as organic life can endure, although animal life has ceased.* The current which is obtained from organic life was named by the Professors of the University of Pisa, the *cardiac current.*

From this corollary, we might be led to see in this electro-physiological doctrine much analogy with that of the *electro-vitalists.* The difference, however, is very great; the authors of this memoir, recognizing the vital force as a primitive force, by which matter, obeying the common laws of nature by its intrinsic virtue, changes into animated matter.

The authors having proved that the force of the neuro-electric current depends upon the nature of the metals which are employed, made use of iron, then of silver instruments; they propose however trying those of platina. The results which they give are obtained then with iron and silver instruments.

The apparatus necessary for the experiment consists of a galvanometer,—of two metallic stylets, which are also called sounds or réophores (whether they are of iron, of silver or of platina), soldered metallically to the two extremities of the conducting wire of the galvanometer,—and of a living animal.

The galvanometer is placed in an isolated spot and far from anything composed of metal, particularly of iron; it is fixed where it neither undergoes jogs or shocks. At a little distance from this galvanometer should be a kind of small coffer rather shallow, made entirely of wood and without an atom of iron; a living animal is slightly fastened in this coffer, which is brought into the circuit of the galvanometer in the following manner:—

One of the metallic stylets which were just mentioned, and which should be furnished with an insulating handle, is suddenly plunged (*plongé*) into some part of the living animal,

t into another part of the same animal is plunged (*en-*
) another stylet formed like the preceding one, and like
nished with an insulating handle. It is understood that
eans of the interposition of the animal, the circuit of the
nometer is thus complete.

the moment when the two stylets are plunged (*enfoncé*)
he two parts of the animal, there is disengaged from the
nt of animal life, by the effect of a vital reaction, an elec-
urrent, which, diverted from its natural conductors by the
nce of the metallic body which is plunged into the or-
substance, directs itself along the stylets and the wire
ictors which are soldered to them, so as to complete the
it by the same wires and in the animal parts which are
osed: thus in this circuit the current having to pass
the multiplicator of the galvanometer, the deviations
take place in the magnetic needle of this instrument
ate its intensity and direction.

is is the explanation which the authors have thought
r to give as well of the operation as of the very simple
atus which serves for it. This, however, is not all which
d concur to ensure the result of the experiments; there
till some circumstances which exert a great influence
it, such as the perfection of the galvanometer, the na-
of the metal of which the instruments are made, the ho-
neity of the stylets, every intervention of chemical sub-
es, the nature of the season, the state of the atmosphere,
ainful preparations of the animal, its organic develop-
as well as its age, and lastly, its greater or less degree of
ility, &c.; conditions which might appear too nume-
to allow us to hope anything from similar researches, if
esults already obtained did not show the contrary.

e authors, considering the existence of electric cur-
in warm-blooded animals as sufficiently proved, ask if
should call them vital, and supposing that this name
d be given them, what should be understood by *vital*
nt? They adopt the whole definition which MM. Puci-
and Pacinotti have given of it, and which I quoted above;
also adopt for the time being the denomination of *electro-*
current.

sides this *electro-vital* current, they have verified in the
animals two other currents which had been mentioned
e Professors of Pisa; the one is the *common electro-chemi-*
irrent; the second the *thermo-electric.* The first is di-
into the *common electro-chemical current,* properly so
l, and the *vital electro-chemical current*; this is the cur-
which is supposed to proceed from the intimate chemism

(*chimisme*) of the organic and animal life which resides in substance of the organs and tissues.

In order to distinguish these currents, they apprise us we should not take as the electro-chemical current in rela with life, that which is obtained by sounding (*sondant*) the ferent products and the different secretions of the organs; s a current is named the *common electro-chemical*, whilst the tal electro-chemical current is that which is developed in internal animal chemism, which is not subject to the comı chemical laws; a current which they are not willing to s pose to be the cause of life, but which may be regarded as special effect which life alone can produce.

By this it is evident that the common electro-chemical rent is obtained by simply placing the stylets in contact the secretions, or with the surfaces of the organs moiste with heterogeneous products.

The second, on the contrary, is only obtained by pene ting with stylets even into the intimate assemblage of the ganic tissues, and by exciting in them a strong reactio means of this deep perturbation.

Consequently, if we look upon this last current eithe drawing its origin from the chemical and proper action of on matter, or as the index and the measure of this ac principle, agent of life, whether it be considered chemic in the first case, whether it be considered physiologicall the second, we must always conclude that the *vital elec chemical* current either is the same thing as that which been called *neuro-electric*, or *electro-vital*; or even if a di ence should exist between them, in the present state of p sical and physiological knowledge, we cannot point out certainty any character which distinguishes them.

The third kind of current arises from the different de of temperature of the different parts of the animal whic subjected to the operation. It is for this reason that the n of *thermo-electric* current has been given to it. It is der from nothing else but a disturbance of calorific equilibri on this account it is reckoned amongst the phænomen matter, and has no essential connexion with life. Besi this current is, so to say, null or of very small importa since physicists have proved that the temperature of ani is nearly equal in every part of their system.

The authors again inform us that the deviations of needle of the galvanometer indicating the currents, were ways measured by them, reckoning from the first excur which the needle makes at the moment when the second st is thrust in, that is to say, at the moment when the circu completed, which being closed, the needle never returned to its first position, but, on the contrary, it always kept itself at a distance all the time that the experiment and the deviation lasted, according to a fixed or certain mark, sometimes of three, sometimes of four, of seven, and even of fifteen degrees. Twenty-seven experiments are described in this memoir: the authors sum up the results of them in five propositions, which are the following:—

Prop. 1. *In warm-blooded animals there is an electro-vital or neuro-electric current, which we will call external or cutaneous, which is found in the cutaneous tissue, and directs itself constantly from the extremities to the cerebro-spinal axis by means of the galvanometer. The intensity of this current, according to the experiments which have been made, is generally greater with stylets of iron than with those of silver.*

Six experiments support this proposition.

Prop. 2. *In warm-blooded animals there is an electro-vital current, which goes from the cerebro-spinal axis to the internal organs situated beneath the skin; for this reason we will call it electro-vital internal current. By means of the galvanometer we see that it is constantly directed from the cerebro-spinal axis to the other viscera, or, if you will, from the nerve to the muscle. The intensity of the internal current is in general greater with the iron stylets than with those of silver.*

Eight experiments served to establish this proposition.

Prop. 3. *The electro-vital current in warm-blooded animals grows weaker in proportion as it comes less from life: death having taken place, it proceeds in an opposite direction to that in which it was directed during life.*

This proposition rests upon eight experiments.

Prop. 4. *Pain weakens or suspends the electro-vital current; it even changes the direction of it if it is very great. The voluntary or convulsive automatic movements give, on the contrary, a very strong current, which may be named discharge of current.*

A single experiment appeared sufficient to the authors to establish this proposition.

Prop. 5. *The electro-vital current either cannot be discovered or measured, or does not really exist in the different parts of the same viscus: it is very feeble, and perhaps it is even null from viscus to viscus.*

Four experiments support this proposition.

The experiments which have served to establish each of the five propositions are followed by summary annotations of the authors. Although they appear to me to be very interesting for one who is occupied in such researches, I did not think it

right to translate them, in order to avoid making the repo
as long as the memoir itself.

Such are the conjectures which the authors have thoug
they might publish, and which new experiments will come
destroy or corroborate. They have described them witho
any sectarian spirit, and only as physiological hypothes
which, if they are found not unworthy of mature examinatio
may serve as guides for new experiments.

XLVIII. *Notices of the Results of the Labours of Continent Chemists. By Messrs.* W. Francis *and* H. Croft.

[Continued from p. 212.]

Black Substance from Sulphuric Acid and Alcohol.

THE nature of the peculiar black substance originating fro
the action of sulphuric acid on alcohol was first point
out by O. Z. Erdmann, who concluded from his experimen
that it was composed of a substance nearly approaching the fo
mula $C^{50}H^{45}O^{4}\dddot{S}$, and of an incombustible residue consisti
of sulphatic salts. Subsequently, L. Lose, who had obtain
this substance whilst preparing the isæthionic salts, was i
duced to examine it, and obtained somewhat different resul
leading to the formula $27\,\dot{C}\;21\,H\;6\,O\,\dddot{S}$. The differen
between these results and those of Prof. Erdmann, M. Lo
attributes to a portion of the substance being subtracted
fixed salts, thus giving rise to a smaller amount of sulph
and a larger amount of carbon and hydrogen. Recently
Erdmann has made known some new researches on this su
ject; and since, as far as we are aware, no notice has appear
in England of the former inquiries, we shall make our pr
sent extract rather full.

It was prepared in the first instance by heating six pa
of fuming sulphuric acid, which left but a slight residue
evaporation, with one of alcohol of 0·83 sp. gr. in a reto
till the evolved gas was no longer combustible; the resid
then frequently treated with boiling water of ammonia, and
last washed with boiling water till the filtrated liquid no long
evinced the presence of sulphuric acid. Dried in pieces it h
the appearance of coal, and is difficult to pulverize. On co
bustion it left behind a very small residuum, which dissolv
easily in nitric acid, and gave a slight precipitate with t
chloride of barium. The substance dried by exposure to t
air gave off much water on being heated. Desiccation w

fected *in vacuo* at 130°–150° C. It does not decompose even
t 170° C. Hess's apparatus was employed for the analysis.
'he substance was placed in a weighed glass capsule of the
rm of a boat, this fixed in the tube between two coils of fine
latinum wire to prevent its coming into contact with the
xide of copper, so that the weight of the incombustible re-
due might be determined. Between the tube containing
e chloride of calcium and the apparatus for the potassa,
tube with oxide of lead was inserted, but no sulphurous
id appeared to be formed. To determine the sulphuric acid
e substance was burnt with a mixture of nitre and carbonate
soda. Mean of the experiments C 66·11, H 3·78, S 8·92,
21·19. It was then prepared according to the method em-
oyed by M. Lose, by passing alcoholic vapours into heated
ming sulphuric acid, and purified as above. The product
as similar, and likewise left an incombustible residue, which
as determined in the same manner. The analysis gave
64·30, H 3·33, S 7·49, O 24·88. It was further prepared
boiling pure distilled sulphuric acid with alcohol; when
e mass began to thicken from the formation of the coaly sub-
ance, the experiment was interrupted and a portion was re-
oved, and cleaned by boiling with ammonia and water; the
cond portion was so long exposed to a temperature of 180° C.
l the evolved gas was no longer combustible, and appeared
consist solely of sulphurous and carbonic acids; it was then
ashed and dried. Both products had the same appearance,
d were subjected to analysis, the mean of which = C 65·58,
4·55, S 8·38, O 21·49.

On treating the substances which had been employed in
e preceding experiments with a solution of caustic potash,
ey all evolved such considerable quantities of ammonia that
is could only be regarded as adhering mechanically to them.
'he treatment with potassa was continued at the boiling point
l no smell of ammonia was perceptible, and the residues were
en washed with boiling water. On combustion they now
ft considerable quantities of sulphate of potash. The black
bstance can therefore combine with bases, and the residue
und by Erdmann in his previous analyses of the substance,
hich had been treated with potash to remove the sulphuric
id, was evidently sulphate of potash. Although the ana-
ses communicated were made with ammoniaferous products,
d cannot consequently give the correct composition of
e black acid, yet as they had all been treated in the same
ay with an excess of ammonia they should evince a coinci-
ence did the black substances possess a constant composi-
on. But this is not the case. The products obtained by

boiling sulphuric acid with alcohol, appear to differ accordin to the duration of the action and temperature: thus one pr duct washed solely with water gave 63·8 C, and 3·3 H; ar other gave 64·3 C, and 2·4 H. At last it was obtained fro three preparations of the same composition, by boiling eigl to ten parts concentrated sulphuric acid with one of absolut alcohol in a sand bath at 180° C. till the mixture had lost a fluidity. The mass was then washed with water till the fi tered liquid gave no precipitate with chloride of bariun The free acid, which most reddens litmus paper, as we as the potash salt, were analysed. Acid = C 67·66, H 3·3 S 6·65, O 22·35. Potash salt = C 59·78, H 2·91, S 5·7 O 20·26, Ka 11·35. Treatment with ammonia, acetate lead, chloride of barium, gave no definite salts. Erdman calls it thiomelanic acid, and observes that the coincidenc between the found and calculated results gives to the formu advanced ($C^{60} H^{48} S^{3} O^{20}$) a high degree of probability, bu that no especial value can be placed in it, as, with the excep tion of the agreement in composition of the substance fro three different preparations, there is no surety for its purit On employing a higher temperature than the one stated, an on heating the mass till it began to solidify, a product wa obtained, the composition of which was essentially different.

For the sake of comparison the black substance was ex amined, which is formed from the action of sulphuric acid o pyroxylic spirit, but the composition of the substance ob tained proved to be very different from that of the thiome lanic acid, viz. 67·14 C, 1·73 H, 1·40 S, 29·73 O. (*Journ für praktische Chemie*, vol. xv. p. 1.—*Poggendorff's Annale* vol. xlvii. p. 619.)

Action of Chlorine on Animal Substances.

M. Mulder has continued his researches on the action chlorine on animal substances. Albumen (from eggs) wa mixed with water, filtered, and submitted to a current chlorine; very shortly white flocks were formed, which gr dually increased; the precipitate was put into a filter ar edulcorated; it appeared to be somewhat soluble in wate It was dried at 80°, at which water was driven off, and shrunk together; became white with a trace of yellow; it w then dried at 100° C. Formula $C^{40} H^{62} N^{10} O^{12}, \underline{\overset{\cdots}{Cl}}$. The san compound was obtained by the action of chlorine on amm niacal solutions of casein and fibrin. Mulder calls it protei chlorous acid. The fluid from which this new substance h been separated contains hydrochloric and chlorous acid hence water must have been decomposed; the chlorous acid produced combined direct with the protein. The fluid contains a little of a brown matter, probably humin, but in very small quantity. Proteinchlorous acid is straw-yellow, has a greasy feel, like proteinsulphuric acid, insoluble in alcohol and æther, almost so in water; soluble in, without colouring, strong sulphuric acid; by water white flocks are precipitated. Nitric acid dissolves it at ordinary temperatures in the course of some days and forms xanthoproteinic acid. Soluble in cold hydrochloric acid; does not form humin as protein does. Soluble in baryta water; the solution gives precipitates with nitrate of copper and zinc. Soluble in ammonia, with evolution of nitrogen; when evaporated leaves a residue soluble in hot water; from this solution alcohol precipitates a new organic matter, which is soluble in cold water; it must be washed with alcohol and dried at 130°; it is called oxyprotein. It is a yellow powder, and, although often treated with boiling alcohol, generally contains a little chlorine. Formula $C^{40} H^{64} N^{10} O^{16}$. Oxyprotein is easily pulverizable; of an amber-yellow colour; heavier than water; soluble therein; almost insoluble in alcohol, quite so in æther; soluble in hot sulphuric and hydrochloric acids; changed by nitric acid into xanthoproteinic acid; soluble in potash, soda, ammonia and baryta water, and the solutions give no precipitate with ferrocyanuret of potassium; sulphuric acid gives a white one, soluble in the heated solution. Solution of galls gives a precipitate: nitrate of silver, acetate of lead and copper, and chloride of iron are precipitated by it. The copper salt is $C^{80} H^{126} N^{20} O^{31}. Cu O$, probably $(C^{40} H^{62} N^{10} O^{15}, Cu O) + (C^{40} H^{62} N^{10} O^{15} + H^2 O)$. Proteinchlorous acid forms a peculiar salt with baryta: the precipitates produced in metallic solutions by this salt have been analysed, but their composition is as yet doubtful. Mulder calls them chloroxyproteates. (*Annalen der Chemie und Pharmacie*, xxxvi. p. 68.)

When hæmatin is treated with chlorine it becomes white; all iron is extracted and remains in the solution with hydrochloric acid and a little organic matter.

The white flocks, when dried at 100°, still smell of chlorous acid, their formula is $C^{44} H^{44} N^6 O^{24} Cl^{12} = C^{44} \quad H N^6 O^6 + 6 (Cl^2 O^3)$, or one atom of hæmatin and six of chlorous acid.

The new compound is light yellow, insoluble in water, soluble in alcohol and æther. Dissolved by strong sulphuric acid, decomposed by heating, and sulphurous acid is evolved. Soluble in ammonia with evolution of nitrogen; soluble in potash and soda with reddish colour.

Xanthoproteinic acid is prepared by acting on several protein compounds with nitric acid; the dried product must b often treated with water and boiling alcohol. It was dissolved in ammonia, evaporated, dissolved again in water, an the solution treated with chlorine: the red colour graduall disappeared, and a whitish or yellowish flocculent precipitate was produced, which when washed and dried at 100° i lemon-yellow coloured. Formula $C^{68} H^{100} N^{16} O^{26} \overset{\cdots}{\underline{Cl}}$, o $2 (C^{34} H^{48} N^8 O^{12} + H^2 O) + Cl^2 O^3$. Heated above 100° i loses water and some chlorous acid. With ammonia i evolves nitrogen; if the fluid be evaporated and the sal-ammoniac extracted with alcohol, the yellow acid remains.

When xanthoproteinate of ammonia is heated to 140° th ammonia is driven off, and the hydrated acid remains. Formula $C^{34} H^{50} N^8 O^{13} = C^{34} H^{48} N^8 O^{12} + H^2 O$.

Action of Potash and Soda on Indigo—Anilin.

M. Fritzsche has found, that when finely-powdered indigo is thrown into a hot and very concentrated solution c caustic potassa or soda contained in a retort, the blue colou is destroyed and a reddish-brown mass produced, containin a peculiar acid; when this is still further heated, a volatile body is given off, which condenses in the neck of th retort into oily drops, and ammoniacal water distills ove By re-distillation this oil may be obtained colourless; it quantity is about 18 or 20 per cent. of the employed indigo he calls it *anilin*. Specific gravity 1·028; refracts light powerfully, and has a strong aromatic but at the same time disagreeable smell; is but little soluble in water; mixes in all proportions with alcohol and æther. Exposed to air anilin becomes yellow, and at length is metamorphosed into a resin Boils at 228° C. It dissolves sulphur. Anilin contains n oxygen; its formula is $C^{12} H^{14} N^2$. Atomic weight 1181·6 It forms salts with acids; with the oxyacids it combines an takes up one atom of water; with the hydracids it unites directly; in this respect it is similar to ammonia. Oxalate o anilin is formed when alcoholic solutions of anilin and of oxalic acid are mixed together; it is a white powder, which is t be washed with alcohol, and then dissolved in hot water, fron which it separates on cooling in long crystals. Formula C $H^{14} N^2 + C^2 O^3 + H^2 O$. (*Annalen der Chemie und Pharmacie*, xxxvi. p. 84.)

Hydrochlorate of anilin is obtained by mixing anilin wit hydrochloric acid and re-crystallization of the salt, which i easily soluble in water, $C^{12} H^{14} N^2 + Cl^2 H^2$.

Other salts have been formed but not examined; the ac tion of iodine and nitric acid seems to be very interesting, but has been as yet so superficially examined, that it is of no use making any extract. In a note to the above treatise, Prof. Erdmann states it to be very probable that anilin is nothing more than Unverdorben's krystallin.*

Palmitic Acid from Palm Oil.

By saponifying palm-oil with potassa, Fremy obtained two acids, one of which he calls palmitic acid; it is volatile: by the distillation a very small quantity of oil is produced, from which it may be freed by means of alcohol.

The composition of the free hydrated acid is $C^{64} H^{124} O^{6}, H^{4} O^{2}$; in the salts, however, it is $C^{64} H^{124} O^{6}$. The distilled acid has the same composition. The ammonia salt is insoluble in water, and has the formula $C^{64} H^{124} O^{6}, N^{2} H^{8} O + H^{2} O$.

The æther is obtained by acting on alcohol with sulphuric and palmitic acids; it is solid, melts at 21° C., crystallizes in prisms. Formula $C^{64} H^{124} O^{6} + 2 (C^{4} H^{10} O)$; the acid is therefore bibasic. By the action of chlorine on palmitic acid a mixture of several products is obtained, which it is almost impossible to separate: one of them has the formula $C^{24} H^{108} Cl^{16} O^{6}, H^{4} O^{2}$. (*Annalen der Chemie und Pharmacie*, xxxvi. p. 44.)†

Oils of Cascarilla and Caraway.

M. Vœlckel has examined cascarilla and cumin or caraway oils. The impure cascarilla oil is deep yellow; specific-gravity 0·909; boils at 180° C. By distillation with water seemed to be separated into two oils, which, however, could not be perfectly isolated. The oil which first passed over was colourless, highly refractive, specific gravity 0·862; boiled at 173° C.: composed of carbon 86·926, hydrogen 10·487, oxygen 2·587. The portions which passed over afterwards were a little thicker, somewhat yellowish, and of a less agreeable odour. Composition, carbon 82·021, hydrogen 10·267, oxygen 7·712. (*Annalen der Pharmacie*, xxxv.)

Oil of caraway may be separated by distillation into several different oils; the first two portions boil at 193° to 195° C., the third at 213°, the fourth at 228°. The smell was almost the same in all.

C.	86·099	85·204	82·126	78·603
H.	11·090	10·517	9·827	9·217
O.	2·811	4·279	8·047	12·180

Caraway oil absorbs hydrochloric acid gas, but is separated again by distillation with water.

---

* Some further particulars relating to *Anilin* will be found in our *Intelligence and Miscellaneous Articles*.—Edit.

† See Mr. Stenhouse's paper on the constituents of Palm-oil, in our last Number, p. 186.—Edit.

Caraway Oil—Æthereal Oils.

MM. Gerhardt and Cahours have made some experimen on æthereal oils. Roman caraway oil (essence de cumi consists of two oils; one is a hydro-carbon, and boils at 16 C.; the other consists of carbon, hydrogen and oxygen, and similar to hydro-benzoyl; they call the hypothetical radi cumyl.

Hydro-cumyl .... = $C^{20} H^{22} O^{2} + H^{2}$
Cuminic acid .... = $C^{20} H^{22} O^{2} + O$
Chloride of cumyl .... = $C^{20} H^{22} O^{2} + Cl^{2}$
Bromide of cumyl .... = $C^{20} H^{22} O^{2} + Br^{2}$
Hydrated cuminic acid ... = $C^{20} H^{22} O^{2} + O + H^{2}O$.

Hydro-cumyl is a colourless or slightly yellow-colour liquid of strong odour, is changed by slow distillation, exc when the operation is performed in an atmosphere of c bonic acid. By the absorption of oxygen is converted i cuminic acid; the metamorphosis takes place more rapi when it is treated with caustic alkali, in which case hydrog is evolved. Cuminic acid is colourless, crystallizes in pr matic needles, scarcely soluble in water, easily in alcoh may be sublimed, and has an acid burning taste. Hyd cumyl treated with sulphuric acid and chromate of pot also forms cuminic acid. Hydrated cuminic acid distil with four parts of caustic baryta gives an aromatic colourl liquid, which boils at 144° C.; its formula is $C^{18} H^{24}$; may called cumen*; with fuming sulphuric acid forms cumens phuric acid, which gives a crystallizable salt with bary With nitric acid forms nitrocumid, like nitrobenzid. (*An len der Pharmacie*, xxxv.) †

Potatoe-Spirit Oil—Oil of Anise—Oil of Fennel.

M. Cahours has examined some other oils. *Potatoe-sp oil* is converted by perchloride of phosphorus into a flu which boils at 102°, and has the formula $C^{10} H^{22} Cl^{2}$; it is chloride of amyl. Acetate of amilen is decomposed by ch rine (in the shade), it is changed into $C^{10} H^{18} Cl^{4} O + C^{4} H^{6}$ in sunshine a crystalline substance is formed. Potatoe oil converted by the action of air and spongy platinum into lerianic acid. (*Annalen der Pharmacie*, xxxv.)

Oil of anise.—The solid oil has the formula $C^{22} H^{26}$ The specific gravity of the vapour is 5·68; with chlori forms a fluid compound, $C^{22} H^{20} Cl^{6} O^{2}$, with bromine a c stalline one, $C^{27} H^{20} Br^{6} O^{2}$. With dilute nitric acid of 20°

---

* May not Cumen be identical with Mesitylene? Their boiling poi differ very little,—276° and 287°,—and their composition is the same R. K.

† See also our *Intelligence and Miscellaneous Articles.*—Edit.

forms an acid whose formula is $C^{16} H^{14} O^{6}$; the silver salt is $C^{16} H^{12} O^{5} + AgO$. With concentrated nitric acid the anisonitric acid is formed, $C^{16} H^{12} O^{6} (N^{2} O^{4})$; its silver salt, $C^{16} H^{10} O^{6} (N^{2} O^{4}) + AgO$. [I may here remark that these two acids were prepared independently, in the beginning of last summer, by Dr. Weltzien, in Prof. Mitscherlich's laboratory: we may probably in a short time expect a better account of them than the above.—H. C.]

Anise oil gives with hydrated sulphuric acid a crystalline compound, with fuming sulphuric acid a peculiar acid, whose baryta salt is soluble.

Oil of fennel consists of two oils; the solid oil has the same formula as the solid oil of anise; the fluid oil has the formula of oil of turpentine: the latter combines with binoxide of nitrogen and forms a crystalline compound, $C^{15} H^{24} N^{2} O^{4}$.

Oil of Bergamot.

Oil of bergamot (from the fruit of *Citrus bergamia*) has been examined by MM. Soubeiran and Capitaine. The oil was distilled with water and dried with chloride of calcium; its specific gravity was 0·869; it refracted the polarized ray 25°. It was again distilled with water and the product collected in four portions: the first refracted 45°, the second 38°, the third 21°, and the fourth not at all. Oil of bergamot must therefore consist of at least two oils. Ohm found that oil of bergamot contained 7·098 per cent. oxygen; and he considers it as a hydrate of lemon oil, $3 (C^{10} H^{16}) + 2 H^{2}O$; according to MM. Soubeiran and Capitaine, the fractionated portions have different compositions; the oxygen varies from 3·37 to 16·14. By the action of anhydrous phosphoric acid an oil is obtained which has the same composition as citron and lemon oil: this body combines with hydrochloric acid gas. The camphor appears to have the usual formula. By the action of the phosphoric acid on the impure oil a peculiar acid is produced (acide phospho-bergamique), which forms soluble salts with lime and oxide of lead. (*Annalen der Pharmacie*, xxxvi.)

Crystallized Substance in Guarana.

MM. Martius, Berthemot and Deschastelus have examined the crystallized substance contained in "guarana;" the nature of guarana was for a long time doubtful, until Martius showed that it was a paste made of the fruit of a tree—*Paullinia sorbilis*, Mart. The guaranin is contained in it in combination with tannic acid, and it appears that the so-called guaranin is identical with caffein. As it has already been shown that thein is also identical with caffein, we have now three families in which it is found, viz. Rubiaceæ, Theaceæ and Sapindaceæ. (*Annalen der Chemie und Pharmacie,* xxxvi. p. 90.)

Action of Chlorine on Marsh Gas.

By the action of chlorine on natural marsh gas, M. Melsens has obtained the chloride of carbon, $C^2\,Cl^6$; hence marsh gas and the gas obtained from the acetates are perfectly identical.

Magnus's *and* Gros's *Salts of Platinum.*

If protochloride of platinum or Magnus's salt, $Pt\,Cl^2,\ N^2\,H^6$, be boiled with ammonia, the green compound is dissolved, and the evaporated fluid deposits acicular crystals; these are soluble in water and ammonia, precipitated by addition of alcohol; when heated evolves sal-ammoniac, and at last free hydrochloric acid; the residue is metallic platinum; by long boiling with potassa ammonia is also evolved. M. Reiset finds the formula $Pt\,Cl^2,\ N^4\,H^{12}$, consequently it is the radical in the salts of M. Gros. These salts have the general formula $Pt\,Cl^2,\ N^4\,H^{12} + O + \bar{A}$. When Reiset's salt is treated with nitric acid red vapours are evolved, and Gros's nitric salt is formed. When the aqueous solution of $Pt\,Cl^2,\ N^4\,H^{12}$ is treated with chlorine, Gros's chlorine salt is produced, viz. $Pt\,Cl^2\,N^4\,H^{12}\,Cl^2$. (*Annalen der Chemie und Pharmacie,* xxxvi. p. 3.) *

Gros's *Platina Salts.*

In the 19th *Arsberattelse*, p. 261, when speaking of Gros's platina salts, Berzelius protests against the introduction of so many improved theories, and says that a much simpler formula for these salts would be $Pt\,\underline{Cl} + (2\,N\,H^3 + \underline{Cl})$, if indeed this latter compound had been prepared in a separate state. He moreover says,—If the formula $\underline{N}\,\dot{\underline{H}}^4\,\dddot{S} + Pt\,\underline{Cl}\,\underline{N}\,\underline{H}^2$ does not explain why the salt is not precipitated by chloride of barium or nitrate of baryta, is the phænomenon any better explained, when we throw ammonia, water, chloride of platinum and amidogen together into one base? Hydrochloric and nitric acids also have an affinity for this base, and therefore the sulphate compound ought to be decomposed by the baryta salts, &c. *

Prussian Blue.

Leopold Gmelin has remarked, that the Prussian blue obtained from a protosalt of iron by means of the ferrosesquiuret of potassium cannot have the same composition as obtained from a sesquioxide or persalt, and the ferrocya-et; the former must contain three atoms of protocyanuret on and one atom of sesquicyanuret, while the common blue ains two atoms of sesquicyanuret of iron. M. Vœlckel analysed the former substance in Wöhler's laboratory; it prepared by adding ferrosesquicyanuret of potassium xcess of protochloride of iron. It contained, iron 89, potassium 5·284, cyanogen 33·684. Probable for-a, $(Fe^2\,Cy^6 + 3\,K\,Cy^2) + 4\,(Fe^2\,Cy^6 + 3\,Fe\,Cy^2.)$ (*An-n der Pharmacie*, xxxv.)

---

* See Dr. Kane's paper "On a New Class of Platina Salts," in the present Number.—Edit.

Double Carbonate of Lead and Soda.

uring his experiments on the atomic weight of carbon, zelius discovered a double salt of carbonate of lead with onate of soda; it is obtained by dropping a solution of te of lead into an excess of solution of carbonate of soda, boiling the mixture. Its formula is $\dot{Na}\,\ddot{C} + 4\,\dot{Pb}\,\ddot{C}$. *berattelse*, xix.)

Acid Products of Citric Acid at high temperatures.

he researches of Lassaigne, Baup, Dahlstroem, Berzelius Robiquet, had led us to suppose that three acids were ed from the decomposition of citric acid at high tempe-res, which is fully confirmed by a series of experiments . L. Crasso, recently published in the *Annalen der Phar-ie* (vol. xxxiv. p. 53). The first formed is the aconitic of Dahlstroem and Berzelius (Baup's *acide citridique*) $H^2\,O^3 + Ag$; then the itaconic acid (pyraconitic acid, saigne's pyrocitric acid, and Baup's *acide citricique*) $H^4\,O^3 + Ag.$; and lastly, citraconic acid (pyrocitric acid obiquet and Lassaigne, Baup's *acide citribique*), isomeric the preceding. The two last appear to be bibasic, the tribasic. No aconitic acid is formed previous to the ngagement of aceton and carbonic oxide. The two other s are products of the aconitic acid. No distillation was inued longer than ten minutes. The acid first melts gives off pure water, upon which white vinous acid and bustible vapours are disengaged, which evidently contain on and carbonic oxide. If the operation be now inter-ed, the residue dissolved in water and brought to cry-ization, pure unaltered citric acid is obtained, of which analysis gave the formula for the citric acid dried at 100°, $C^{12}\,H^{10}\,O^{11} + 3\,H^2\,O$; and besides which, a small por-of uncrystallizable mother-ley proportional to the quan-of aceton and carbonic oxide evolved. When further

heated the white vapours disappear, the products are n
longer combustible, and the recipient contains an acid colou
less fluid abounding in aceton. When white vapours a
no longer visible, oily stripes are formed in the neck of th
retort, indicating the formation of a new product. If th
yellow residue be dissolved in water it forms no crystals eve
at 0° cent., when evaporated to the formation of a pellicl
and the cold and solidified mass then treated with æther, b
little citric acid remains undissolved. The æther on evapor
tion leaves behind a warty granular mass—the aconitic aci
Since citric acid does not form an æther on treating its alc
holic solution with hydrochloric gas, the aconitic acid ma
be thoroughly purified by dissolving it in five parts of abs
lute alcohol, and passing hydrochloric gas through the sol
tion. The aconitic æther is then decomposed with a vino
solution of potash, the potash salt by acetate of lead, and th
washed precipitate decomposed with sulphuretted hydroge
it is then obtained as a laminar crystalline acid, which on r
solution in æther and slow evaporation appears colourle
and verrucous-crystalline: it consists of C 41·84, H 3·4
O 54·75. The silver salt was employed to determine th
atomic weight, which was found to be 618·11; the formula i
therefore, $C^4\,H^2\,O^3 + H^2\,O$. The silver salt is white, n
quite insoluble in water; formula $= C^4\,H^2\,O^3\,Ag\,O$. Ac
nate of barytes is gelatinous, when dried, amorphous. Th
salts of potash, soda and ammonia, do not crystallize distinctl
are easily soluble in water and in alcohol. Aconitic æthe
the preparation of which has already been described, is c
lourless, of aromatic smell and bitter taste; specific gravi
= 1·074 at +14° C.; boils at 236°, above which it decompos
almost entirely into white vapours and a black fatty residu
Formula $= C^8H^{12}\,O^4 = Ae\,O\,\overline{At}$.

The same æther has likewise been prepared by Dr. Ma
chand in the following manner. A mixture of half a pound
citric acid, three quarters of a pound of alcohol, and a quart
of a pound of concentrated sulphuric acid was distilled in
sand bath for about four hours, the liquid that passed ov
being constantly poured back. At last only sulphuric æth
mixed with a little alcohol passed over, and the mass had
very dark brown colour. On mixing with water a quanti
of coloured æther separated, which was washed with wat
till acid reaction ceased. When decolorated by means
animal charcoal, it formed a yellow fluid, very similar to th
usual citric æther. Its boiling point above 230°. It coul
not be distilled without decomposition, so that it was imposs
ble to take the specific gravity of its vapour. Dr. Marchan

arrives at the same formula, $C^8 \ddot{H}^{12} O^4$, and states that this therefore is a combination of the oxide of ethyl, with an acid of the same constitution as aconitic acid.

The free aconitic acid is easily soluble in water, alcohol and æther; decomposes on being heated above its melting point, disengaging pyraconitic acid, empyreumatic oil, and leaving behind a black pitchy mass, which subsequently carbonizes entirely.

Itaconic acid (pyraconitic acid) is best obtained by heating the aconitic acid in a retort till the evolution of yellow vapours occurs. It is, further, this acid which forms the oily bands when the second period in the distillation has commenced: carbonic acid and water appear simultaneously with it. It condenses to an oily mass which sometimes passes into a crystalline solid. As soon as yellow vapours and empyreumatic oil begin to appear the distillation is to be stopped. On slowly cooling acicular crystals are formed in it, and the remaining mass no longer solidifies. On being rapidly cooled the oily acid solidifies entirely, and may be obtained pure by solution in six times its volume of water, and slow evaporation; colourless by recrystallization from alcohol or æther. From the aqueous solution it crystallizes in the form given by Baup for the citricic acid. The crystals give off no water at 100° cent., and consist of 46·62 C, 4·56 H, 48·82 O, agreeing perfectly with Baup's analysis of the *acide citricique.* On being heated to its boiling point, it passes into the isomeric volatile citraconic acid. The acid potash salt is easily soluble, crystallizes in small shining laminæ, which lose one atom of water at 100°; the dried salt contains 28·06 per cent. potash. The neutral baryta salt crystallizes in long slender stellate groups, which lose no water at 100°, it contains 54·92 per cent. barytes. The acid salt forms crystalline crusts. Neither salt of lime is distinctly crystalline. The neutral strontia salt resembles that of barytes, and contains 45·69 per cent. strontia. The silver salt forms a white powder nearly insoluble in water; formula $= C^5 H^4 O^3 \dot{Ag}$. The atomic weight determined from the silver salt $= 707·12$, and its symbol $\overline{\dot{It}}$. Itaconic æther had already been described by Malagatti as pyrocitric æther; it is colourless, of aromatic odour and bitter taste; specific gravity = 1·050 at 15°, boiling point = 227°; it is partially decomposed at a higher temperature: it consists of $C^9 H^{14} O^4 = Ae\,O, \overline{It}$.

The same æther has likewise been prepared by Marchand; by continuing the distillation of his mixture for a longer time, he found for it the same constitution.

The *citraconic acid* is identical with Baup's acide citribique

and Robiquet's anhydrous pyrocitric acid. It is obtained
distilling the itaconic acid, which gives water and citracon
acid, which forms the inferior layer. On distilling afresh
is obtained anhydrous: or it may be obtained by treati
the non-crystalline residue remaining from the itaconic ac
with acetate of lead, and decomposing the precipitate wi
sulphuretted hydrogen. The acid is void of colour and sme
has a strong acid acrid taste, sinks in water, does not mix wi
it, but on longer contact soluble in it in every proportio
specific gravity = 1·247 at 14°; begins to volatilize at 90°, boi
at 212° cent.; heated further it leaves a carbonaceous residu
Attracts moisture from the atmosphere, first becoming cr
stalline, then deliquescing. The hydrate crystallizes in fou
sided prisms, which melt at 80°, and volatilize entirely a fe
degrees higher; soluble in water, alcohol and æther. C
being heated in a retort they first give off water, and th
anhydrous acid. The anhydrous acid = 54·12 C, 3·53 H
42·35 O; the crystallized hydrate = 46·62 C, 4·56 H, 48·82
The atomic weight was determined from the anhydrous sal
of silver, lead and barytes, and was found to be 715·40; i
formula, therefore, = $C^5 H^4 O^3$, the symbol $\overline{Ct}$. Citracon
æther is prepared in the same way as itaconic æther, with whi
it agrees in every respect; its formula $C^9 H^{14} O^4 = Ae\,O, \bar{C}$
Anhydrous citraconic acid absorbs ammonia with viole
disengagement of heat, forming a yellow, ductile, transparen
hygroscopic body, easily soluble in water and alcohol. Th
anhydrous citraconate of ammonia = $2\,C^5 H^4 O^3 + N^2 H$
Dissolved in water and evaporated, small crystalline laminæ
the same salt are obtained as crystallizes from the acid sol
tion by saturating direct with ammonia; formula = $2\,C^5 H^4$
$+ N^2 H^6 + H^2 O$. The neutral potash salt is easily solubl
forms a pulverulent mass; the acid salt crystallizes in easi
soluble laminæ. Both soda salts easily soluble, non-crysta
line. The acid salt of magnesia transparent, radiately cr
stalline. The neutral lime salt is amorphous, easy of sol
tion; the acid salt crystallizes in prisms and laminæ; formul
$2\,C^5 H^4 O^3 + Ca\,O, H^2 O + 3\,Aq$; lose one atom of wat
at 100°, three atoms at 120°, and at 140° blacken and evol
acid. The neutral salt of barytes is white, crystalline, of di
ficult solution in cold water; the acid salt forms white silk
verrucous groups, loses no water at 100°, = $2\,C^5 H^4$
$+ Ba\,O\,H^2 O + Aq$. The neutral salt of strontia is n
distinctly crystalline, efflorescent; the acid salt forms large co
lourless prisms, which become opake at 100°, and lose 26·19 pe
cent. in weight; at 120° gives off acid. The silver salt is onl

wn down after an addition of ammonia, as a gelatine of cult solution in water. From its boiling solution it cryizes in long, thin lustrous needles, loses nothing at 100°, burns with slight explosions; formula $C^5 H^4 O^3$ Aq O. n the liquid filtered from this salt are deposited on evapoon crystals in six-sided columns, which at 100° lose one n of water. The citraconate of lead exists in four differstates. From an aqueous solution of the acid, to which e ammonia has been added, acetate of lead throws down luminous precipitate, which on being boiled is partially olved, but the greater portion is converted into an insoe crystalline powder $= C^5 H^4 O^3 + Pb O$. From the fild liquid crystallizes on cooling a non-crystalline powder $C^5 H^4 O^3 + Pb O +$ Aq. If the neutral salt of ammonia recipitated by the neutral acetate of lead a gelatinous preate is formed, which dried immediately becomes opake, ntly yellow and gummy, $= C^5 H^4 O^3 + Pb O + 2$ Aq. ic acetate of lead throws down from citraconatic salts hite nearly insoluble crystalline powder $= C^5 H^4 O^3$ Pb O. The citraconate of zinc, and of the protoxide of cury, are white and nearly insoluble. The neutral salt ickel green, the acid salt green, crystalline. The acid of cobalt red, granular. The acid citraconate of the oxide of manganese forms an opake ductile mass. The rate of the peroxide of iron is but slowly dissolved by aconic acid.

itric acid therefore presents four distinct periods of deposition: the first from the melting point to the evolution as, during which water of crystallization (?) is given off. second period begins with the evolution of aceton and onic oxide, and pyrocitric or aconitic acid is formed. s is decomposed in the following period, under formation carbonic acid and pyraconitic or itaconic acid. In the th period empyreumatic oil is given off, and the residue arbonified. Three different acids therefore originate in d order. The aconitic acid is probably tribasic, the itaic and isomeric citraconic acids bibasic.

Vackenroder has published in the *Archiv der Pharmacie* iii. p. 266–279) a series of experiments on the amount of er of crystallized citric acid, from which he concludes that two kinds of citric acid hitherto admitted with four and atoms of water do not exist, or are never formed from usual citric acid under known conditions; that the crylized citric acid cannot exist with less than three atoms of er; that the aqueous fluid which Crasso obtained by dry illation, and regarded as water of crystallization, is nothing

but hygroscopic water; further, that the citric acid wh
Crasso separated from the residuous acid heated until
disappearance of white vapours, and which he found by a
lysis to be $\overline{Ci} + 3\,\overline{Aq}$, must no longer be considered as cit
acid dried at 100° C., but is the usual crystallized citric a
which has remained unchanged.

Wackenroder likewise contradicts the assertions of Ro
quet and Crasso respecting the insolubility of citric acid
æther: he found that rectified æther, dry as well as mo
dissolved abundantly the melted acid, that the solutions
not become milky in closed vessels, and on evaporation g
a syrup which but slightly reddened dried litmus, and wh
on standing exposed to the atmosphere gradually passed i
the crystallized state.

Before quitting this subject we may notice the observati
of Berzelius (*Arsberettelse*, No. xviii.) on the views of Lie
and Dumas on the constitution of organic acids: he reje
them entirely, and states that the citrate of soda when heated
190° may be a compound of two atoms of citrate of soda, a
one atom of aconate of soda. In a remarkable paper "
certain Questions of the Day in Organic Chemistry," pu
lished in Poggendorff's *Annalen*, xlvii. p. 289, which is pr
cipally directed against Liebig's article "On the Compositi
of Organic Acids," *Annalen der Pharmacie*, xxvi. p. 1
Berzelius observes with regard to citric acid, that he
found that when the citrate of soda dried at 190° C. was d
solved in water and placed aside to crystallize, regular cryst
of citrate but no aconate of soda was produced. As citrate
soda is insoluble in alcohol, but aconate somewhat solub
he tried to extract the latter from the metamorphosed citr
by this means, but he obtained much less than the theo
required. Citrate of silver when just precipitated is wh
and voluminous; it has then the formula $\dot{A}q\, C^4 H^4 O^4$,
by heating, or even by standing in the fluid, it becomes he
vier, granular and crystalline, and has a composition simi
to that of the metamorphosed soda salt. The metamorphos
silver salt was treated with alcohol and a quantity of hydr
chloric acid not sufficient to decompose the whole of it;
mixture was well shaken until there was no more hydr
chloric acid in the alcohol. The alcoholic solution wh
evaporated left a perfectly uncrystalline syrup, which, d
solved in water, let fall a trace of citrate of silver; out of
water was obtained a colourless syrup, similar to the aconi
acid, and which when saturated with carbonate of soda giv
citrate of soda and aconate, which may be extracted by alc

l of 0·833, and the quantity of which fully agrees with that
quired by theory. Three atoms are decomposed, giving

$$2 \text{ atoms } \dot{A}q\,\overline{C} = 2\,(4C\ 4H\ 4O) + 2\,\dot{A}q, \text{ and}$$

$$1 \text{ atom } \dot{A}q\,\overline{Atc} = (4C\ 2H\ 3O) + \dot{A}q$$

The salt $2\,\dot{N}a\,\overline{Ci} + \dot{N}a\,\overline{Atc}$ is a chemical compound, which
en dissolved in water attracts one atom of water, and forms
ee atoms of citrate of soda, &c.

Milk of the Cow-Tree.

We are indebted to Dr. Marchand for an examination of
milk of the cow-tree (*Palo de Vacca*). This vegetable
lk had already been made the subject of inquiry by Bous-
gault and Boldero (*Ann. de Chim. et de Phys.* xxiii. p. 210),
l by E. Solly, Jun. (L. & E. Phil. Mag., xi. p. 542), with whose
per Marchand appears to have been unacquainted. Solly's
actin seems to have been a mixture; he had, however, like-
e recognized its difference from wax. According to the
sent examination, this milk would consist of water, ferment-
ve sugar, lime, magnesia, combined with phosphoric acid,
tic acid (traces), butyric acid (?), resin $C^{10}\,H^{16}\,O$, resin
$H^{32}\,O$, resin $C^{50}\,H^{82}\,O$, so-called wax (Solly's galactin?),
ubstance resembling caoutchouc, $C^{41}\,H^{66}\,O^{3}$. We find,
refore, in the milk of the cow-tree only those substances
ich the milky saps of plants generally contain, but espe-
lly little albumen, and much of a caoutchouc-like substance.
e resins are all compounds belonging to the radical $C^{5}\,H^{8}$.

1. Resin $C^{10}\,H^{16}\,O = 2\,(C^{5}\,H^{8})\,O$. The same composition
copaiva resin (according to Hess, however, this is com-
sed of $C^{40}\,H^{62}\,O^{4}$).

2. Resin $C^{20}\,H^{32}\,O = 4\,(C^{5}\,H^{8})\,O$. Camphor oil, accord-
to Martius, possesses the same composition. In the wax
Ceroxylon andicola Boussingault found a resin to which he
ewise assigns this formula. Perhaps Mulder's anthiar
in is the same.

3. Resin $C^{50}\,H^{82}\,O = 10\,(C^{5}\,H^{8})\,H^{2}\,O$. This composition
fers so much from those of the waxes that it can scarcely
reckoned in this class.

4. Caoutchouc-like substance $C^{40}\,H^{66}\,O^{3} = 8\,(C^{5}\,H^{8})\,H^{2}O$
O^{2}. The remarkable resemblance which this substance
to caoutchouc would lead one to believe that it is
lly identical, but according to Faraday's experiments the
ter contains no oxygen. It is, however, possible, that ca-
chouc in the state in which it is contained in the milky
s is capable of combining both with water and oxygen,
l thus to produce a combination which from its properties
ls to mind its origin. This view would agree perfectly

with Fourcroy's experiments, according to which, caoutchou is precipitated from the milk-saps by the oxidizing action the atmosphere. (*Journal für Praktische Chemie*, **xxi.** 43–54.)

Croconate of Copper.

According to L. Gmelin, when warm aqueous solutions the croconate of potash and hydrochlorate or sulphate copper are mixed, and the liquid let to cool, small rhoml prismatic crystals of the croconate of copper are deposite They have a lively semi-metallic lustre, reflect light dark blu transmit it of a brownish yellow. The powder is of an i tense lemon-yellow colour. On being heated in a tube they gi off water, then decompose violently under evolution of ga the parts driven out of the tube burn with showers of sparl The products of this decomposition are, first, a mixture carbonic oxide and carbonic acid in variable proportion second, an empyreumatic faint yellow acid fluid which sme of pyroligneous acid, colours the salts of iron brown, those silver red (aldehydic acid?); third, a dull brownish-bla coally residue, which on being heated, exposed to the atm sphere, burns lively to metallic copper and then to oxic When the croconate of copper is heated in the atmosphere gradually decomposes with slight explosions and showers sparks. The analysis of the crystals, washed with cold wat and dried in bibulous paper, gave C 23·36, H 2·23, O 43·4 Cu O 31·00. The formula for the crystallized salt is, ther fore, $C^5 O^4 Cu O, + 3 H^2 O$. Of the three atoms of water tv are expelled at 162°, the last does not go off without decor position. (*Annalen der Pharmacie*, xxxvii. p. 58–65.)

The Fatty Acids.

An examination into the action and constitution of a ser of fat bodies has been made under the guidance of Profess Liebig, the results of which have appeared in various nu bers of the *Annalen der Pharmacie* for 1840. The questic to be solved were, do the solid fats, which contain not a tra of margaric acid, give margaric acid on dry distillation? Ho and in what manner, can margaric acid originate from stea or oleic acid? This question presupposed the most accura knowledge of the composition of stearic, margaric and ol acids. Professor Redtenbacher undertook the examinati of stearic acid, and of the acid in the drying oils; Dr. Va rentrapp that of margaric acid, the oleic acid in the grea oils, human fat, olive oil, and in almond oil. Connected w this question was the examination into the origin of seba acid, and of the substance characterized by its penetrati smell, to which Berzelius has assigned the name of Akrole

'he composition and mode of formation of elaidic acid was
ndertaken, as his task, by Dr. Meyer. The accurate ex-
nination of coccostearic acid (coccinic acid), as well as of
ıe products which originate from the action of nitric acid on
t bodies, was undertaken by M. Bromeis: the composition of
ıe margaric acid in palm oil, and of the acid in cacao butter,
y Mr. Stenhouse of Glasgow; on the composition of the fat
ıbstances of the nutmeg butter, and of the wax, by Mr. Play-
ir of Calcutta*.

[To be continued.]

LIX. *Abstract of the History of a new class of Platina-salts, discovered by* M. Gros, *with some Remarks on their true Constitution.* *By* Robert Kane, *M.D., M.R.I.A.*†

THE very singular substance discovered by Magnus, and
formed by boiling protochloride of platina in water of
nmonia, is well known to consist of $Pt\ Cl + NH_3$. By this
rect process it is formed but slowly and in small quantity,
ıd a better mode of obtaining it was invented by Liebig.
solution of perchloride of platina is treated by a current of
ılphurous acid gas, until the liquor becomes deep reddish
rown, and ceases to precipitate a solution of sal-ammoniac.
eing now brought to boil, and water of ammonia being
lded, Magnus's salt precipitates in fine deep green silky
eedles. In this way Gros obtained it in quantity sufficient
r his researches.

When this substance is treated with warm nitric acid it (the
id) becomes brown, and finally converts the salt into a white
owder. Red fumes are given off, and generally some double
erchloride of platina and ammonium produced, and some
etallic platina separated. The white salt is obtained pure
y solution in warm water, from which it crystallizes on cool-
g. This salt is colourless, and in brilliant flat prisms.
eated with caustic potash it evolves ammonia, but not
herwise. It is not precipitated by sulphuretted hydrogen.
n analysis it yields $Pt\ Cl\ N_3\ H_6\ O_6$.

---

* [The authors had subjoined to this notice of the recent investigations
specting the fatty bodies, particulars of some of their results; but as
veral of the memoirs containing them have now appeared entire in our
ges, together with an abstract of others, we deem it best to add merely
e following references:—An abstract of the researches on the fatty sub-
nces by Redtenbacher, Varrentrapp, Meyer and Bromeis, was given in
e Number for February, pres. vol., p. 113, preceded, at p. 102, by Dr. L.
ayfair's paper on a new fat acid in the butter of nutmegs; and in our last
mber, p. 186, appears Mr. Stenhouse's memoir on palm oil and cacao
tter.—Edit.]

† See p. 284, of the present Number.

When a solution of this salt is treated with sulphate of soc a new compound separates in needles. This is also obtain by decomposing the first salt by dilute sulphuric acid, nitı acid being expelled. A solution of this salt yields no pre pitate with nitrate of barytes, but gives sulphate of baryt when an excess of nitric or muriatic acid is added. Its analy gives the formula $Pt\,Cl\,N_2H_6\,SO_4$.

On mixing a warm solution of the nitric acid salt wi oxalic acid, a white powder, insoluble in water, is throw down, which has the composition $Pt\,Cl\,N_2H_6\,C_2O_4$.

With a solution of common salt another compound is g nerated, the composition of which is $Pt\,Cl_2\,N_2H_6$.

When we compare the above formulæ with one anoth we observe that there is common to all, the elements of an ato of protochloride of platina, one of amidogene and one ammonium, $Pt\,Cl + NH_2 + NH_4$, and that the changeab element is such, as to form, with the ammonium, an ordina ammoniacal salt. The simplest rational formula which cou be devised for them, should therefore be to look upon t platina as having been raised to the state of percombination l the nitric acid, a chloramide being formed, $Pt + Cl\,NH_2$, r sembling the bichloride, an atom of chlorine being replac by amidogene, as an atom of oxygen may be replaced chromic acid by chlorine. Then the formulæ of Gros's sal become

1. $(Pt + Cl\,.\,Ad) + (Am\,.\,O + NO_5)$.
2. $(Pt + Cl\,.\,Ad) + (Am\,.\,O + SO_3)$.
3. $(Pt + Cl\,.\,Ad) + (Am\,.\,O + C_2O_3)$.
4. $(Pt + Cl\,.\,Ad) + (Am\,.\,Cl)$.

MM. Gros and Liebig reject this view on the followi grounds: 1st, that the difficulty which is found in expelli the ammonia from these compounds excludes the idea of t presence of an ordinary ammoniacal salt; and 2nd, that t sulphuric acid compound not immediately precipitating a s of barytes, shows that common sulphate of ammonia does n exist therein. Gros therefore considers that the platin chlorine, nitrogen, and hydrogen are all united together in compound radical, which in No. 4. is united to chlorine, a in the others with oxygen and an acid. Thus, Pt $N_2H_6 = X$ we have,

1. $Pt\,Cl\,N_3H_6O_6 = X\,.\,O + NO_5$.
2. $Pt\,Cl\,N_2H_6SO_4 = X\,.\,O + SO_3$.
3. $Pt\,Cl\,N_2H_6C_2O_4 = X\,.\,O + C_2O_3$.
4. $Pt\,Cl_2N_2H_6 = X\,.\,Cl$.

Liebig endeavours to establish a relation between this co pound radical and ammonium, by proposing to consider t radical to consist only of $Pt\,Cl\,N_2\,H_2$; that this fulfils a function like that of azote, and combining with H_3 forms the true basis, which is associated in Gros's salts with hydrated acids or with chloride of hydrogen.

In this confused state of our knowledge of so interesting a class of bodies, I think it right to publish some facts with regard to their origin and constitution which I had discovered a long time ago; for after having completed my researches on the mercurial compounds formed by ammonia, I commenced the analogous examination of the platinum bodies, and had formed many new substances, among others the muriatic and oxalic salts of Gros, when his memoir appeared. I then thought that he would pursue the subject further, but as it would be useless for two persons to be losing time doing the same thing, and I thought that he was ahead of me in time, I laid the subject aside, and passed to other matters. I have lately resumed that investigation, and combining my old results with those which I since obtained, I consider that the nature of those bodies may be now explained.

I formed these salts originally, not as M. Gros did, from Magnus's salt, but by acting with ammonia on the perchloride of platina. There are produced a series of bodies, which it is not my object to describe here, but ultimately a colourless solution, which gives with alcohol a white precipitate, which is the basis of Gros's salts, that he had endeavoured in vain to isolate. Its composition is $Pt\,Cl\,N_2\,H_6\,O + 2\,Aq$. Its solution in water gives with acids all Gros's salts. Now in its formation the series of decompositions from the perchloride $Pt\,Cl_2$ is fully developed. And it is important also that by passing an excess of dry gaseous ammonia over perchloride of platina, Gros's muriate is produced, for its constitution may be represented as $Pt\,Cl_2 + 2\,N\,H_3$. It is evident, therefore, that these complex bodies are deduced from the perchloride of platina just as the complex mercurial amides are formed from corrosive sublimate, and as the ammoniacal sulphates, &c. of silver, zinc, copper, &c. are produced by an excess of ammonia on the simple salts. But the platinum is remarkable for involving together the types of both the mercurial and copper ammoniacal compounds, and hence rising to a still greater complexity of constitution. Thus in place of $Hg\,Cl + Hg\,Ad$ and $Hg\,I + 2\,Hg\,O + Hg\,Ad$, there are in the platinum series $Pt\,Cl_2 + 3\,Pt\,Ad_2 + 4\,Aq$, and $Pt\,.\,I_2 + Pt\,Ad_2 + 4\,Aq$. In combination there is, however, $Pt\,Cl_2 + Pt\,Ad_2$, and it is this which exists in Gros's salts combined with the common salt of ammonia. From these circumstances, I consider that we have good grounds for

looking upon Gros's salts, the sulphate for example, as co
taining the bichloramide of platina united with two equiv
lents of sulphate of ammonia $(Pt\,Cl_2 + Pt\,Ad_2) + 2\,(NH$
$O + S\,O_3)$. As to the objection made by Gros to the exi
ence of common sulphate of ammonia, that neither the ac
nor the base can be directly detected, it is well remarked
Berzelius that the view taken by Gros does not in the slight
degree render it easier of explanation. We find, howeve
many analogous cases. Thus in the double oxalate of chron
and potash, a very trifling quantity of the oxalic acid is show
on the addition of a salt of lime.

I consider that it is not calculated to advance science, whe
in order to avoid difficulties in the rational explanation of t
constitution and properties of any substance, the theory of c
ganic radicals is employed as unreservedly as it appears no
to be by many chemists. It gives an air of simplicity to r
actions, very attractive at first sight, and very convenient f
explanation, but which, by making us satisfied by surface r
semblance or plausibility, may retard important investigation
If, in accordance with Gros's ideas, we make $Pt\,Cl\,N_2\,H_6 =$
the question still remains, what is the structure of X? A
that question should never be lost sight of. But the substan
described by Reiset as the radical of Gros's salts[*], and whi
has indeed the formula of it $(Pt\,Cl\,N_2\,H_6)$, is not so const
tuted; it is not at all of the series of the bichloride of platin
to which Gros's salts belong, but to the protochloride, bein
$Pt\,Cl + 2\,NH_3$, whilst Magnus's salt is $Pt\,Cl + NH_3$. This
evident from the way it is made.

L. *Some Remarks on* Messrs. Francis and Croft's *Abstra*
from the Foreign Journals. By A CORRESPONDENT.

SIR,

IT has lately been the fashion, especially among those w
have spent some time under the great teachers on t
continent, to declaim much against the backward state of ch
mistry in England, and to censure its cultivators in this count
for their tardy reception of the new doctrines and new fac
continually emanating from the fertile inventions and laborio
investigations of their continental brethren.

Perhaps, to a certain extent, the charge may be w
founded, and thanks are due to Messrs. Francis and Cr
for their laudable endeavours to remove this opprobrium.

On reading their abstracts from the foreign journals insert your last Number, I could not, however, avoid being struck h some inconsistencies in the reasonings adduced; and ould these be a sample of what we are generally to meet with, may account in good measure for the reluctance manifested our countrymen to adopt every new theory that may be t forth, even under the sanction of men whose reputation nds deservedly so high. Theories now succeed each other h such surprising rapidity, that scarcely has one been zoned to the world ere another is proposed to supersede and this experiences, it may be, a dominion of still shorter ration.

---

[*] Francis and Croft's Abstracts, p. 284.

Take, for example, the theory of the constitution of alcohol, ich MM. Dumas and Stas, in their last paper, propose to odify: let us consider the views that have been taken, or may taken of its molecular arrangement, and then let us decide, ve can, which view is the true one, or even which may be d to merit the preference.

Alcohol. Empirical formula	$C_4 H_6 O_2$
(1.) Bihydrate of Olefiant Gas	$(C_4 H_3 + H) + 2 H O.$
(2.) Bihydrate of Etherine,	$(C_4 H_4) + 2 H O.$
(3.) Hydrate of oxide of Ethyl,	$(C_4 H_5 + O) + H O.$
(4.) Methylic Aldehyd + Marsh Gas,	$(C_2 H_2 O_2) + C_2 H_4.$
(5.) Bihydruret of Aldehyd, &c. &c.,	$(C_4 H_4 O_2) + H_2.$

Each of these views may be said to be supported by expenent. (1.) Thus, when alcohol and sulphuric acid are mixed certain proportions, and heated, olefiant gas and water are results. (2.) In other proportions, the same ingredients ld etherine, or etherole, and water; (3.) whilst, in again ffering proportions, oxide of ethyl and water are the results.

Thus much the effects of *acid* re-agency on alcohol: MM. umas and Stas have tried the effect of *alkalies* in producing decomposition. The results, as might be expected, are ually various.

Heated with *hydrated* alkalies, acetate of the base is formed d hydrogen evolved. If the temperature be somewhat raised, rbonic acid and marsh gas result; here it is the *hydrated id* that undergoes this decomposition, $C_4 H_4 O_4 = C_2 O_4 + H_4$.

In this instance the formula of acetic acid is represented to as $C_4 H_4 O_4$; a little further on, when mentioning the relts of the transmission of aldehyd, $C_4 H_4 O_2$ over heated lime d potassa, acetic acid is said to be formed by the action, d the formula given is $(C_4 H_3 O_3)$, the true formula for the hydrous acid. Surely we must not admit and discard the esence of water in these cases just when it suits our connience to do so, for by this mode of proceeding, alcohol d ether would become synonymous and convertible terms,

$C_4\,H_6\,O_2 = C_4\,H_5\,O$. In the experiment just described H is given off, and it is concluded that aldehyd is analogous to hydruret of benzule, while a few months ago the opinion of its being a hydrate of oxide of acetyl was promulgated; and both opinions are founded on experiment! but this by the way.

(4.) To return to the theory of alcohol. After stating, as above, the decompositions produced by hydrated alkalies, the abstract (for with that alone I am at present dealing) proceeds —"In the usual preparation of acetic acid, two atoms of hydrogen are replaced by two atoms of oxygen; by the action of alkalies chloracetic acid $C_4\,H\,Cl_3\,O_4$ [the hydrated acid] is decomposed into carbonic acid and chloroform $C_2\,O_4 + C_2\,H\,Cl_3$; acetic acid is decomposed into $C_2\,O_4 + C_2\,H_4$, or $C_2\,O_4 + C_2\,H_3\,H$. Alcohol may therefore [!] be considered as composed of two bodies representing carbonic acid, one the methylic aldehyd, the other marsh gas, $C_4\,H_6\,O_2 = C_2\,H_3$ [intended, I suppose, to be H_4] $+ C_2\,H_2\,O_2 =$ [or analogous to] $C_2\,O_4 + C_2\,O_4$." Why we are to consider alcohol as composed of marsh gas and methylic aldehyd, to me is by no means obvious from these premises. If, however, we are to imagine it as containing a compound of the methyl series, we might, as it seems to me, with as much reason suppose alcohol an oxide of methyl ($C_2\,H_3 + O$) with which it is evidently isomeric. But I forget myself; this supposition is contrary to the theory of types.

The abstract continues: "In the usual method of forming acetic acid $C_2\,H_2\,O_2$ loses H_2 and takes up O_2, there then remains $C_2\,O_2\,O_2 + C_2\,H_4 = C_4\,H_4\,O_4$, or acetic acid. By the action of alkalies H_4 is given off; the water of the hydrated alkali is decomposed, H_2 is developed, and O_2 assimilated." If these changes really occurred, it is evident the result would be an oxalate, not an acetate.

$$\left.\begin{array}{lr}
\text{1 eq. hydrated acetic acid} = & C_4 + H_4 + O_4 \\
\text{2 eq. water from the alkali} = & H_2 + O_2 \\
\hline
 & C_4 + H_6 + O_6
\end{array}\right\} =$$

$$\begin{array}{lcll}
C_4 + & & O_6 & \text{2 eq. oxalic acid.} \\
 & H_4 & & \text{Evolved.} \\
 & H_2 & & \text{Evolved.} \\
\hline
\multicolumn{3}{l}{C_4 + H_6 + O_6} &
\end{array}$$

Hence it appears that the alkalies act, by their affinity for carbonic acid, in a manner analogous to that in which sulphuric acid operates by its affinity for water.

(5.) Lastly, we may consider alcohol a bihydruret of aldehyd, and still appeal to experiment for confirmation of our view;

f alcohol be passed through heated porcelain tubes, the lucts are water, aldehyd and carburetted hydrogen*.

$$\text{s. alcohol} = C_{16}\,H_{24}\,O_8 = \left\{\begin{array}{lccc} \text{2 eqs. aldehyd...} & C_8 & H_8 & O_4 \\ \text{4 eqs. water......} & & H_4 & O_4 \\ \text{1 eq. olefiant gas} & C_4 & H_4 & \\ \text{2 eqs. marsh gas} & C_4 & H_8 & \\ \hline & C_{16} & H_{24} & O_8 \end{array}\right.$$

hydrogen set free from 2 eqs. of alcohol decomposing other two, as here represented. These are but a few of numerous views that might be taken of the constitution of hol. Can we then wonder if considerable deliberation ıld be exercised before closing with any particular theory? o not pretend that some of these views are not more ngly supported by analogies than others; but into these ils it would be out of place for me on the present occa- to enter.

new acid, the *Ethalic*, is described in this paper, the nula of its hydrate is $C_{32}\,H_{32}\,O_4$; this, it will be remarked, lentical with the formula given by Fremy and Stenhouse hydrated palmitic acid.

ince MM. Francis and Croft have undertaken these abs- ts for the benefit of British chemists, I would beg to gest rather more attention to the terminology and ortho- phy of new compounds, a point by no means unimportant, ecially when, as so very frequently happens in the present , fanciful and arbitrary names, some nearly identical with se applied to totally distinct substances†, are imposed. It esirable that in the same paper at least the same things uld possess the same names; that, for example, a body uld not sometimes be called valeric, sometimes valerianic l; nor methyl, mæthyl; ethyl, æthyl, or the like.

t would certainly, moreover, be a convenience, if in these tracts, which we sincerely hope may be continued, the perature, instead of being given on the centigrade scale y, were expressed likewise in corresponding degrees of renheit, as being the scale generally used here.

With regard to the symbols mentioned in the foot note at 200, in which it is queried whether the plan of representing constitution of organic bodies merely by the *position* of figures, as proposed by Berzelius, be known in England, ely on the present occasion we may rejoice in our igno- ce. Our symbols are already sufficiently abridged to di-

---

Turner's Elements of Chemistry, Gregory's edition, p. 859.

For example, ammelin, ammelid; phlorizine, phlorizeine; benzile, zole, benzule; and the chlorovalerisic and chlorovalerosic acids of the ıors.

minish their usefulness. We do not require abbreviati of abbreviations. Mistakes arising from this cause are alre too numerous. How liable they would be to multiply w we still further to complicate our systems of notation, may imagined by reading the following sentence from the abstr already so often quoted; where, speaking still of the tion of hydrated alkalies on various substances, it is sa "Acetic æther gives hydrogen and acetic acid; benz æther gives hydrogen, benzoic and acetic acids; iodide ethyl gives iodide of potassium and olefiant gas, C_4 H [should be I] + $\dot{K} = C_4 H_4 + \dot{K}I^2$ [should be I, and the dot o the K omitted] + HO. This æther forms with chlorine; ch ride of ethyl and iodine is separated; $\frac{4}{10}$ E F is exactly simi to RF." Here neither R nor F are defined.

I am, Sir, your obedient servant,

To R. Phillips, Esq., F.R.S. &c. &c. W. A. M

March 5, 1841.

Note.

We do not find in the communication of our Berlin Correspond any thing implying unwillingness on the part of English chemists to rec "new doctrines and new facts" from abroad. Messrs. Francis and C merely offer to assist us in supplying information which the chemists of own country are, we believe, anxious to obtain. Whatever caution ma necessary in the reception of new *doctrines,* correct information respec them must be desirable; and still more so with regard to new *fa* Neither, if there are "inconsistencies in the reasonings" of Dumas and S is it logical to infer "that these are a sample of what we are generall meet with" among continental chemists; some of whom, indeed, are opponents of those reasonings. It is our province to assist in brin them under examination.

With regard to the remarks on the Symbolic Notation, we have to s that our Correspondents are not accountable for any ambiguity wh may appear in the formulæ given in that portion of their communica which appeared last month. Some of those formulæ were altered by with the intention to suit the views generally taken by English chemi In the continuation given in the present number, however, we have ferred leaving the symbols exactly as written by Messrs. Francis and Cr and to this practice we intend in future to adhere. As to the ter nology, we presume that in preparing the abstracts, they have retai the terms employed in the original papers, except in cases where English equivalents were obvious. For want of uniformity in orthog phy we are alone to blame. Much diversity exists among our chem writers; but our practice has been to prefer, as usual in English, Latin orthography of Greek terms, and not to present them in a Fre garb. The advantage of forming terms from the Greek is lost by so di guring them as that they are scarcely to be recognised. We theref prefer æther, æthyl, &c., more especially as we have not, like the Fren the accented é to substitute for the diphthong; nor the ä as in Germ We are fully aware that with the present immense influx of new nam the subject of chemical nomenclature requires strict critical supervisi and we trust it may engage the further attention of Professor Whew who has so ably treated certain parts of the subject in his Philosophy the Inductive Sciences.—Edit.

LI. *On the Cause of the Production of Daguerreotype Pictures.*
By MARTYN J. ROBERTS, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

ALTHOUGH the Daguerreotype process has long excited intense interest in the scientific world, we have not as yet had an explanation of the wonderful effects produced. I believe that even M. Arago has failed to elucidate the theory; and not only has this great philosopher been foiled, but the inventor of the process is unable to solve the problem; with these facts before me, I feel it almost presumptuous in so humble a votary of science as myself to attempt an explanation of the cause of the production of Daguerreotype pictures, but if my attempt has the effect of directing attention to the right path for arriving at a solution of the question, I shall be satisfied. We all know that light has a powerful influence on crystallization; solutions that will not crystallize in the dark instantly form on the admission of light; the crop of crystals is always more copious on the enlightened side of a glass containing a crystallizing liquid than on the dark side. Ice forms more rapidly during moonlight, and on the break of day, than on a dark night; but I need not adduce examples of the influence of light on crystallization, for the fact is allowed by all scientific men.

Let us then suppose, that in the Daguerreotype process the cleansed silver plate is exposed in the dark to the vapour of iodine; this deposits itself in a flocculent or powdery state on the plate, unable to form the peculiarly shaped iodic crystals, from the absence of light; but yet all other requisites being present, it may be considered in an incipient state of crystallization, or balanced so finely, that the admission of the excitant light instantly throws it into plate-formed iodic crystals, but only in those parts where the light has impinged, and here its perfection of, or continuity of crystallization, is merely in proportion to the intensity of light.

Having now the iodated plate removed from the camera obscura, where it has undergone a surface crystallization, more or less perfect in those parts where the lights and shades have fallen, we submit it to the mercurial vapour; the atoms, vesicles, or globules of this vapour being very minute, attach themselves to all the minute inequalities of face in the iodine; on those parts which are fully crystallized, the vapour is precipitated on the flat tabular surface of the crystals, and here offering a continuous and equal angle of reflection to the eye it appears white and resplendent. On the non-crystallized imperfectly crystallized surface of the iodine, which being a measure powdery and offering no determinate angle of flection to the eye, the mercurial vapour adheres, but in flat surface or continuous determinate angle capable of flecting a mass of light; it may be said it is here unpolishe

Again, may not the angle under which it is necessary view a Daguerreotype picture be that of the facet of the io crystal, and this be a further confirmation of my theory? T mercurial vapour covers the whole of the iodine, and th protects it from the further action of light.

Such are the crude views I have formed on this subject, a I trust they may lead to a further elucidation.

I am, Gentlemen, yours, &c.

London, March 19, 1841. MARTYN J. ROBERT

LII. *Notices respecting New Books.*

A System of Crystallography. By JOHN JOSEPH GRIFFIN. Glasg. 18

WE have been favoured with a copy of a work bearing the ab title, but which might better have been called "A descriptio 120 porcelain models of crystals, according to a method newly vented by the author"; for, with regard to Crystallography, we mig on first turning over the uncut leaves, and meeting with zenith nadir of crystals, and north-east, north-west, and other meridia have supposed that the object of the book was to create a la at the expense of the cultivators of that science; but recollecting cost of publishing a rather bulky octavo volume, we felt compel to suppose the author serious, and we accordingly began at preface to separate some of the leaves.

The first line of the treatise defines crystallography to be "the of describing crystals." But this, according to our notions on subject, is only one, and not the most important of its objects. have been accustomed to regard crystallography as a science c much higher order, by which the mineralogist is enabled, fron fragment of a crystal, to discover its relation to some simple t by which he may connect it with the species of mineral to whic belongs.

This view of the subject, however, does not appear from the ab definition to have entered into Mr. Griffin's contemplation, and system therefore, even if it possessed the merit he ascribes to it, wo render no additional service to mineralogy.

We cannot afford either time or space for even a brief analysi the author's method, nor do we conceive that we should much nefit our readers by giving it; we shall therefore limit oursel to a few extracts and remarks.

The preface begins thus: "There are many systems of crys lography in print, but none in general use," a fate from which may confidently predict the work before us will not be exempt. "The different systems," it proceeds, "hitherto published have failed to satisfy the wants of the *public*." We do not know to what *public* Mr. Griffin refers. Surely not to those by whom the tens of thousands of copies of 'Nicholas Nickelby' and 'Jack Sheppard' are eagerly purchased, or the novels of fashionable life as eagerly perused; and if not to these, what reading public is to be found, and more especially where the cultivators of science, whose *wants* the existing works on Crystallography have failed to satisfy?

The different systems to which our author alludes are, he continues, "either too difficult to learn, or, when learnt, too troublesome for service. Hence," he says, "crystallography is little studied by chemists or mineralogists; and the consequence is, that for want of *a language in which observations can be recorded*, the study of crystallization is also shunned." Our mineralogical readers must know that this statement is unfounded; and we can only account for its having dropped from the pen of Mr. Griffin, by supposing the preface to have been written after the work was finished, and the author to have entirely forgotten not only a work to which he has so frequently referred, but even the name of William Phillips.

The preface proceeds to state that "the present publication is an attempt to show the way, not merely how to describe a crystal, but how to do it *easily*."

The following are examples of the *simple* and *easy* method of description employed by the author:

54. TURNERITE. Pictite. Cleavage=m, T.

5. 5. t, m $\frac{2}{20}$ t, M$\frac{2}{10}$T, m$\frac{2}{3}$t. $\frac{1}{2}$P$\frac{1}{4}$M Z^2n, $\frac{1}{2}$P—M Zn^2, $\frac{1}{2}$P—M Zs,
$\frac{1}{2}$P—M$\frac{2}{10}$ T Zn^2e Zn^2w, 5 ($\frac{1}{2}$p—m_xt) Zne^2 Znw^2,
2P_xM$\frac{2}{10}$T Zs^2e Zs^2w, $\frac{1}{2}$p + m$\frac{2}{3}$t Zse^2 Zsw^2, ... $P84^2$.

5. 5. T, M$\frac{2}{20}$T, M$\frac{2}{10}$T, m$\frac{2}{3}$t. $\frac{1}{2}$P$\frac{1}{4}$M Z^2n, $\frac{1}{2}$p—m Zs,
3p—m_xt Zne Znw, p—m_xt Zse Zsw, ... $Ly82^2$.

Part II. p. 91.

ANORTHITE.

5. 3. T, $\frac{1}{2}$M$\frac{4}{8}$T nw, $\frac{1}{2}$M$\frac{5}{8}$Tne. $\frac{1}{4}p_xm_yt_z$ Znw^2, $\frac{1}{4}P_xM_yT_z$ Z^2ne,
$\frac{1}{4}p_xm_yt_z$ Zn^2e, $\frac{1}{4}p_xm_yt_z$ Zne^2, $\frac{1}{4}p_xm_yt_z$ Zs^2e^2, $\frac{1}{4}p_xm_yt_z$ Zs^2e,
$\frac{1}{4}p_xm_yt_z$ Zs^2w^2, ... $R3^{30,31}$.

5. 3. T, $\frac{1}{2}$M$\frac{4}{8}$T nw, $\frac{1}{2}$M$\frac{5}{8}$T ne. $\frac{1}{4}p_xm_yt_z$ Znw^2, $\frac{1}{4}P_xM_yT_z$ Z^2ne,
3 ($\frac{1}{4}p_xm_yt_z$ Zne), 4 ($\frac{1}{4}p_xm_yt_z$ Zse), 2 ($\frac{1}{4}p_xm_yt_z$ Zsw), ... $R3^{32\,33}$.

5. 5. T, $\frac{1}{2}$M$\frac{4}{8}$T nw, $\frac{1}{2}$M$\frac{5}{8}$T ne. $\frac{1}{4}p_xm_yt_z$ Znw^2, $\frac{1}{4}P_xM_yT^z$ Z^2ne,
$\frac{1}{4}p_xm_yt_z$ Zn^2e, $\frac{1}{4}p_xm_yt_z$ Zne^2, ... $R3^{28},^{29}$.

5. 5. T, $\frac{1}{2}$M$\frac{4}{8}$T n^2w, $\frac{1}{2}$m + t nw^2, $\frac{1}{2}$ $\frac{5}{8}$T n^2e, $\frac{1}{2}$m + t ne^2,
$\frac{1}{4}p_xm_yt_z$ Znw^2, $\frac{1}{4}P_xM_yT_z$ Z^2ne, $\frac{1}{4}p_xm_yt_z$ Zne^2,
4($\frac{1}{4}p_xm_yt_z$ Zse), 3 ($\frac{1}{4}p_xm_yt_z$ Zsw), ... $R3^{34}$.

Part II. p. 93.

Here Z stands for zenith.
N ,, ,, nadir.
e, w, n, s, east, west, north, south.
P M T denote faces, p, m, t, axes.

And in p. 2 of the first part we find that the subscript indice x, y, z, imply *unknown* or *variable* axes.

The indices representing *powers* of the quarters of the compass a thus *explained* in p. 24 of Part I.:

" In *Phillips's Mineralogy*, p. 174, there is a figure of a crystal Fluorspar, which has 32 vertical planes, the arrangement of which as follows, taking the nw quadrant as an example :

$$\begin{array}{ccccccccc} \mathrm{n} & \mathrm{n^4w} & \mathrm{n^3w} & \mathrm{n^2w} & \mathrm{nw} & \mathrm{nw^2} & \mathrm{nw^3} & \mathrm{nw^4} & \mathrm{w} \\ \mathrm{M}; & \mathrm{MT}\overset{+}{++}, & \mathrm{MT}\overset{+}{+}, & \mathrm{MT}+, & \mathrm{MT}, & \mathrm{M}+\mathrm{T}, & \mathrm{M}\overset{+}{+}\mathrm{T}, & \mathrm{M}\overset{+}{++}\mathrm{T}, & \mathrm{T}. \end{array}$$

" The signs $+$, $\overset{+}{+}$, $\overset{+}{++}$, in all these symbols, indicate the gre greater, and greatest *distance* of the poles of the axes referred while the figures 2, 3, 4 indicate the great, greater, and great *proximity* of the poles to whose symbol they are added."

It would have been more satisfactory to us to have found occasi to commend this performance, instead of regretting as we do that much time and labour should have been employed upon a system little inviting in its notation, and affording so small a chance finding readers. ***

Elements of Chemistry, including the most recent discoveries and plications of the Science to Medicine and Pharmacy, and to Arts. By Robert Kane, M.D., M.R.I.A., Professor of Natu Philosophy to the Royal Dublin Society, and of Chemistry to Apothecaries' Hall of Ireland, &c. Part I. Dublin, 1840, 8 pp. 356, with 120 wood-cuts.

The following is an analysis of the first part of this work.

Introduction.—Objects, Utility, and Origin of Chemistry.

Chapter I. Of Gravity and Cohesive Forces as characterizi Chemical Substances.—Specific gravity of Liquids, Gases and Soli Constitution of Matter. Infinite Divisibility. Ultimate Particles of Matt Molecular Constitution. States of Aggregation. Limits of Cohesi Capillarity. Elasticity of Gases. Correction for Pressure. Liquefacti of Gases. Solubility. Phænomena of Solution. Formation of Cryst by Fusion and Solution. Dimorphism. Crystallization. Primary and condary Forms of Crystals. Systems of Crystallization. Regular Syste Hemihedral Forms. Rhombohedral System. Square, Right, and Obli Systems. Doubly Oblique System. Pseudomorphism. Forms modif by Foreign Bodies. Isomorphism. Goniometers.

Chapter II. Of the Properties of Light as characterizing Ch mical Substances.—Simple Refraction. Double Refraction. Crystall Systems. Decomposition of Light. Prismatic Colours. Analysis Absorption. Natural Colours of Bodies. Polarization of Light. Acti of Crystallized Bodies on Polarized Light. Crystalline Systems kno by Polarized Light. Macled Crystals. Circular Polarization. Rotat Power of Liquids. Plagihedral Crystals. Wave Theory of Light. Int ference of Light. Phosphorescence. Chemical Agencies of Light. C mical Rays in the Spectrum.

Chapter III.—OF HEAT CONSIDERED AS CHARACTERIZING CHEMICAL
BSTANCES.

Sect. I. *Of Expansion.*—Repulsive power of heat. Influence of co-
sion on expansion. Of the measure of heat. Nature of temperature.
onstruction and use of air-thermometers. Mercurial thermometer, its
inciple of correction. Determination of the standard interval. Con-
uction of the various thermometric scales. Estimation of tempera-
res above the boiling point of mercury. Daniell's pyrometer. Nobili's
ermo-multiplier. Table of temperatures. Expansion of air. Results
tained by Dalton, Gay-Lussac, and Dulong and Petit. Rudberg's cor-
tion of them. Correction of volumes for change of temperature. De-
rmination of the expansion of liquids. Expansibility of liquids changes
th the temperature. Determination of the expansion of solids. Ex-
nsibility of solids increases with the temperature: exceptions to this
le. Effects of the expansion of compound metallic bars. Metallic
ermometer of Breguet. Compound pendulum.

Sect. II. *Of Specific Heat.*—Method of mixtures. Process of Dulong
d Petit. Researches of Lavoisier and Laplace. Calorimeter. Relation
specific heat and chemical constitution. Specific heats of atoms.
eat by chemical combination. Determination of the specific heats of
ses.

Sect. III. *Of Liquefaction.*—Latent heat. Absorption of heat during
uefaction. Heat evolved in solidifying. Cold by liquefaction. Arti-
ial cold produced by frigorific mixtures. Nature of special heat.

Sect. IV. *Of Vaporization.*—Latent heat of vapours. Vaporization
companied by great increase of volume. Determination of the elasti-
ies of vapours. Relations of vapours to temperature and pressure.
ture of the boiling point. Alterations of the boiling point with the
perincumbent pressure, and the nature of the vessel. Apparent anoma-
s property of liquids. Sum of latent and sensible heat constant.
tificial cold produced by the evaporation of liquids. Spontaneous
aporation. Gases and vapours offer no resistance to each other's elasti-
y. Correction for moisture. Hygrometers. Moist bulb-hygrometer.
niell's hygrometer. Analogy of solution to vaporization. Steam as a
ving power. Similarity of constitution of gases and vapours. Specu-
ion on the boiling points of the condensed gases.

Sect. V. *Of the Transmission of Heat through Bodies.*—Conduction of
at. Relative conducting power of solids and liquids. Communication
heat by gases. Communication of heat by diffusion and radiation.
operties of radiant heat. Of the radiating, absorbing, and reflecting
wers of bodies. Researches of Melloni and Forbes on radiant heat.
flecting power of bodies as determined by Buff. Permeability of bodies
heat from sources of different temperatures. Analogy of heat to co-
red light. Polarization of heat. Relations between the physical con-
utions of heat and light. Change of refrangibility of heat.

Sect. VI. *Of the Cooling of Bodies.*—Equilibrium of temperature. Theory
dew and frost. Sources of terrestrial heat. Central heat of the Earth.

Chapter IV.—OF ELECTRICITY CONSIDERED AS CHARACTERIZING CHE-
AL SUBSTANCES. Nature of electricity. Statical and dynamical elec-
city.

Sect. I. *Of Statical Electricity.*—Electricity produced by friction.
nductors and insulators. Relative conducting powers. Velocity of
tion of electricity. Distribution of electricity. Opposite conditions
excitation. Gold-leaf electroscope. Electrical attractions and repulsions.
utralization of opposite electricities. Law of electrical attractions.
eories of electricity. Theory of two fluids. Theory of one fluid.

Electrical machines. Theory of the electrical machine. Quadrant electrometer. Various modes of excitation. Excitation by induction. Theory of the prime conductor. Theory of the gold-leaf electroscope. Doublers and condensers. Construction and theory of the Leyden jar. Electrophorus of Volta. Induction an action of contiguous particles. Specific inductive capacity. Lateral inductive action. Difference between conducting and non-conducting bodies. Relation of induction to conduction. Other sources of statical excitation. Atmospheric electricity.

Sect. II. *Of Dynamical Electricity.*—Magnetic electricity, thermo-electricity, chemical electricity, animal electricity. Galvanic electricity. Conditions of the generation of a galvanic current. Simple galvanic circles. Connexion of galvanic and chemical action. The direction of the current determined by the chemical action. Principle of electrotype copying. Compound voltaic circles. Theory of the voltaic battery. Relation of intensity and quantity. Statical action of the battery. Volta's theory of contact. Chemical theory of the battery. Various forms of galvanic batteries. Interfering action of common zinc. Amalgamation of zinc plates. Forms of constant batteries by Mullins, Daniell and Grove. Relative conducting powers of bodies for voltaic electricity. First discovery of galvanism. Various kinds of galvanoscopes. *Thermo-electricity.* Thermo-electric currents. Production of cold by electricity. Construction of the thermo-multiplier of Nobili. *Magnetic electricity.* Magnetism a form of electricity. Magnetic properties of iron and steel. Magnetic attraction and repulsion. Intimate structure of magnets. Magnetic properties of a galvanic current. Attraction and repulsion of currents. *Electro-magnetism.* Action of a galvanic current on a magnet. Astatic combinations of magnets. Construction of the galvanometer. Magneto-electric induction.

Chapter V.—Of Chemical Nomenclature.—Table of simple bodies and their symbols. General principles of the Lavoisierian nomenclature. Construction of the names of simple bodies and primary compounds. Construction of the names of various classes of primary compounds. Names of secondary compounds. Ternary and quaternary compounds. Symbolical nomenclature.

Chapter VI.—Of Chemical Affinity, and its relations to Heat, to Light, and to Cohesion.—Nature of chemical affinity. Principles of elective decomposition as characteristic of chemical affinity. Order of elective decomposition. Of double decomposition. Of quiescent and divellent forces. Order of affinity not constant. The power of affinity influenced by external modifying causes. Characteristic distinctions between affinity and cohesion. Diversity of chemical properties equally a cause and an effect of chemical action. Influence of external physical agents upon chemical affinity. 1. *Influence of cohesion.* Influence of cohesion on the order of chemical decomposition. Mode of arrangement of acids and bases which co-exist in solution. Distribution not invariable. 2. *Influence of elasticity.* Change in the order of decomposition produced by cohesion and elasticity. Change in the order of decomposition produced by various modifying causes. Relation of affinity to neutralizing power. *Influence of light on chemical affinity.* Photographic drawing. Colouring effects of the chemical rays. Interference of the chemical rays. Theory of the formation of Daguerreotype images.

Chapter VII.—Of the Light and Heat disengaged during Chemical Combination.—Nature of combustion. Products of slow combustion. Aphlogistic lamp. Action of spongy platina on gaseous mixtures. Construction of the platina gas-lamp. Constitution of flame. Blowpipe. Heating effects of flame. Cooling effects of apertures. Construction of the

ety-lamp. Determination of the quantity of heat evolved in perfect
mbustion. Experiments of Despretz and Hess. Theories of combustion
oposed by Lavoisier and others. Heat of combination derived from the
ange of specific heat in combustion.

Chapter VIII.—Of the Influence of Electricity on Chemical
finity.—Electro-chemical decomposition. Chemical affinity is elec-
cal attraction. Electro-chemical classification. Electro-chemical theories
oposed by Davy, Ampère, and Berzelius. Elements evolved not on
racting poles, but on the limiting surfaces of the liquid. Internal mo-
ular mechanism of electro-chemical decomposition. Phænomena of
nsfer described. Of electrolysis and electrolytes. Electro-chemical
uivalents. The chemical voltameter. Molecular arrangement of the
ments of the generating cell. Theory of galvanism. Origin of the gal-
ic current. Compound bodies formed by the current. Agency of weak
rrents in the production of compound bodies. Synthetic action of
ctricity. An electro-chemical theory proposed by the author. Theory
the relations of electricity and affinity. Electro-chemical theory pro-
sed by Becquerel. Of the measure of affinity.

Chapter IX.—On the Laws of Combination.—Numerical determi-
tion of chemical equivalents. Scales of chemical equivalents. Table of
e equivalents of simple bodies. Double decomposition occurs in equiva-
t proportions. Equivalents of compound bodies. Law of multiple
oportions. Researches of Pronot on the definiteness of composition.
ethods of determining the equivalent constitution of bodies. Law of
ltiple proportions. Theory of volumes. Equivalent volumes of simple
l of compound bodies. Calculations of the theoretical specific gravities
compound vapours.

Chapter X.—Of the Relations of Chemical Constitution to the
olecular Structure of Bodies.

Sect. I. *Of the Atomic Theory.*—Dalton's theory of combination. Phy-
al and chemical atoms.

LIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 141.]

c. 10, 840. A MEMORANDUM, addressed to the Royal Society, November 28th, 1840, by Martin Barry, M.D., F.R.S.,
& Ed., was read.

Dr. Barry, in reference to the memorandum of Mr. Wharton Jones,
iming for himself the contemporaneous discovery of the germinal
ot in the mammiferous ovum, states that, after having bestowed
nsiderable pains to ascertain who was the original observer of a
ucture which has proved to be of great importance, he had men-
ned incidentally in his paper the result of his inquiry, namely, that
e merit of the discovery was due to Professor Rudolph Wagner; but
serves that the inquiry may be resumed by all who will take the
uble to examine the works, both in German and English, on this
bject; and that he will ever be open to conviction, and ready to de-
re his change of opinion, on the production of sufficient evidence.

A communication was also read, entitled "Supplementary Note
a Paper, entitled 'Researches in Embryology. Third Series;

a Contribution to the Physiology of Cells.'" By Martin Barry, M. F.R.S. L. & Ed.*.

In the paper referred to, the author had shown, that after the ovu of the Rabbit has entered the Fallopian tube, cells are found c lected around its thick transparent membrane or " zona pellucida which cells, by coalescing, form a thinner membrane—the incipie chorion. He now adds, that the formation of this thinner membra does not exhaust the whole layer of these cells; but that a stratu of them is found remaining on, and entirely surrounding the " zona after the thinner membrane has risen from it. The fluid space als between the "zona" and the thinner membrane, presents a lar number of cells or discoid objects, each of which contains a bri liantly pellucid and highly refracting globule. In some parts, seve of these discs, closely joined together, have the appearance of shre of membrane; in others, there are found pellucid globules, some which are exceedingly minute. The discs now mentioned coll at the periphery, for the thickening of the chorion. They seem proceed from the region of the "zona;" and probably have th origin in the cells by which the latter is surrounded. If so, the a thor thinks we cannot suppose them to arise in any other way th that which, according to his observations, appears to be the univer mode of reproduction; namely, by division of the nuclei of the p rent cells. Nor can we suppose that minuteness is any hinderan to their subsequent increase by the same means.

December 17, 1840.—1. "Present state of the Diamond Mi of Golconda." By T. J. Newbold, Esq., of the Madras Army, A.D. to Major-General Wilson, K.B. Communicated by S. H. Christ Esq., M.A., Sec. R.S.

The author gives an account of the tract of country in which t diamond mines of Golconda are situated, and of the processes which the diamonds are obtained. The latter consist merely digging out the rolled pebbles and gravel, and carrying them to sm square reservoirs, raised on mounds, having their bottom paved w stones, and then carefully washing them. Dry weather is select to carry on these operations, in order to avoid the inconvenience a expense of draining. A description is then given of the mines Banaganpully, Munimudgoo, Condapilly, Sumbhulpoor, and Poon in Bundlekund.

2. " Magnetic-term Observations made at Milan." By Profess Carlini, Director of the Observatory at that place: also " Magnet term Observations made at Prague." By Professor Kreil, Direct of the Observatory at that place.

3. " On the Production of Heat by Voltaic Electricity." By J. Joule, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

The inquiries of the author are directed to the investigation of t cause of the different degrees of facility with which various kinds metal, of different sizes, are heated by the passage of voltaic ele tricity. The apparatus he employed for this purpose consisted of l of the wire, which was to be subjected to trial, placed in a jar water, of which the change of temperature was measured by a very sible thermometer immersed in it; and a galvanometer, to indicate quantity of electricity sent through the wire, which was estimated the quantity of water decomposed by that electricity. The consion he draws from the results of his experiments is, that the cafic effects of equal quantities of transmitted electricity are protional to the resistances opposed to its passage, whatever may be length, thickness, shape, or kind of metal which closes the cirt: and also that, *cæteris paribus*, these effects are in the duplicate io of the quantities of transmitted electricity; and consequently in the duplicate ratio of the velocity of transmission. He also ers from his researches, that the heat produced by the combustion zinc in oxygen is likewise the consequence of resistance to electric duction.

---

* [An abstract of Dr. Barry's Third Series of Researches in Embryolo appeared in L. and E. Phil. Mag., vol. xvi. p. 526; vol. xvii. p. 385.—Edi

The Society then adjourned over the Christmas recess, to meet in on the 7th of January next.

January 7, 1841.—The following communication was read, viz.— " Variation of the Magnetic Declination, Horizontal Intensity, l Inclination observed at Milan on the 23rd and 24th December 40." Communicated by Professor Carlini, Director of the Milan servatory.

A paper was also read, entitled, " On the Chorda dorsalis." By artin Barry, M.D., F.R.SS. L. & E.

The author of this communication, after pointing out the similarity appearance between an object noticed by him in the mammiferous um, and the incipient chorda dorsalis described by preceding obvers in the ova of other Vertebrata, mentions some essential difences between his own observations and those of others as to the ture and mode of origin of these objects, and their relation to surnding parts. Von Baer, the discoverer of the chorda dorsalis, scribes this structure as " the axis around which the first parts of fœtus form." Reichert supposes it to be that embryonic struce which serves as " a support and stay" for parts developed in o halves. The author's observations induce him to believe that, tead of being " the axis *around which* the first parts of the fœtus m," the incipient chorda is the last-formed row of cells, which ve pushed previously-formed cells farther out, and that, instead of ng merely " a support and stay" for parts developed in two lves, the incipient chorda occupies the centre out of which the wo halves" originally proceeded as a single structure, and is itf in the course of being enlarged by the continued origin of fresh bstance in its most internal part.

The author enters into a minute comparison of the objects in quesn; from which it appears that the incipient chorda is not, as Baer pposed, developed into a globular form at the fore end, but that linear part is a process from the globular; and that the pellucid vity contained within the latter—a part of prime importance, being main centre for the origin of new substance—is not mentioned Von Baer. Farther, that the origin of the " laminæ dorsales " of this naturalist (the "central nervous system" of Reichert) is r simultaneous with, but anterior to, that of the chorda.

The author then reviews the observations of Rathke and Reich on the chorda dorsalis, which contain internal evidence, he thin of a process in the development of Fishes, Reptiles, and Birds, t same as that which he has observed in Mammalia; namely, the o gin of the embryo out of the nucleus of a cell.

And it is his opinion that this observation may assist to solv question on which physiologists are not agreed; for it shows, that the nucleus of a cell is a single object, the first rudiments of t embryo are not two halves. The author thinks that unless the ve earliest periods are investigated, it is in vain that we attempt learn what that is, of which the rudiments of the embryo are co posed. From not attending to this, physiologists have suppos their "primitive trace" to arise in the substance of a membra which the author, in his second series on the embryo*, showed co not be the case. To the same cause he thinks is referable an opini recently advanced by Reichert, that the first traces of the new bei are derived from cells of the yelk.

January 14.—A paper was read, entitled, "On the Corpuscles the Blood." Part II. By Martin Barry, M.D., F.R.SS. L. and E

The observations recorded in this memoir are founded on an e amination of the blood in every class of vertebrated animals, in so of the Invertebrata, and in the embryo of Mammalia and Birds. T nucleus of the blood-corpuscle, usually considered as a single obje is here represented as composed, in some instances, of two, thr or even many parts; these parts having a constant and determin form. In the substance surrounding the nucleus, the author frequently been able to discern, not merely "red colouring matte but cell-like objects; and he points out an orifice as existing at c tain periods in the delicate membrane by which this substance surrounded. In a former memoir he had differed no less from p vious observers regarding "cells." He had shown, for instan that the nucleus of the cell, instead of being "cast off as usele and absorbed," is a centre for the origin, not only of the transit contents of its own cell, but also of the two or three principal a last-formed cells, destined to succeed that cell; and that a separati of the nucleus into two or three parts, is not, as Dr. Henle had su posed in the case of the Pus and Mucus-globule (the only instan in which the separation in question had been observed), the eff of acetic acid, used in the examination,—but that such separatio natural, apparently common to nuclei in general, and forming p of the process by which cells are reproduced. The author h farther shown the so-called nucleolus to be not a distinct object e isting before the nucleus, but merely one of a series of appearan arising in succession, the one within the other, at a certain part the nucleus, and continuing to arise even after the formation of t cell. These views he now confirms; and in the present paper shows that they admit of being extended to the corpuscles of the blood.

---

* [See L. and E. Phil. Mag., vol. xiv. p. 493.—Edit.]

† [For a notice of Part I. of this memoir, see vol. xvii. p. 300.—Edi

He then compares appearances observed in the latter with those he had traced in the ovum. These relate to the number of parts of which the nucleus is at different periods composed,—the nature of the nucleolus,—the communication between the nucleolus and the exterior of the cell,—the formation of the contents of the cell out of the nucleus,—the final division of the nucleus into the foundations of a limited number of young cells, destined to succeed the parent cell,—and the escape of the young cells for this purpose. It follows from these investigations, that the corpuscles of the blood are generated by a process essentially the same as that giving origin to those cells which are the immediate successors of the germinal vesicle, or original parent cell; it being also by a continuation of the same process that the corpuscle of the blood divides itself into the minuter objects figured by the author in his former paper on the blood.

He adds, that in its form and internal state, the blood-corpuscle found in the adult of certain animals, very much resembles that existing only in the fœtal life of others. It is incidentally remarked, that the fœtal brain, at certain periods, appears to consist almost entirely of objects very much resembling those which, in some stages, form the nuclei in the fœtal corpuscles of the blood.

The author concludes, by expressing his opinion, that the mode of evolution of the minute mammiferous ovum is deserving of close attention, in connexion with some of the processes by which nourishment is communicated, and the growth of the body effected, at all future periods of life.

January 21.—A paper was in part read, entitled, "On the action of certain Inorganic Compounds, when introduced directly into the Blood." By J. Blake, Esq., M.R.C.S. Communicated by P. M. Roget, M.D., Sec. R.S.

GEOLOGICAL SOCIETY.

[Continued from vol. xvii. p. 542.]

April 29, 1840.—A paper was first read, "On a few detached places along the coast of Ionia and Caria; and on the island of Rhodes;" by William John Hamilton, Esq., Sec. G.S.

The localities described in this paper are, 1. Fouges (anc. Phocæa); 2. Ritri (anc. Erythræ); 3. Sighajik (anc. Teos); 4. Scalanuova, near Ephesus; 5. Boodroom (anc. Halicarnassus); 6. Cnidus; 7. Island and shores of the Gulf of Syme; and 8. Rhodes.

1. *Fouges* is situated in a small bay at the northern extremity of the Gulf of Smyrna, and all the formations in its neighbourhood examined by Mr. Hamilton are volcanic. On the north side of the bay, a range of hills, from 300 to 400 feet high, extends several miles o the eastward, and consists in the uppermost part, of beds of smooth semivitrified red and gray trachyte, containing numerous cavities ined with mammillated chalcedony. The trachyte passes downwards into a soft, white, pumiceous sandy rock. The greater part of the hills to the north of the bay are composed of the same formation, traversed, in several places by north and east, narrow trap-

dykes, which have altered the adjacent rocks into an imperfec banded jasper. About one mile to the north-east of Fouges, M Hamilton noticed a mass of black hornstone, and to the west a north-west, near the water's edge, trappean and amygdaloidal roc overlaid by the pumiceous sandstone.

2. *Ritri* is situated on the shores of the bay of Erythræ, oppos the island of Scio; and the geological structure of the neighbouri district consists of red crystalline, apparently stratified trachyte, a of blue or gray, more or less, crystalline limestone, with associa sandstone. The two latter rocks are of anterior date to the trachy but Mr. Hamilton could not determine their relative geological a as they appear to be destitute of organic remains. The beds of li stone are sometimes vertical. On the shore near the Acropolis author noticed also vertical strata of indurated shale and jasper, a near the juncture of the trachyte and limestone, to the north of Acropolis, that the calcareous beds were much shattered. The t long islands which form the anchorage, are also composed of b semi-crystalline limestone, without traces of bedding.

3. *Sighajik.*—A rich alluvial plain, connecting the harbours Sighajik and Teos, gradually rises to the eastward, towards mountainous district, which extends to Smyrna. To the west plain is separated from the sea by a range of hills composed thickly-bedded, white, cretaceous limestone, resembling closely limestone near Smyrna, described by Mr. Strickland*. In so places it is underlaid by beds of sandstone, and sand containing c careous concretions. Just above the ruins of Teos, the limestone very thinly bedded, with slightly micaceous marly way-boards, inclination of the strata being 15° to the west; and near the anci harbour the white limestone is underlaid by a hard, brown, micace sandstone, associated with beds of hard nodular limestone, eviden belonging to a much older formation. Low undulating hills of t sandstone bound the plain to the north-west. One of the two in lated remarkable hills, seen from the anchorage, also consists of the other being composed of vertical beds of blue marble, proba belonging to the same formation. To the north-west of the pla this marble passes into a beautiful breccia, associated with strata brown sandstone. Mr. Hamilton saw no igneous rocks ***in situ***, numerous blocks of greenstone are scattered about the country.

4. *Scalanuova.*—This town stands upon an insulated hill of b semi-crystalline limestone, part of the western chain of Mount M sogis. The limestone is similar to that which occurs near Ephe and Mount Prion, where it is associated with beds of yellow mi ceous sandstone.

5. *Boodroom.*—The castle is built upon an insulated rock of si lar limestone, connected with beds of argillaceous shale, of vari colours. The hills to the north of the town, and on which are tra able the walls of the Acropolis of Halicarnassus, consist of the sa formation, interstratified at one point with thin projecting bands

---

* Geol. Proceedings, vol. ii. p. 538; (or L. and E. Phil. Mag., vol. p. 202) Geol. Trans., 2nd Series, vol. v. p. 393.

siliceous limestone. The low hills near the shore, and on which the ruins of Halicarnassus stand, are composed of horizontal beds of volcanic sand and trachytic conglomerate, formed chiefly of angular fragments of brown porphyritic trachyte. Five or six miles to the south-west of Boodroom is the conical hill of Chifoot-Kaleh, 1000 feet high. It consists entirely of reddish trachyte; and all the country between it and Boodroom is composed of trachyte or trachytic conglomerates. The hills to the west of Chifoot-Kaleh are also trachytic, with indications of columnar structure. Trachyte likewise forms part, if not all, the promontory of Karabaghla, and the islets to the westward of it. The north-east dip of the limestone of Boodroom, Mr. Hamilton thinks, may be owing to the protrusion of the igneous rocks of Karabaghla and Chifoot-Kaleh. The shore abounded in one place with pebbles of pumice.

6. *Cnidus* is situated near the extremity of Cape Krio, the western end of the south shore of the Gulf of Cos. The whole peninsula is formed of blue semi-crystalline limestone, shale and sandstone, the strata dipping near the extremity of the promontory 45° to the south-west, but increasing to a higher angle towards the east-north-east.

The following is given by Mr. Hamilton as the general structure of the country:—

Summit of the peninsula towards the west, thin-bedded calcareous shale and blue limestone, thickly bedded and cavernous. Eastward of the ruins, it is in some places interstratified with a hard greenish sandstone, resembling graywacke. The sides of the hills are occasionally obscured, by a loose limestone breccia of more modern origin.

The hills rise rapidly towards the east and north-east, and at the distance of two miles exceed 2000 feet in height. Their summit is a narrow ridge, a quarter of a mile in length from north-west to south-east, and consists of laminated calcareous shales, dipping 45° to the south-west. These shales present a very steep escarpment towards the north-east, but are overlaid towards the south-west by the blue limestone.

7. *Island and shores of the Gulf of Syme.*—The Gulf of Syme is separated from that of Cos by a narrow isthmus. The island is an uniform mass of grayish-white compact scaglia, with occasional bands and nodules of siliceous limestone. In some places the limestone is thickly bedded, but in others thinly, with way-boards of marl; and in one locality it was observed to rest on greenish sandstone. The thinner-bedded variety is sometimes reddish, and resembles the limestone of Mount Atairo, in the island of Rhodes. The strata are occasionally horizontal; but on the brow of the high table-land above the town of Syme and in other districts they are inclined from 30° to 35° to the north and north-north-west; and beyond the harbour of Panermiotis 20° to the south and south-south-east. Mr. Hamilton found no organic remains in the island.

The southern shore of the Gulf consists of the same whitish compact scaglia, with nodules of flint and jasper. Some portions of it are

a breccia, composed of fragments of white limestone in a pale
paste, or of red limestone in a white paste. At the eastern ex
mity of the Gulf, a thinly bedded limestone alternates with band
pale red jasper, the strata dipping 50° north-west; but in s
places they are curiously contorted. The jasper increases in quan
towards the north-west, the limestone becoming less promin
Mr. Hamilton did not land on the north side of the Gulf, but sev
points appeared to him, viewed from the sea, to consist of a br
arenaceous conglomerate.

8. *Rhodes.*—The northern half of the island, the portion vis
by the author, consists chiefly of tertiary marine deposits, of
condary limestone and of scaglia, with sandstones and conglo
rates. No igneous rocks were observed *in situ*, but numer
pebbles of greenstone and other traps were noticed in the con
merates near the centre of the island.

TERTIARY STRATA.—These consist of a shelly testaceous li
stone, sandstone, and conglomerates, and extend in a zone of va
ble breadth, having a quâquâversal dip, along those parts of
island visited by the author. At the north-east end, the tertiary st
rise into high and considerable hills, which stretch across the isl
from east to west.

The following is the order of succession:—

1. Summit of the hills three miles, south-south-west of the t
of Rhodes.

	Feet.
Sandy gravel and conglomerate consisting of pebbles of scaglia	10 to 1
Fine sand, with indications of false stratifications, true dip 5° north-east	10 to 1
Gravel	8
Sand, with perpendicular veins of marl	10 to 1
Sand, with concretions of marl	
Sand, with bands of marl	

2. These beds repose on an extensive formation (considere
be from 200 to 300 feet thick) of yellow, calcareous, shelly con
merate, the beds of which dip 10° to north-east. It contains nu
rous shells of the genera Pecten, Cardium and Venus, and it is
stone principally used in masonry. It extends to the town of Rho
and re-appears to the south of the table-land in nearly horizo
beds, some of which are very arenaceous. It is extensively
veloped in several places along the coast, as far as Lindo, w
it rests unconformably against the secondary limestone.

3. A bed of sandy marl, containing thin bands of calcareous n
Thickness not great.

4. A thick bed of conglomerate and gravel, extending a consi
able distance to the south and south-west, and rising into lofty h
which form steep and broken cliffs on the western coast of
island, several miles from Rhodes. It thins out gradually fur
south, resting at the entrance of a deep valley, upon upraised b
of blue and white scaglia and sand.

Near Archangelo, half-way between the town of Rhodes and
do, a similar system of tertiary rocks is extensively developed.

About one mile north of Lindo Mr. Hamilton noticed between
tertiary and secondary series a thick bed of large limestone peb-
s, with sometimes quartz pebbles and boulders, cemented by a
d calcareous paste. This conglomerate rests immediately on
blue limestone, filling up its interstices ; and it is considered by
. Hamilton to be the lowest tertiary deposit.

SECONDARY ROCKS.—The greater part of Rhodes consists of sca-
, generally considered to be the equivalent of the cretaceous sy-
n of Europe. It is composed, (1.) of red and brown sandstones
h conglomerates ; (2.) of whitish gray and red scaglia limestone ;
d (3.) of blue limestone; but the last deposit Mr. Hamilton
nsiders to belong to a different epoch.

. The sandstones and conglomerates occur near the centre of
island, and apparently form the upper division of the deposit.
ed conglomerate, which is found between Apollona and Embona,
s 50° to the south-south-west, and rests conformably upon whitish-
y scaglia. At the same locality exist indurated red marls and
dstone grits ; and at the north-north-west foot of Mount Atairo
nother bed of conglomerate, containing chiefly boulders of green-
ne, and a greenish granular rock, but inclosing also rounded
sses and pebbles of the gray scaglia of the neighbouring hills.
e greenstone was not seen by Mr. Hamilton *in situ*.

2. The scaglia limestone is chiefly developed in the lofty ridge of
unt Atairo (anc. Mons Atabyrius), which is from 3500 to 4000
t in height. The summit is a narrow ridge about two miles
g, extending from north-east to south-west, or nearly in the di-
tion of the axis of the island. The bed dips from 15° to 20° to
south-east. The upper portion consists of thick-bedded gray
glia, without flints; lower down occurs a thinly-laminated lime-
ne, with tabular masses or beds of flint; and still lower, the beds
again thicker and the flints are nodular. The total vertical di-
nsions of these deposits is from 800 to 900 feet. Beneath them,
scaglia is interstratified with a red marly limestone, and further
vn the hill are thick beds of scaglia without flints. Below the
age of Embona, situated to the north-west of the mountain, a
enish compact sandstone crops out from beneath the limestone
Mount Atairo, and dips to the south-east. The range of hills
the north-north-east consists also chiefly of the gray limestone,
ch rests on the red and brown sandstones. Mr. Hamilton did
ascertain how far the formation ranges to the north-west.

The Acropolis of Camiro, on the east coast of the island, and six
es north of Lindo, stands upon an insulated table-rock of whitish
npact scaglia, encircled at its base with tertiary strata.

3. The blue limestone is classed provisionally by Mr. Hamilton
h the secondary rocks ; but he is of opinion it may be of the same
as the limestone of Halicarnassus, and belong to a much older
tem. It occurs extensively along the east coast, particularly near
do, where it forms high and steep hills, against which remnants

of horizontal strata of tertiary limestone rest at a considerable hei
The Acropolis of Lindo is situated upon beds of it, having an i
nation of 20° to 25° to the north-west. It occurs likewise fur
north, between Rhodes and Archangelo, where it forms the
ridge of hills about two miles from the shore, and the low ridg
rocky islets in the middle of the plain, and parallel to the coast.

Older Rocks.—The only locality at which these are satis
torily shown, is half-way between Archangelo and Lindo, and c
to the shore at the bottom of a deep bay. At this point the
limestone, which in its lowest beds is hard and siliceous, and
between 60° and 70° to the north-west, is underlaid constantl
a hard, black, schistose, crystalline rock, like the limestone of
Bosphorus.

In conclusion, Mr. Hamilton gives the following general st
ments: 1. The scaglia is more abundant in Rhodes and the s
of Asia Minor than further north, and is apparently a prolonga
of the scaglia which constitutes the mass of Mount Taurus. N
mulites have been found in it near Adalia, and Mr. Hamilton
tained near Deenair a species resembling one found in the scagli
the Ionian Islands. 2. Igneous rocks are much more rare tow
the south, and do not appear so often associated with the scagli
with the older limestones. 3. Trachyte and other igneous prod
almost constantly accompany the blue semi-crystalline limeston
at Erythræa and Boodroom. 4. In the absence of organic rema
Mr. Hamilton hesitates to state positively whether the blue li
stone is an altered rock, or is an older formation which has
raised to the surface; but he is inclined to adopt the latter opi
in consequence of the resemblance of the limestone to that
Constantinople, which is associated with schists, containing tr
ition fossils.

A letter from Mr. Ottley, of Exeter, was then read, "On s
specimens from the new red sandstone," considered by the writ
be casts of Alcyonia.

The specimens alluded to in this letter were found by Mr.
ker in a quarry about two miles from Exeter, in the road tow
Bath. In the lower part of the quarry coarse sandstones and
conglomerates occur, and in the upper a flat, flaggy sandstone.
beds dip 10° or 12° to the south-east. Interstratified with the
glomerate is a looser red sandstone, in which the branched co
tions, considered to be of alcyonic origin by Mr. Ottley, princi
occur; but they have been found also in the conglomerates, and
sandstone of the upper part of the quarry.

A paper was afterwards read, entitled, "Description of the
mains of a Bird, Tortoise, and Lacertian Saurian, from the Cha
by Richard Owen, Esq., F.G.S.

Bird.—The three portions of Ornitholite were obtained by
Enniskillen from the chalk near Maidstone, and were recognise
him and Dr. Buckland as belonging to some large bird. On
the bones is nine inches in length, and has one extremity nearly
tire, though mutilated, but the other is completely broken off.

extremity, partially preserved, is expanded. The rest of the shaft of the bone has a pretty uniform size, but is irregularly three-sided, with the sides flat and the angles rounded: its circumference is two inches and a quarter. The whole bone is slightly bent. The specimen differs from the femur of any known bird, in the proportion of its length to its breadth; and from the tibia or metatarsal bone, in its triedral figure, and the flatness of the sides, none of which are longitudinally grooved. It resembles most the humerus of the Albatross in its form, proportions and size, but it differs in the more marked angles bounding the three sides. The expanded extremity likewise resembles the distal end of the humerus of the Albatross, but it is too mutilated to allow the exact amount of similarity to be determined.

On the supposition that this fragment is really a part of the humerus, Mr. Owen says, its length and comparative straightness would prove it to have belonged to a longipennate natatorial bird, equalling in size the Albatross.

The two other portions of bone have been crushed; but Mr. Owen states that they belong to the distal end of the tibia, the peculiar strongly-marked trochlear extremity of which is well preserved. Their relative size to the preceding bone, supposing that specimen to be part of a humerus, is nearly the same as in the skeleton of the Albatross. There is no bird now known north of the Equator with which the fossils can be compared.

Tortoise.—The remains of the Chelonian Reptile consist of four marginal plates of the carapace, and some small fragments of the expanded ribs. The marginal plates are united by the usual finely-indented sutures, and each is impressed along the middle of its upper surface with a line corresponding to the margin of the horny plate which originally defended it. The external edge of each plate is slightly emarginated in the middle. These plates are narrower in proportion to their length than in any of the existing marine Chelonia; and they deviate still more in the character of their internal articular margin, from the corresponding plates of terrestrial Chelonia; but they sufficiently agree with the marginal plates of the carapace of the Emydes, to render it most probable that these cretaceous remains are referable to that family of Chelonia which live in fresh water or estuaries.

Lacertian Saurian.—This fossil belongs to the collection of Sir Philip Egerton; and it consists of a chain of small vertebræ in their natural relative position, with fragments of ribs and portions of an ischium and a pubis.

The bodies of the vertebræ are united by ball and socket-joints, the socket being on the anterior and the ball on the posterior part of the vertebra; and they are further proved to belong to the Saurian class of reptiles by the presence of many long and slender ribs, as well as by the conversion of two vertebræ into a sacrum, in consequence of the length and strength of their transverse processes. The remains of the ischium and the pubis are connected with the left side of the sacrum, proving incontestably that this reptile had

hinder extremities as well developed as in the generality
rians. Of these extremities, as well as of the anterior and
head, there are no traces.

Mr. Owen then proceeds to determine to which division
rians, having ball and socket vertebral joints, the fossil sh
referred. In the Crocodilian or Loricate group, the transvers
gerous processes are elongated, and three, four, or five of th
bræ which precede the sacrum are ribless, and consequentl
oned as lumbar vertebræ: in the Lacertian Sauriæ there are
more than two lumbar vertebræ, and those which have ribs
them on short convex processes or tubercles.

In the fossil from the chalk, the ribs are articulated wit
processes of the kind just mentioned, resembling tubercles, ar
are attached to the sides of the anterior part of all the ver
except the one immediately preceding the sacrum. These
ters, Mr. Owen says, in conjunction with the slenderness an
form length of the ribs, and the degree of convexity in the ar
ball of the vertebræ, prove incontestably, that the fossil is pa
Saurian, appertaining to the inferior or Lacertian group.

The under surface of the vertebræ is smooth, concave in t
of the spine, and convex transversely. As there are twen
costal vertebræ anterior to the sacrum, including the single l
the fossil, Mr. Owen observes, cannot be referred to the
Stellio, *Leiolepis*, *Basiliscus*, *Agama*, *Lyriocephalus*, *Anolis*, o
mæleon, but that a comparison may be instituted between it a
Monitors, *Iguanas*, and *Scinks*. In conclusion, he states, that
absence of the cranium, teeth, and extremities, any further ap
mation of the fossil would be hazardous, and too conjectural t
any good scientific result.

May 13, 1840.—A memoir was commenced "On the Classifi
and Distribution of the Older or Palæozoic Rocks of the North
many and of Belgium, as compared with formations of the sa
in the British Isles;" by the Rev. Prof. Sedgwick, F.G.S., an
derick Impey Murchison, Esq., F.G.S.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

At a meeting of this Society, held on Monday evening,
8th, Dr. Hodgson, the President, in the Chair.

A communication was made by Mr. Tozer, of Caius Colle
some mathematical formulæ for determining the permanent
of Emigration and Immigration on numbers. The solution in
a consideration of all those causes by which the duration of
the rate of its production, may be affected, which may be calle
operation by the transfer.

Where all the elements are constant the numbers of a
after a given time, may be determined by the solution of an eq
of finite differences in which the coefficients are constant; th
bable change in the value of those coefficients may, when the
are sufficient, be calculated by a method suggested by Laplace
Where the data are insufficient for the complete solution

em, Mr. Tozer considers the attempt at accurate investiga-
to afford the best means of analysing the processes which the
employs in arriving at its conclusions, when the correctness or
rectness of those conclusions is incapable of demonstration; and
iowing where the data are insufficient, of suggesting the nature
e observations by which they may be supplied, and thus of
ually approximating the science to which the problem belongs
e demonstrative sciences.

Scientific Works lately published.

ylor's Scientific Memoirs—Part VIII. completing the Second
me.

talogues of the Miscellaneous Manuscripts and of the Manu-
t Letters in the possession of the Royal Society.

ie Philosophical Transactions. Part II. 1840.

Description of British Guiana, Geographical and Statistical:
iting its Resources and Capabilities, together with the present
'uture Condition and Prospects of the Colony. By Robert H.
mburgk, Esq.

ı the Heat of Vapours and on Astronomical Refractions. By
. W. Lubbock, Bart.

METEOROLOGICAL OBSERVATIONS FOR FEB. 1841.

wick.—Feb. 1. Snowing. 2. Snow-showers. 3. Frosty: dry and cold:
evere frost at night. 4. Frosty: overcast. 5. Dry cold haze: windy at
6. Boisterous. 7. Boisterous: hazy and cold. 8—11. Hazy and cold.
ense fog: very fine: rain. 13. Overcast: rain. 14. Rain: cloudy. 15.
y: slight rain. 16, 17, Hazy. 18. Fine. 19. Rain: cloudy and fine.
oudy and fine: rain. 21. Overcast and fine. 22. Dense fog. 23. Hazy:
24. Hazy and cold. 25. Cloudy and cold: rain. 26. Rain. 27. Cloudy:
28. Very clear: cloudy and fine.

ton.—Feb. 1. Cloudy: snow A.M. and P.M. 2. Fine: snow early A.M.:
P.M. 3. Cloudy: snow early A.M. and P.M. 4, 5. Cloudy. 6, 7. Stormy.
udy: snow P.M. 9, 10. Cloudy. 11—13. Cloudy: rain P.M. 14. Cloudy.
loudy: rain P.M. 16. Cloudy. 17. Rain. 18, 19. Cloudy. 20, 21.
22, 23. Foggy. 24. Rain. 25. Cloudy: rain P.M. 26. Rain: rain P.M.
ain. 28. Fine.

legarth Manse, Dumfries-shire.—Feb. 1, 2. Sprinkling of snow: frost P.M.
ow-showers: frost. 4. Frost: fair but cloudy. 5. Frost: sprinkling of
6. Frost: occasional snow-showers. 7. Frost: severe and cold. 8, 9.
cold and withering. 10. Frost, but giving way. 11. Thaw and heavy
sleet. 12. Fog: rain: fine thaw. 13. Rain all day. 14. Rain in the
g: mild. 15. Rain all day. 16, 17. Fair but cloudy. 18. Wet all day.
lear and cold. 20. Fine. 21, 22. Fine, but cloudy. 23. Rain A.M:
P.M. 24. Clear and cold. 25. Cloudy and threatening rain. 26. Cloudy
igh wind. 27. Frost in the morning. 28. Frost in the morning with
on the hills.

shone out 19 days. Rain fell 8 days. Frost 11 days. Snow 6 days.
nd north 1 day. North-east 8 days. East-north-east 2 days. East 2 days.
outh-east 1 day. South-east 4 days. South 4 days. South-west 2 days.
1 day. North-west 1 day. North-north-west 2 days.
m 6 days. Moderate 11 days. Brisk 4 days. Strong breeze 4 days.
rous 3 days.

Mean temperature of the month	36°·50
Mean temperature of February 1840	36 ·78
Mean temperature of spring-water	42 ·60
Mean temperature of spring-water, Feb. 1840	**44 ·16**

Days of Month. 1841. Feb.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dum-fries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	30·358	30·346	30·268	30·12	30·42	30·47	30·7	42·0	29·7	31	23	32	35	32	N.	NE.	calm	NE.	·116	...	...	...	30
2.	30·072	30·093	29·931	29·90	30·25	30·10	27·7	32·4	25·8	31	18	28·5	33	26½	N.	NE.	calm	NE.	...	...	...	...	24
3.	30·000	30·020	29·977	29·80	30·07	30·01	22·2	29·7	21·6	28	14	25·5	33	27½	N.	E.	calm	ENE.	...	...	...	...	20
4.	29·760	29·803	29·670	29·62	29·87	29·77	25·7	25·7	22·2	30	22	27	34	27	E.	NE.	E.	NE.	...	...	...	...	21
5.	29·682	29·722	29·688	29·55	29·78	29·85	25·7	30·0	24·6	30	26	29·5	34	30½	NE.	NE.	E.	NE.	...	...	...	...	22
○ 6.	29·648	29·661	29·609	29·56	29·80	29·78	27·3	29·8	25·2	28	24	28	30½	26½	NE.	E.	E.	E.	...	...	...	...	24
7.	29·480	29·500	29·443	29·46	29·65	29·58	23·8	28·8	23·8	27	24	23·5	28½	27½	NE.	E.	E.	ENE.	...	...	...	0·05	20
8.	29·372	29·442	29·395	29·24	29·50	29·57	26·7	26·7	24·0	29	26	26	31½	27	N.	NE.	E.	NE.	...	...	...	...	23
9.	29·636	29·952	29·645	29·47	29·70	29·87	27·4	29·3	27·2	30	27	28·5	32½	28½	N.	NE.	E.	NE.	...	...	...	...	24
10.	30·014	30·017	29·959	29·76	29·80	29·68	28·3	30·2	27·0	32	27	27·5	33	27	SE.	SE.	calm	NE.	...	·04	...	...	25
11.	29·850	29·866	29·657	29·58	29·53	29·38	36·7	37·2	27·0	41	37	33	39	29	SE.	S.	calm	E.	...	·04	...	...	28
12.	29·672	29·745	29·640	29·33	29·40	29·48	41·7	42·5	35·6	51	38	38	40	36	S.	SW.	calm	S.	...	·04	·58	...	36
☾ 13.	29·672	29·659	29·313	29·30	29·28	29·02	44·8	49·0	41·4	51	44	41	45	37½	SSE.	S.	calm	SE.	...	·01	·10	...	39
14.	29·180	29·288	29·157	28·82	28·94	28·87	47·3	48·3	45·0	52	41	43	45½	41	S.	SW.	calm	SE.	·022	...	·09	2·06	42
15.	29·274	29·263	29·071	28·89	28·95	28·85	43·4	51·4	42·4	49	42	43	45	38	SSE.	S.	calm	ESE.	...	·15	...	...	42
16.	29·076	29·141	29·086	28·73	29·00	29·19	45·7	49·2	42·5	51	39	42	46	39½	E.	SE.	calm	SE.	·091	·11	·15	...	43
17.	29·268	29·584	29·584	28·97	29·30	29·34	40·3	50·0	40·8	46	36	36·5	46½	41	NW.	W.	calm	SE.	·125	·01	·06	...	41
18.	29·642	29·614	29·490	29·26	29·24	29·26	43·3	45·4	40·7	53	38	40	45	38	SE.	S.	calm	S.	...	·04	·02	...	41
19.	29·672	29·744	29·601	29·22	29·44	29·43	46·8	51·4	43·0	50	35	44	45	41	S.	SW.	calm	SW.	·088	·02	...	...	46
20.	29·846	29·977	29·808	29·33	29·51	29·85	46·7	50·0	40·4	56	40	42	48	40	S.	S.	SE.	S.	·033	·11	...	...	45
● 21.	30·198	30·296	30·162	29·77	30·05	30·15	45·3	52·2	44·7	56	29	36	46	38½	NW.	NE.	calm	S.	·063	...	·15	1·56	44
22.	30·390	30·345	30·323	29·93	30·18	30·19	40·3	50·2	40·2	42	38	32	46	38	NW.	NE.	calm	SW.	...	...	...	...	42
23.	30·356	30·323	30·298	29·98	30·24	30·28	38·6	43·7	38·6	44	32	38	48½	41	NE.	NE.	calm	NE.	...	...	...	...	41
24.	30·328	30·304	30·298	29·95	30·39	30·45	34·7	41·3	33·8	40	36	36	46	37	N.	NE.	E.	N.	...	·01	·07	...	36
25.	30·336	30·308	30·076	29·98	30·33	30·04	38·8	41·5	33·8	42	35	38	45	31½	N.	NE.	calm	W.	...	·07	...	...	37
26.	29·378	29·872	29·638	29·47	29·80	29·67	43·7	44·5	38·7	48	35	41·5	48	41	WNW.	NW.	calm	NW.	·108	·04	·07	...	42
27.	29·756	29·730	29·368	29·36	29·80	29·87	39·7	47·7	37·0	45	32	38	43½	34	NW.	N.	N.	NNW.	·100	·07	·24	...	37
☽ 28.	29·880	29·874	29·681	29·52	29·76	29·47	36·9	43·3	35·0	47	32	35	43½	28½	WNW.	NW.	calm	NNW.	·044	...	·05	0·11	34
Mean.	29·778	29·833	29·708	29·49	29·713	29·695	36·4	40·8	34·0	41·42	31·78	34·7	40·5	33·2					Sum. ·790	·76	1·58	3·78	Mean. 34

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

MAY 1841.

LIV. *Remarks on* Professor Challis's *Reply to* Mr. Airy's *Objections to the Investigation of the Resistance of the Atmosphere to an Oscillating Sphere. By* GEORGE BIDDELL AIRY, *Esq., M.A., F.R.S., Astronomer Royal.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I HAVE not had leisure earlier to notice the remarks of Professor Challis in your February Number, upon my objection to his investigation of the resistance to a spherical body oscillating in an elastic medium. I beg to express my sense of the courtesy with which Professor Challis has replied to my objection, and to avow my opinion that a discussion conducted in this manner cannot but be advantageous to the interests of science. With this feeling, I think it right again to state that the considerations urged by Professor Challis in his last communication, do not at all remove my objection to the fundamental parts of his investigation. And I trust that, by putting my own reasoning in a different form, I shall be able to show, to the satisfaction of Professor Challis, that the solution which he has adopted for expressing the movement of the particles of air surrounding the ball, is untenable.

The reasoning of a mixed nature upon which Professor Challis has founded and supported his investigation, and upon which also I have objected to it, is (if I may use such a term) extremely hazardous. I mean by this expression to assert, that, unless managed with the greatest caution, it is apt to introduce or to conceal important errors of principle. I shall, therefore, abandon this kind of reasoning, and shall confine myself to another mode of investigation, of which results will be beyond doubt, namely, that by rectangular ordinates.

The point in dispute may be well stated in ProfessorCha words: "I have certainly considered it possible that the city of the fluid at a given distance from the centre to or which it is directed, may, at a given instant, be differe different directions from the centre, provided there b abrupt variation."

Now I undertake to show that,

1st. If the motion of the particles be directed to or fr centre (or approximately so, the whole investigation approximate), the motions must be the same in all direct in phase as well as in coefficient.

2nd. If, in order to support Professor Challis's expre for the movement of the particles to or from a centre suppose other movements perpendicular to these rad coexist, then it will be shown that the possibility of su combination of movements is not proved, and that (as i pends upon finding two quantities which shall satisfy equations) the presumption is, that it is impossible.

I proceed now with the investigation.

If $u, v, w,$ be the velocities in the directions $x, y, z,$ particle which at the time t has the coordinates x, y, z (velocities being expressed in terms of $x, y, z,$ and t), a ϱ be the density at that point and p the corresponding p ure, and if $\varrho = kp$; the equations to the motion of particle (no accelerating force being supposed to act) are following:

$$\frac{1}{\rho}\cdot\frac{dp}{dx} = -\frac{du}{dt} - \frac{du}{dx}u - \frac{du}{dy}v - \frac{du}{dz}w$$

$$\frac{1}{\rho}\cdot\frac{dp}{dy} = -\frac{dv}{dt} - \frac{dv}{dx}u - \frac{dv}{dy}v - \frac{dv}{dz}w$$

$$\frac{1}{\rho}\cdot\frac{dp}{dz} = -\frac{dw}{dt} - \frac{dw}{dx}u - \frac{dw}{dy}v - \frac{dw}{dz}w$$

$$\frac{d\rho}{dt} = -\frac{d.\rho u}{dx} - \frac{d.\rho v}{dy} - \frac{d.\rho w}{dz}.$$

These equations are rigorous. If, however, we co ourselves to quantities of the first order of the displace and velocities: since $u\frac{du}{dx}, v\frac{du}{dy}$, &c. are evidently of second order: and since $u\frac{d\rho}{dx}$, or $ku\frac{dp}{dx}$, or $k\rho u$

$+ u\,\frac{du}{dx} + \&c.\Big)$ is a quantity of the second order: the equations are reduced to these:

$$\frac{d\,.\log\rho}{dx} = -k\,\frac{du}{dt}$$

$$\frac{d\,.\log\rho}{dy} = -k\,\frac{dv}{dt}$$

$$\frac{d\,.\log\rho}{dz} = -k\,\frac{dw}{dt}$$

$$\frac{d\,.\log\rho}{dt} = -\frac{du}{dx} - \frac{dv}{dy} - \frac{dw}{dz}.$$

And the method of testing the possibility of any assumed system of movement, or of discovering the conditions which must be satisfied by the system of movements, will be, to form the expressions on the right-hand side of these equations, and to try whether they satisfy the equations which must hold among the four differential coefficients of the same quantity.

I. Now suppose the whole velocity of the particle x, y, z, to be directed from the centre, and at the time t to have the value υ (motion towards the centre being implied by a negative value of υ); and suppose that centre to be the origin of coordinates. Then

$$u = \upsilon\frac{x}{r},\ v = \upsilon\frac{y}{r},\ w = \upsilon\,\frac{z}{r}:\ \text{where } r = \sqrt{x^2+y^2+z^2}.$$

$\frac{du}{dt} = \frac{x}{r}.\frac{d\upsilon}{dt},\ \frac{dv}{dt} = \frac{y}{r}.\ \frac{d\upsilon}{dt},\ \frac{dw}{dt} = \frac{z}{r}.\frac{d\upsilon}{dt}.$ Also

$$\frac{du}{dx} = \frac{d\upsilon}{dx}.\frac{x}{r} + \upsilon\left(\frac{1}{r} - \frac{x}{r^2}.\frac{dr}{dx}\right) = \frac{d\upsilon}{dx}.\frac{x}{r} + \upsilon\left(\frac{1}{r} - \frac{x^2}{r^3}\right):$$

$$\frac{dv}{dy} = \frac{d\upsilon}{dy}.\frac{y}{r} + \upsilon\left(\frac{1}{r} - \frac{y^2}{r^3}\right):\ \frac{dw}{dz} = \frac{d\upsilon}{dz}.\frac{z}{r} + \upsilon\left(\frac{1}{r} - \frac{z^2}{r^3}\right):$$

$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = \frac{1}{r}\left(x\frac{d\upsilon}{dx} + y\frac{d\upsilon}{dy} + z\,\frac{d\upsilon}{dz}\right) + \frac{2\,\upsilon}{r}.$ Substituting these values in the equations above, they become

$$\frac{d\,.\log\rho}{dx} = -\frac{kx}{r}.\frac{d\upsilon}{dt} \qquad = \mathrm{X}$$

$$\frac{d\,.\log\rho}{dy} = -\frac{ky}{r}.\frac{d\upsilon}{dt} \qquad = \mathrm{Y}$$

$$\frac{d\,.\log\rho}{dz} = -\frac{kz}{r}\,.\,\frac{dv}{dt} \qquad = \mathrm{Z}$$

$$\frac{d\,.\log\rho}{dt} = -\frac{1}{r}\left(x\frac{dv}{dx} + y\frac{dv}{dy} + z\frac{dv}{dz}\right) - \frac{2v}{r} = \mathrm{T}.$$

In order that the supposed motion may be possible, these expressions must satisfy the following equations:

$$(1.)\ \frac{d\mathrm{X}}{dy} = \frac{d\mathrm{Y}}{dx}: \quad (2.)\ \frac{d\mathrm{Y}}{dz} = \frac{d\mathrm{Z}}{dy}: \quad (3.)\ \frac{d\mathrm{Z}}{dt} = \frac{d\mathrm{T}}{dz}.$$

Equation (1.), on performing the differentiation, becomes

$$\frac{kxy}{r^3}\,.\,\frac{dv}{dt} - \frac{kx}{r}\,.\,\frac{d^2v}{dy\,dt} = \frac{kxy}{r^3}\,.\,\frac{dv}{dt} - \frac{ky}{r}\,.\,\frac{d^2v}{dx\,dt},$$

or $x\dfrac{d^2v}{dy\,dt} = y\dfrac{d^2v}{dx\,dt}$.

Let $\dfrac{dv}{dt} = \varpi$; then this equation becomes $\dfrac{1}{y}\,.\,\dfrac{d\varpi}{dy} = \dfrac{1}{x}\,.\,\dfrac{d\varpi}{dx}$. If we solve this equation, in the usual way of solving partial differential equations of the first order, we find that ϖ must be a function of x^2+y^2.

Treating equation (2.) in the same manner, we find that ϖ must be a function of y^2+z^2.

It is evident that these results can be united only by the supposition that ϖ is a function of $x^2+y^2+z^2$, or of r^2, or of r.

That is, $\dfrac{dv}{dt}$ is a function of r and t only; and the quantities x, y, z, do not enter into it except as combined in the formula $x^2+y^2+z^2$.

Integrating this function, $v = \mathrm{R}+\mathrm{S}$, where R is a function of r and t, and S is a function of x, y, z.

To form equation (3.), we remark that $\mathrm{Z} = -\dfrac{kz}{r}\,.\,\dfrac{d\mathrm{R}}{dt}$, and therefore $\dfrac{d\mathrm{Z}}{dt} = -\dfrac{kz}{r}\,\dfrac{d^2\mathrm{R}}{dt^2}$. Also that part of T which depends on R is $-\dfrac{1}{r}\left(x\dfrac{d\mathrm{R}}{dr}\,.\,\dfrac{x}{r} + y\dfrac{d\mathrm{R}}{dr}\,.\,\dfrac{y}{r} + z\dfrac{d\mathrm{R}}{dr}\,.\,\dfrac{z}{r}\right) - \dfrac{2\mathrm{R}}{r} = -\dfrac{d\mathrm{R}}{dr} - \dfrac{2\mathrm{R}}{r}$. Put S′ for $-\dfrac{1}{r}\left(x\dfrac{d\mathrm{S}}{dx} + y\dfrac{d\mathrm{S}}{dy} + z\dfrac{d\mathrm{S}}{dz}\right) - \dfrac{2\,\mathrm{S}}{r}$. Then $\mathrm{T} = -\dfrac{d\mathrm{R}}{dr} - \dfrac{2\mathrm{R}}{r} + \mathrm{S}'$: and $\dfrac{d\mathrm{T}}{dz} = -\dfrac{z}{r}\,.\,\dfrac{d^2\mathrm{R}}{dr^2} - \dfrac{2z}{r^2}\,.\,\dfrac{d\mathrm{R}}{dr} + \dfrac{2z}{r^3}\mathrm{R} + \dfrac{d\mathrm{S}'}{dz}$.

quation (3.) therefore becomes

$$\frac{kz}{r}\cdot\frac{d^2R}{dt^2}-\frac{z}{r}\cdot\frac{d^2R}{dr^2}-\frac{2z}{r^2}\cdot\frac{dR}{dr}+\frac{2z}{r^3}R+\frac{dS'}{dz}.$$

he two parts of this expression which depend upon R
upon S' are essentially distinct, and the equation there-
expresses these two things:

$$=\frac{dS'}{dz}\quad\left(\text{and similarly } 0=\frac{dS'}{dx},\quad 0=\frac{dS'}{dy}\right)$$

$$=k\frac{d^2R}{dt^2}-\frac{d^2R}{dr^2}-\frac{2}{r}\cdot\frac{dR}{dr}+\frac{2R}{r^2}.$$

rom the first of these equations $S'=C$,

$$\frac{dS}{dx}+y\frac{dS}{dy}+z\frac{dS}{dz}+2S=-Cr,$$

ntegral of which is $S=\frac{1}{r^3}\cdot\phi\left(\frac{x}{r},\frac{y}{r},\frac{z}{r}\right)-\frac{Cr}{3}$.

he solution of the second equation is

$$=\frac{f'(r-at)}{r}-\frac{f(r-at)}{r^2}+\frac{F'(r+at)}{r}-\frac{F(r+at)}{r^2},$$

e $ka^2=1$,

nd hence the complete value of v in its most general form

$$=\frac{f'(r-at)}{r}-\frac{f(r-at)}{r^2}+\frac{F'(r+at)}{r}-\frac{F(r+at)}{r^2}$$

$$\phi\left(\frac{x}{r},\frac{y}{r},\frac{z}{r}\right)-\frac{Cr}{3}.$$

ill now explain these several terms.

t. Motion from the centre is possible without any other
on if the velocity is expressed by $-\frac{Cr}{3}$, that is, if every
ical layer to unlimited distance increases or diminishes
antly with the time in the same geometrical proportion.
meaning of this term will be fully understood if we ex-
e the corresponding expression for $\frac{d.\log\rho}{dt}$, which is then
C. As we cannot conceive a case in which the sur-
ling atmosphere can have the density proper for this
we may at once dismiss it as inapplicable to any practi-
roblem.

d. Motion from the centre is possible without any other
on if the velocity is expressed by $\frac{1}{r^3}\phi\left(\frac{x}{r},\frac{y}{r},\frac{z}{r}\right)$,

that is, if in every different direction it is the same, or dif according to any arbitrary law; but if in different poi that direction, it is inversely as the square of the distance the centre. This shows that the same quantity of air in every second of time in the same direction through section of each small solid angle. As we cannot in any tical case provide for the incessant supply of air whicl will require, we may lay aside this form of solution. remark, that this solution is made possible only by the n of terms of the second order of velocities; for it would be found, upon examining the forces corresponding t motion, that there are forces depending on the squares locity which are inconsistent with this motion.

3rd. Motion from the centre is possible without any motion if the velocity is expressed by $\frac{f'(r-at)}{r} - f(r$

$+ \frac{F'(r+at)}{r} - \frac{F(r+at)}{r^2}$, where $ka^2 = 1$. This expr represents two series of waves, one rolling from the and the other towards the centre: but, for each of these the phase and the intensity are the same in every dir from the centre.

The form of solution adopted by Professor Cha $\cos\theta . \left(\frac{f'(r-at)}{r} - \frac{f(r-at)}{r^2}\right)$. If z be the axis which θ is measured, $\cos\theta = \frac{z}{r}$, and this expressio comes $\frac{z . f'(r-at)}{r^2} - \frac{z . f(r-at)}{r^3}$. This expression included in any of the formulæ already discussed; evidently will not satisfy the conditions found above; cannot therefore apply to the problem before us.

II. But suppose (as an assumption of which the poss is to be tested), that the motion from the centre is repre by the formula $\frac{z\,R}{r}$, as assumed by Professor Challis, an this motion is accompanied by other motions normal radius for every point. These motions may be repre in the most general way by one velocity P in the plane ing through z and r, and another velocity Q perpend to that plane. Resolving all the assumed velocities directions of x, y, and z, we have

$$u = \mathrm{R}\,\frac{xz}{r^2} - \mathrm{P}.\frac{z}{r}.\frac{x}{\sqrt{x^2+y^2}} - \mathrm{Q}\,\frac{y}{\sqrt{x^2+y^2}}$$

$$v = \mathrm{R}\,\frac{yz}{r^2} - \mathrm{P}.\frac{z}{r}.\frac{y}{\sqrt{x^2+y^2}} + \mathrm{Q}\,\frac{x}{\sqrt{x^2+y^2}}$$

$$w = \mathrm{R}\,\frac{z^2}{r^2} + \mathrm{P}\,\frac{\sqrt{x^2+y^2}}{r}.$$

For simplicity, put $\frac{\mathrm{P}z}{r\sqrt{x^2+y^2}} = p$, $\frac{\mathrm{Q}}{\sqrt{x^2+y^2}} = q$: then

$$u = \mathrm{R}\frac{xz}{r^2} - px - qy$$

$$v = \mathrm{R}\,\frac{yz}{r^2} - py + qx$$

$$w = \mathrm{R}\,\frac{z^2}{r^2} + p.\frac{x^2+y^2}{z}.$$

From these,

$$\frac{d.\log\rho}{dx} = -k\frac{du}{dt} = k\left(-\frac{xz}{r^2}.\frac{d\mathrm{R}}{dt} + x\frac{dp}{dt} + y\frac{dq}{dt}\right) = \mathrm{X}$$

$$\frac{d.\log\rho}{dy} = -k\frac{dv}{dt} = k\left(-\frac{yz}{r^2}.\frac{d\mathrm{R}}{dt} + y\frac{dp}{dt} - x\frac{dq}{dt}\right) = \mathrm{Y}$$

$$\frac{d.\log\rho}{dz} = -k\frac{dw}{dt} = k\left(-\frac{z^2}{r^2}.\frac{d\mathrm{R}}{dt} - \frac{x^2+y^2}{z}.\frac{dp}{dt}\right) \quad = \mathrm{Z}$$

$$\frac{d.\log\rho}{dt} = -\frac{du}{dx} - \frac{dv}{dy} - \frac{dw}{dz} = -\frac{d\mathrm{R}}{dr}.\frac{z}{r} - 2\,\mathrm{R}\,\frac{z}{r^3}$$

$$+ p.\frac{x^2+y^2+2z^2}{z^2} + x\frac{dp}{dx} + y\frac{dp}{dy} + y\frac{dq}{dx} - x\frac{dq}{dy} - \frac{x^2+y^2}{z}.\frac{dp}{dz} = \mathrm{T}.$$

Now $\frac{d\mathrm{X}}{dy}$ must $= \frac{d\mathrm{Y}}{dx}$, or

$$0 = x\,\frac{d^2p}{dy\,dt} - y\,\frac{d^2p}{dx\,dt} + 2\frac{dq}{dt} + y\,\frac{d^2q}{dy\,dt} + x\frac{d^2q}{dx\,dt}.$$

And $\frac{d\mathrm{Y}}{dz}$ must $= \frac{d\mathrm{Z}}{dy}$, or

$$0 = -\frac{y}{r^3}.\frac{d\mathrm{R}}{dt} + \frac{2y}{z}.\frac{dp}{dt} + y\,\frac{d^2p}{dz\,dt} - x\,\frac{d^2q}{dz\,dt}$$

$$+ \frac{x^2+y^2}{z}.\frac{d^2p}{dy\,dt}.$$

And $\frac{d\mathrm{Z}}{dt}$ must $= \frac{d\mathrm{T}}{dz}$, or

$$0 = \frac{k z^2}{r^3} \cdot \frac{d^2 R}{d t^2} - \frac{z^2}{r^2} \cdot \frac{d^2 R}{d r^2} - \frac{2 z}{r^3} \cdot \frac{d R}{d r} + \frac{2 z^2}{r^4} R$$

$$- \frac{x^2 + y^2}{r^3} \cdot \frac{d R}{d r} - 2 R \frac{x^2 + y^2}{r^4} + k . \frac{x^2 + y^2}{z} \cdot \frac{d^2 p}{d t^2}$$

$$- 2 p \frac{x^2 + y^2}{z^3} + 2 \frac{d p}{d z} \cdot \frac{r^2}{z^2} + x \frac{d^2 p}{d x d z} + y \frac{d^2 p}{d y d z}$$

$$+ y \frac{d^2 q}{d x d z} - x \frac{d^2 q}{d y d z} - \frac{x^2 + y^2}{z} \cdot \frac{d^2 p}{d z^2}.$$

The form assumed for R makes the four first terms of
last equation destroy each other. Removing these, and s
stituting the value of R in the other terms in which that le
occurs, we have *three* differential equations to determine
unknown functions. Unless it can be shown that one of th
equations can be deduced from the other two, the discov
of these two unknown functions must be, in the nature
things, impossible. Without professing to have examined
equations minutely, I can only say at present that the th
equations appear to me to be inconsistent, and therefore I c
sider that the motion assumed by Professor Challis is imp
sible, even when accompanied with any other transversal
tions whatever. The *onus* of proving that the three equati
are consistent, rests with the supporter of such a solutio
that to which I have alluded.

I am, Gentlemen,
your very obedient servant,
Royal Observatory, Greenwich, G. B. Ai
March 22, 1841.

LV. *On the Electrical Phænomena attending the Efflu Condensed Air, and of Steam generated under Pressure.*

Wm. Geo. Armstrong, *Esq.**

THE investigation of the causes which induce and mo
the development of electricity during the emissio
steam and compressed air, derives unusual importance f
the prospect it affords of elucidating certain obscure princi
in electrical science, and of leading to an explanation of s
ral meteorological and other natural phænomena, of which
satisfactory theory has been hitherto adduced. The n
novelty of the subject, and its connexion with that myster
and all-pervading agent, electricity, the further knowledg
which is so essential to our progress in natural philoso

---

* Communicated by the Author: see p. 265 of our last Number.

art peculiar interest to the inquiry, and render it worthy ealous pursuit.

n the hope, therefore, of promoting an investigation which ears to me to possess so much interest and importance, I e recently undertaken a series of additional experiments, n on the electricity of expanding air, and on that of efflu- steam, and I trust that the results which I have now to mmunicate will be found deserving of attention, and condu- to further discovery.

'hese experiments will be treated in the following pages er two heads; the first embracing the experiments on ex- ding air, and the second those on effluent steam.

n my last communication to the Philosophical Magazine* ated—

st, That I had succeeded in producing a powerful elec- al development by discharging highly compressed air from insulated receiver of considerable capacity.

'ndly, That on repeating the experiment a great number imes, I found the electricity manifested in the receiver to generally *negative*, but occasionally *positive*.

rdly, That the intensity of the development had proved eedingly unequal, the receiver having sometimes been so hly electrified as to yield a spark a quarter of an inch long, le at other times its electricity had been feeble, and that quently I could detect no electricity at all.

thly, That the electricity of the emitted air had been posi- in every instance that I had tried it.

ithly, That I had more frequently succeeded in producing electrical development when the receiver was cold, and tained a little moisture, than when it was warm and dry.

Having thus briefly recapitulated what I have already tten on the subject, the necessity of referring to the com- nication I have mentioned will be avoided.

I have now clearly ascertained that the temperature of the eiver, and the presence of water within it, powerfully in- ence the phænomena. The colder the receiver the stronger he development of electricity; and if the receiver, when fectly dry, be heated until it becomes unpleasantly hot to touched with the naked hand, the discharge of the air ses to produce any indication of electricity. If the inside the receiver be damp, a higher temperature is required to literate the signs of electricity; and if a few ounces of water poured into the receiver, it may be heated to any degree hout preventing an electrical development, or even render- it very inconsiderable. At low temperatures, however, presence of water in the receiver, or a humid condition of the compressed air, appears to have little tendency to augment the effects. This I infer from the following experiment.

---

* Number for February, p. 133.

After thoroughly drying the interior of the receiver I introduced a quantity of caustic potash for the purpose of absorbing the moisture of the condensed air. I then charged the receiver, and set it in a cold place, where I allowed it to remain for about twelve hours, to afford sufficient time for the potash to produce the desired effect. After this I insulated the receiver and discharged the air, and found the effects to be nearly the same as when no precautions were taken to exclude moisture.

Whether the receiver be wet or dry, I find that a rapid discharge of the air is indispensable to the production of the phænomena. The effects are always strongest when the cock for discharging the air is fully opened; and when by only partially opening the cock the emission of the air is prolonged beyond the period of about a minute, no electricity appears to be developed.

My former experiments, of which the results were so exceedingly capricious, were made in very frosty weather, but those of which I am now speaking were made when the weather was mild and damp, and were much more uniform in their results; but whether the singular fluctuations observed in the first set of experiments were owing to the state of the atmosphere, or to some other cause which has escaped my detection, I feel quite unable to say.

In my latter experiments the electricity of the receiver was uniformly negative, and the intensity of the electrical development did not vary considerably. When the receiver was not insulated, the electricity of the effluent air was always positive; but when the receiver was supported upon an insulated stand, it frequently happened, especially if the receiver were not internally dry, that the gold leaves of the electroscope connected with the pointed conductor, by which the electricity was drawn from the jet of air, separated, first with *positive* electricity, then closed, and opened with *negative* electricity. This effect might, with much probability, be ascribed to the formation of a conducting communication between the insulated receiver and the pointed conductor, by means of watery particles ejected with the air; only, if this were the case, the pointed conductor would not retain its electricity, as it invariably does when the receiver is *not* insulated.

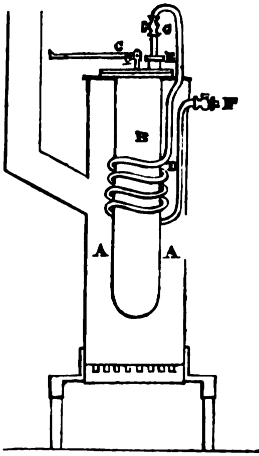
Sometimes the development of electricity does not take place until the air is almost wholly discharged, and then the pith balls suspended from the receiver, and the gold leaves of the electroscope attached to the pointed conductor, abruptly start open, the action being accompanied by a peculiar sound indicating the ejection of drops of water from the tube through which the air is escaping. It would be a curious fact if the sound thus occasioned were proved identical with that which Dr. Schafhaeutl mentions as uniformly accompanying the development of electricity in his experiments on the steam evolved from a Marcet boiler. This sound, however, often occurs *after* the electricity has appeared, but it is uniformly the signal for the electroscope to open when the development has not previously taken place. I cannot say that I have ever noticed a similar noise in any of my experiments on steam.

I am wholly at a loss for an explanation of these singular phænomena. It is well known that a copious precipitation of vapour, and a greatly accelerated evaporation, are the simultaneous effects of the sudden dilatation of condensed air in a receiver—the precipitation being caused by the extreme cold produced by expansion, and the increased evaporation by the diminution, which the precipitation occasions, of the quantity of transparent vapour contained in the air—and to the agency of these two principles I was at first inclined to attribute the effects in question, conjecturing that the variation in the kind of electricity developed in the receiver, might be owing to the predominance, sometimes of evaporation, and sometimes of precipitation. The remarkable influence of heat and moisture, and the effect of a slow evacuation of the receiver, by which precipitation is known to be prevented, seem to point out the connexion of the phænomena with one at least of the principles I have named; but I find so much difficulty in accounting for many of the effects I have recently observed, by reference to both or either of these causes, or to any other cause I can think of, that I am at present unable to form any opinion on the subject.

I now come to the second division of this paper, and shall commence with a description of the evaporating apparatus which I used in the experiments on effluent steam.

This apparatus was constructed under my own directions for the especial purpose of experiment, for which it is much better adapted than a steam-engine boiler; although from the smallness of its size it is not calculated to exhibit effects equally brilliant. It consists, chiefly, of a strong cylindrical boiler, and a stove, in which the boiler is placed vertically, in such a manner as to be exposed on all sides to the heat of the fire. The boiler is thirty inches deep and four inches wide in the inside, and is made of the alloy of copper and tin, usually called gun-metal. The stove is supported upon glass legs to insulate it.

The accompanying figure represents the entire apparat A A is a vertical section of the stove; B is the boiler; C a graduated safety valve; D a copper pipe which proceeds out of the boiler through the stuffing-box E, then enters the stove, in which it is coiled three or four times round the boiler a little above the fuel, and finally terminates outside of the stove in the cock F, at which the steam is discharged, after being thoroughly dried in passing through the pipe thus subjected to the fire. This pipe is easily removed, and instead of it I frequently inserted in the boiler a simple glass tube with a cock upon it. The fuel which I used was coke.



This apparatus, considering its diminutive size, proved e ceedingly effective. When the rate of evaporation was abo a gallon in an hour, and the pressure in the boiler 100 lbs. o the square inch, I could charge a pint coated phial of ve thin glass in a couple of minutes, sufficiently to give a rath smart shock. I found the best method of charging the phi was simply to connect its knob with the boiler or the insu lated stove which contained it, for I could always collect ele tricity in much greater abundance from the evaporating vess than from the issuing steam.

I confidently anticipated, that, in accordance with all forme experiments on the subject, the electricity of the evaporatin vessel would invariably be negative, and that of the effluen steam uniformly positive; but I soon discovered, to my grea surprise, that under certain conditions, these electrical state of the boiler and steam-cloud were *reversed.*

Lest it should be suspected that my assertion of this ex traordinary fact is founded upon erroneous observation, I fee it necessary to state, that I was most unwilling to believe tha ejected steam could, under any circumstances, evolve *negativ* electricity, inasmuch as its doing so was diametrically op posed to the theory which in a former paper I ventured t advance in explanation of the electrical phænomena of effluen steam, and that I was only convinced of the reality of th change, after making repeated trials and applying a variet of tests, which gave the same result in every instance.

I made numerous experiments to ascertain what the con ditions were under which the transmutation took place, an am led to believe, from the following circumstances, that a overheated state of that part of the boiler which contains the steam, is one, at least, of the causes which induce the effect. It will be readily perceived, that by closing the door of the stove a considerable increase of temperature would be occasioned in the upper part of the boiler where the steam was lodged, and I found that in almost every instance in which this was done, the *negative* electricity of the boiler, and the *positive* electricity of the steam, began to decline, and continued so to do until they vanished entirely, and then *positive* electricity gradually appeared in the boiler, and *negative* electricity in the emitted steam. A diminished state of the water in the boiler would also, of course, favour the accumulation of heat in the higher region of the boiler, and such a state of the water proved to be likewise conducive to the conversion of the electricities. I have reason, however, to think that there are other causes by which a change is effected in the electrical states of the boiler and steam-cloud; but before any definite opinion can be formed on this point, I consider further experiments to be necessary.

I shall now speak of the effects of pressure, first, when the steam-cloud evolves *positive* electricity, and secondly, when it liberates *negative* electricity.

In experiments to ascertain the comparative intensity of the electricity evolved by steam at different pressures, it is of course essential that equal *weights* of steam, and not equal *volumes*, should be emitted in equal times; and to effect this condition it is necessary that the discharging aperture should be diminished in the same proportion that the pressure is increased, and *vice versâ*. The safety-valve is in this respect a self-regulating apparatus, because it permits an escape of steam precisely equal to the production, which, with a uniform fire, is the same at all pressures. Another requisite in such experiments is, that the steam should issue in states of equal *dryness*. If a strong fire be used, the discharge at low pressures is so *voluminous*, that it is exceedingly apt to sweep unvaporized water out of the boiler, and thereby to dissipate the electricity, or conduct it to the valve.

By availing myself, then, of the safety-valve as a means of equalizing the escape, and by maintaining the fire at once uniform and moderate, I was enabled to effectuate equal emissions of dry steam, under successive augmentations of pressure, and the following are the results I obtained, so far as they apply to the usual condition of the steam-cloud, or that in which it evolves positive electricity.

In the first place I removed the valve from its seat, and suffered the steam to escape without any restraint whatever;

but under these circumstances I could detect no electrical velopment beyond what was attributable to the combustion the fuel, which always produced sufficient electricity in evaporating apparatus to affect the gold leaves of the elect scope, when a condensing plate was used. I then resto the valve, and when the pressure scarcely exceeded one pou on the square inch, the gold-leaf electroscope, connected w the boiler, gave the first indication of electricity distinguis able from that produced by combustion. The slightest dition to this pressure caused an excessive increase of elect city, and at three pounds on the square inch, small spa could be drawn from the boiler at the rate of five or six i minute. After this each successive equal increase of pressu produced a less augmentation of electricity than the pre ding one. I should say that the electricity manifested at th pounds on the square inch, was not doubled until the pre ure became fifteen pounds per square inch ; not trebled u it reached fifty pounds; not quadrupled until it was raised 120 pounds; and that when the pressure was augmented 250 pounds on the inch, which was as high as I ventured carry it, the electricity of the boiler did not appear to be m than five times greater than at a pressure of only three poun on the inch. Such were the results I obtained; but as causes which tend to produce the opposite electrical states the boiler and steam-cloud were probably not altogeth quiescent in these experiments, the effects might possibly ha been different, if the apparatus had been so constructed as be more or less favourable to the operation of such caus The feebleness of the electricity evolved by the steam d charged from Perkins's gun*, is probably owing to a count acting agency of the nature I have mentioned; and the vari tion which has been observed in the intensity of the electri effects, produced by the steam discharged at similar pre ures from different steam-engine boilers, may be explain upon the same principle.

When circumstances were such as to render the electri states of the boiler and steam-cloud the reverse of what th usually are, the effects of pressure were exceedingly inco gruous and perplexing.

---

* I presume it has been tried whether the steam from Perkins's gun positive or negative; but if not, I recommend the trial to be made, an think it possible that the electricity may prove to be *negative*; especiall the pressure be run down a little and the steam not permitted to esca very rapidly. The condensation which must take place in the gun-barrel w be unfavourable to a development of electricity, and the steam oug therefore, to be discharged, if practicable, by some other means.

'he lowest pressure at which I could detect positive elec-
ity in the boiler, was much the same as that at which ne-
ive electricity first appeared; and an increase of pressure,
ar as about thirty pounds on the inch, augmented the in-
sity of the development in the same ratio, apparently, as
en the boiler was negative; but further additions to the
d on the valve were attended with very variable results,
netimes increasing, and sometimes diminishing the electri-
. The most remarkable circumstance, however, was that a
siderable increase of pressure always produced in the first
tance a temporary recurrence to *negative* electricity in the
ler, which lasted for a minute or two, and then again
e place to *positive* electricity. I suspect that this curious
ct is occasioned by the steam and water acquiring an in-
ased temperature more rapidly than the metal of the upper
t of the boiler, so that when the boiling point correspond-
to the load on the valve is again attained, the metal in
tact with the steam is not at first hot enough to reverse the
tural electrical state of the boiler. If this view of the sub-
t be correct, it follows that the degree of heat which is ne-
sary in the metal to render the boiler *positively* electrified,
reference to the boiling point of the water, and the tem-
rature of the steam.

When the pressure was considerable, the tendency to ne-
ive electricity in the steam-cloud was greatly increased by
sing the steam through the copper pipe acted upon by the
e; so much so indeed, that I could frequently obtain a jet
steam *negatively* electrified from the pipe, at the same time
t a jet of steam *positively* electrified was escaping from the
lve. At low pressures, however, the effect of passing the
am through the pipe was rather the reverse of what I have
ntioned, and I therefore scarcely know whether to regard
result as favourable or adverse to the agency which I
ve ascribed to heated metal.

By much the most powerful effects were obtained when
steam was discharged through a short glass tube inserted
the boiler. When the tube was long a considerable con-
nsation took place within it, which greatly diminished the
ctricity of the jet. The electricity was seldom so strong
en the boiler was positive, and the steam-cloud negative,
when they were in the opposite and usual states.

The neutrality of the steam in the boiler, whether the
am-cloud evolved positive or negative electricity, was clearly
inced by the electricity of the jet not being abated by pass-
g the steam through the copper pipe, which was ten feet
ng, and only three-eighths of an inch in diameter.

The superior intensity of the electrical development w accompanies the emission of high-pressure steam, and the of the upper or distended portion of the jet being more hi electrified than the lower or unexpanded part, seem to fa the supposition that the evolution of electricity in the ste cloud depends upon the *dilatation* of the steam. I hav ready adduced, in former communications, reasons for re ing this hypothesis; but feeling anxious to remove all d upon the subject, I resorted to the following method o termining the question. To the cock F, which termin the copper pipe, I attached a metallic cylinder, which was sufficiently hot to prevent any condensation of steam ta place within it, and was perforated at one end with a m tude of small holes, through which the steam, after expan in the cylinder, was suffered to escape at a density scar exceeding that of the atmosphere. Now if *expansion* produced the development of electricity, the steam would become electrical in the cylinder, and would have parted its electricity in passing through the holes, and thus the ste cloud would have been rendered neutral; but so far from being the case, I could perceive no diminution of electr when the steam was subjected to this treatment. We therefore confidently infer that the liberation of electr in the jet does not proceed from expansion, and that altho the intensity of the electrical development is so greatly fected by pressure in the boiler, it is quite independent o density at which the steam is ejected.

The precipitation of the steam appears then to be the cause to which the liberation of electricity in the jet ca assigned. Assuming this conclusion to be correct, the question is, does the precipitation of the steam give rise to electricity of the evaporating vessel, as well as to that of steam-cloud; or is the electricity manifested in the boiler, effect of evaporation, or of some other process distinct that which excites electricity in the jet? The neutralit the boiler when the steam is confined, seems to imply, the same cause which produces electricity in the cloud casions also the opposite electricity in the evaporating ve I endeavoured to resolve the question so far as regards agency of evaporation, by discharging the steam which time to time remained in the boiler, after all the water been evaporated, and the boiler always became electri provided the pressure of the steam were considerable. cessation of escape from the valve was the only indica I could have of the perfect dryness of the boiler; and if liance may be placed upon this criterion, the conclusi

: is inevitable, that the electricity which is excited in the orating vessel on the emission of steam, is independent ncomitant evaporation. I should observe, that the elec-y which appeared in the boiler after, as well as shortly re, the exhaustion of the water, was uniformly *positive*.

nder these circumstances, I think there are certainly nds for supposing that the same cause which excites ricity in the steam-cloud, produces also the contrary elec-y of the boiler; but if this be so, I am not aware that there iy known principle upon which the effects can be ex-ed, especially as the electricity of the boiler appears independent of the proximity of the jet.

ne transmutation which, under certain conditions, takes in the electrical states of the boiler and steam-cloud, is rt of the subject embarrassed with difficulty. What ble change can the steam undergo in the boiler, either ontact with heated metal, or otherwise, which can cause evolve, on its subsequent condensation, the opposite ricity to that which it usually liberates?

ne effects of pressure appear equally inexplicable. It seems iceivable to me that steam should acquire by pressure any erty which would not be taken away by expansion, and yet nd that the influence of pressure in the boiler, upon the ricity of the jet, is not destroyed by suffering the steam late before it is ejected.

ear this paper has extended to a somewhat tedious length, [shall therefore conclude it here by expressing a hope what has been stated may have the effect of stimulating ry into the curious subjects of which I have treated.

Wm. Geo. Armstrong.

On the supposed Moraines of Ancient Glaciers in Scotland. William Kemp, Esq., introduced by a Letter from J. E. wman, Esq., F.L.S.

[Illustrated by Plate III.]

he Editors of the Philosophical Magazine and Journal.

Gentlemen,

l. KEMP, of Galashiels, having favoured me with a copy of a paper lately drawn up by himself, containing roofs of the former existence of glaciers in Scotland, I ht it desirable that they should be generally known, be-I am aware that some of my ablest geological friends are g for further evidence (and wisely so) before they adopt the theory of Prof. Agassiz. I therefore wrote to request Mr Kemp to publish this paper, and illustrate it by diagrams In his reply he modestly states that he had no design of doin this, "being, as you will at once perceive, no fit person fo preparing papers for the press;" but he grants me full permission to do so, if I think it worthy, and has inclosed th accompanying diagrams, respecting which he says, "Thes very rude and hasty sketches of those gullied tracts called th Meigle Pots, together with the vale above Holilee, are prett correct, being copied from sketches taken on the spot. Bu that of the vale of Galashiels is not so faithful, I having onl examined it in detail; that is, the mounds round Gala Hous are not at all drawn to the truth, I having as yet got no leisur to take a plan, nor can I for many weeks to come."

Mr. Kemp's account of the "Meigle Pots" is highly interesting and important; and being, as far as I know, the onl instance as yet observed in Britain of this modification of glacier action, it appears to me that I should have been remis had I not suggested its publication. To make it more intelligible, I have given in a note an extract from a short but abl account of the *Creux* and *Lapiaz* of Prof. Agassiz, by th talented editor of the 'Scotsman,' Mr. C. Maclaren. I wi only add, that some of the extensive mounds of gravel describe in the paper may possibly have been modified, if not originall produced, by the subsequent action of torrents.

J. E. Bowman.

Manchester, 10th Feb., 1841.

On the Moraines of Ancient Glaciers, &c. By Wm. Kem

Read to the Galashiels Geological Society, Jan. 29th, 1841.

Gentlemen,

Having had my attention turned to this interesting subjec first by Mr. Bowman's opinion that the terraces in this neighbourhood, which I attributed to the action of water, are i fact the moraines of ancient glaciers, besides having read i the Athenæum and other periodicals of Prof. Agassiz's discovery of similar evidences in different parts of Scotland, &c. I, among many others, hailed the announcement with grea pleasure, as conviction at once flashed upon my mind, that hi discovery, by the clear description given of those ancient moraines, would at once apply to those beautiful combs or mound of gravel that have been so frequently noticed in many part of the country, of which no satisfactory account had hithert ever been given. How gratifying to the ardent followers of tha science, is the discovery of that great man, in pointing to, an giving such a clear exposition of, these hitherto mysterious formations!

Such moraines as were probably formed by ancient glaciers are very conspicuous in the vale of the Gala, immediately around the town of Galashiels. The vale there will be at one part about three quarters of a mile broad, by upwards of three miles long, surrounded by hills of considerable elevation, except where the Gala enters the vale by a narrow passage at the north-west, and again where it opens to the south-east, and joins the more spacious vale of the Tweed. The glaciers there appear at one time to have extended over the vale to a great height, as lateral mounds of debris can be traced about 300 feet above the level of the water. But I wish to confine my observations to what was possibly a later æra, when the glaciers had much diminished, and were perhaps not above 150 feet high; for it is then that they seem to have left their most legible traces.

Nearly half a mile below Galashiels, where the Gala flows through a narrow channel, with high banks on each side, there is a very conspicuous moraine about 140 feet high upon the north-east side, and about 600 feet long, extending across the lower end of the vale. (See No. 2. B.* Pl. III.) Upon the south-west side, in a direct line, it can likewise be traced for a considerable distance, where it takes a curvilinear bend to the west, until it becomes imperceptible in the higher ground. The greater part of that moraine is composed of clay and boulders, many of which are quite sharp and angular, but the greater portion are rather well-rounded; and what, perhaps, is worthy of notice, the top, for about 25 feet down, is composed of unstratified gravel and coarse sand, which has seemingly been caused by the water filtrating through the mass while it was being thrown up, and sweeping away the finer comminuted particles. The breach through which the water flows is about 500 feet wide. This is, perhaps, what may be called a terminal moraine, left on its final retreat by the glacier of the Gala valley.

At the west end of Galashiels, rather more than a mile upward from that moraine, the remains of another may be seen, standing out from the banks on each side, the south end in particular being very conspicuous. (See No. 2. C C.) Likewise, along each side of the vale, there are lateral moraines from 50 to 70 feet high, in some places very distinctly marked.

A number of these interesting formations is to be seen near

---

* The dotted portions indicate the moraines.

Gala house (see No. 2. A.), in a fine sheltered situation, where the vale extends to the south for a considerable distance, at a lateral opening between the hills.

Nearly parallel with the Gala there is a lateral moraine, which divides the town from Gala house and parks, and behind that there is a series of beautifully formed mounds, ranging nearly parallel one with another. These are not continuous, although some parts are of considerable length: one, called the long knoll, being about 700 feet long, of a serpentine form, averaging 35 feet high; others are like small hillocks, from 20 to 40 feet high, with narrow trough-like valleys between. To use a simile, I thought, while examining these interesting relics, that to convey the clearest conception of that place, would be to imagine portions of the rolling waves of a troubled sea to be instantly arrested in their course, while the intervening parts passed onward for a little space, and were then likewise immovably fixed.

Such seems to have been the action of glaciers in the vale of Galashiels, as to have scooped out the basin in some places to an unknown depth, which had afterwards been filled up by impalpable sedimentary sand, over which there is a covering of gravel varying from four to six feet deep, so that a great part of the town may literally be said to be built upon a quicksand*.

There are many lateral moraines along the valley of the Tweed; but the only remarkable terminal ones I have yet observed, are two, one at Dryburgh, the other at Holilee. The first is upon the Dryburgh side of the river, and memorable enough by a fine Doric temple being built upon its summit by the late Earl of Buchan. The one at Holilee (see No. 1.) has attracted the notice of many a passing traveller, by its singular uniformity and beauty; so much so, that the most skilful engineer could not construct a finer mound. It crosses the lower end of an extensive vale, immediately below which the hills on each side press close upon the river. The northern part of this mound is about forty feet high, and is composed of rolled gravel. The road to Peebles runs through it. It has swept across and along the opposite side of the vale for about a mile long, but the river has long since carried away a great part of it. A few years ago a splendid portion of it rose out of the vale to the height of sixty feet; but the farmer, unmindful of this fine geological monument of ancient time, after long cursing it for a barren and unproductive track, at considerable expense caused it to be much reduced in height, and now the plough (still with difficulty) passes annually over it*.

---

* This sand below the gravel is probably of anterior origin.—J. E. B.

I have likewise observed several remains of moraines along the vale of the Ettrick. About three quarters of a mile above its junction with the Tweed, at the south end of the fine new bridge upon the road to Selkirk, the approach to the bridge is cut through a very bold one. It sweeps round the lower end of the rich haugh of Lindean, and forward to the side of the water, where it turns obliquely to the stream. About half a mile further up the river at Bridge-heugh, there is another upon the same side of a still bolder character, forming an immense bank about sixty feet high, crossing the vale about two-thirds of its breadth, which is there about half a mile broad, it having evidently once crossed it to the opposite side, as a part in a direct line is still remaining. Following up the water, we observe vast lateral moraines on each side. Upon the north there are three curvilinear ranges of hillocks, some 20, 40, and 50 feet high; these join towards the west, in a cross mound, terminating in an abrupt precipitous angular ridge at the side of the water, which at present undermines its broken end. This is fully sixty feet high, and, like the one at Bridge-heugh, seems to have crossed the vale, by the evidence of a remaining portion boldly projecting out of the south bank, a little below the extensive factory of the Messrs. Browns. Gentlemen, these are a few of the leading proofs by which I have attempted to prove the former existence of glaciers along the valleys of this district. You are all more or less familiar with those remarkable mounds I have pointed out; and if you turn to M. Agassiz's description of moraines, you will find that these are similar in every respect.

As a further proof, I have observed the striated and grooved appearance of the rocks in many places in this neighbourhood, particularly at a hill nigh Redhead, by the vale of the Caddon, and even in our own town, at the back of the Buckholmside factory.

At a land-strait or water-shed, between the Gala and the Tweed, about two miles and a half west from Galashiels, on the north side, upon a steep spur of the Meigle Hill, which here rises at an angle of near thirty degrees, there are three lines of round hollows with gutter-like tracks between. (See No. 3.) They are known by the name of the "Meigle Pots," and are each nearly 200 feet long, not in straight lines, b of a serpentine form. The highest (No. 3. A.) is about 2 feet above the vale, running *nearly horizontally across* t steep spur of the hill, and has three circular hollows in i course, two about fifteen feet deep, and the third about thirt five, the tracks between being from 10 to 12 feet deep. T next (No. 3. B.) commences close below the west end of th horizontal one, bending to the right and left down hill, a is more uniform in depth than either of the other two. contains two deep and finely rounded oval holes, one abo thirty, the other forty feet deep. The third (No. 3. C.) begi near the middle of the upper one, and continues down hill a slanting direction towards the east: it has the most rugg appearance of the three, the rock being bare and precipito upon the higher side, and terminating at the lower end wi a circular cavity about fifty feet deep. The rough markin on the more precipitous or eastern sides of the hollows *a* a *c* represent the bare rock partially covered with soil and veg tation; but none such occur on the side of B, which runs do hill with the ground nearly level on each side, covered with fine grassy surface.

---

* This seems to be a beautiful example of a terminal moraine united to a lateral one, very similar to that of the Viesch, described by Agassiz, and represented in the 9th Plate of his *Etudes sur les Glaciers de la Suisse.* —J. E. B.

The sides of these gullies are very steep, although most covered with a fine grassy surface. It is evident they h been originally much deeper, having been greatly filled up debris, which from time to time has fallen from the heigh above*. By what means can we conceive these hollows have been scooped out of the rock, except by the agency water, and by water falling from a considerable height? T rock seems favourable for being worn down in that mann the graywacke strata rising near to the perpendicular, a the beds of the rock being thin, intersected with numero cutters (joints). Yet where is the Water? None runs the now, nor can ever have run there in the present state of t locality. It seems to me likewise that these hitherto una countable ravines have been clearly explained by M. Agas in his description of the *Creux* and *Lapiaz*, names given similar holes and guttered tracks which he had observed up the flanks of the Swiss Alps, and which, he says, have be worn out by water pouring through fissures in the glaci while they were slowly pressing onward†.

---

* The Meigle Hill rises about 450 feet above the rude pillar represen in the sketch No. 3. This pillar stands about 60 feet above the upper l of hollows marked *a*, and is built on the lowest *terrace* I can trace in t quarter, which is No. 10 of the series described in Chambers's Journ but others are more distinctly marked upwards to the south-west.

† On the sides of the Swiss valleys, round holes, such as cascades ma

Gentlemen, however hastily surveyed and imperfectly described, this is still another proof in confirmation of the glacier theory in Scotland, so lately promulgated by the great philosopher of Neufchatel, to whom the geological world is so deeply indebted, by revealing so bright a page in the dark history of the past. W. Kemp.

LVII. *An Examination of the Phænomena of Conical Refraction in Biaxal Crystals.* *By* R. Potter, *Esq., B.A.*[*]

THE verification of the two species of conical refraction, which were discovered by Sir William Hamilton to be results of Fresnel's analytical expression for the wave surface in biaxal crystals, forms an epoch in the progress of the conversion of the scientific world to a belief of the undulatory theory of light. Many waverers were confirmed in their belief by so singular a coincidence of theory and experiment; and Professor Lloyd, who made the experiments, had a harvest of reputation from them, such as is seldom reaped in the field of science.

I joined with the scientific world, in the confidence which it gave to Professor Lloyd's investigations; and although I had seen the undulatory theory fail in so many important cases, yet I believed that the true law of refraction in biaxal crystals had been discovered by Fresnel, as certainly as that of uniaxal crystals has been by Huyghens; but in both cases from wrong premises. It was from a desire to view these interesting phænomena, that I availed myself of an opportunity

are sometimes found in the rock, but in places remote from running waters, and where the form of the surface will not permit us to suppose that any cascade could ever have existed. In other cases, a long, sinuous, dry, water-worn gutter or channel is observed, the course of which runs *across* instead of along the natural declivity of the ground. The study of the glaciers has enabled Agassiz to find a key to these enigmatical phænomena, which had perplexed previous inquirers. Streams of water flow along the surface of a glacier, and when one of these falls into a fissure which is open to the bottom, if it forms a cascade, it cuts a round cavity in the rock with the gravel and sand which it either finds there or carries down with it, as some of our rivulets work out the hollows termed caldrons. If the glacier is travelling downwards the cascade will travel with it, and convêrt the round cavity into a long gutter; or supposing the water to reach the bottom without falling in a cascade, still, in finding an issue below the glacier, it will be compelled to follow the sinuous openings left by inequalities in the bottom of the ice, and thus take a course at variance with the natural inclination of the surface. We have here an explanation of the *Creux* or holes, and the long water-worn gutters found in such unlikely situations, which bear the local names of *Lapiaz* or *Karren*. These are chiefly observed where the rock is soft, and are seldom visible on the granite.—*Scotsman*.

[*] Communicated by the Author.

of purchasing some crystals of arragonite, in order to have pieces of larger dimensions than I possessed before, and which from perusing Professor Lloyd's paper, in the seventeenth volume of the Transactions of the Royal Irish Academy*, I conceived to be necessary. The prepossession I had conceived in favour of Fresnel's law, as expressed in the equation to the wave surface in such crystals, was, however, changed into distrust, when I was enabled to observe the phænomena. I employed a different mode of experimenting from Professor Lloyd, on commencing; and possessed more confidence in the properties of an eye-lens. Such a lens exhibits to the sight what takes place at the focus of the eye and the lens, as an optical combination, when there is a real image; and when there is a virtual image, it is exhibited such as would have really arisen if the rays of light in falling upon the eye-lens had proceeded in a similar manner from a real image. The transition from a virtual to a real image, and *vice versâ*, occasions no difficulty; for both arise from the form of the pencil of rays, as it falls on the eye-lens; which is, in fact, the thing concerned in the representation of the image to the sense of vision.

Professor Lloyd passed a pencil of sun-light through the crystal, and received the transmitted pencil on a screen of ground glass, in most of his experiments, and only mentions using a lens whilst seeking for the conical refraction within the crystal which gives a cylindrical refraction in air; and then soon gave it up again for sun-light. I have chiefly worked with an eye-lens, or else for micrometer measures with a compound microscope; and having tried sun-light also, I concluded that it bears in accuracy some such relation to the former method as a camera-obscura bears to a telescope, and yields a less perfect result; but one which, as far as it goes, is exactly the same as that from the other method. With the screen you can only examine real images; with the lens you can examine virtual ones also, and that in a more critical manner.

Arragonite having been judiciously chosen by Professor Lloyd, on account of the considerable angular separation of its optic axes and its double refractive energy, as well as on account of the refractive indices, in the principal directions of the crystal, having been determined by Professor Rudberg, I have continued to use it solely. I have employed slices of the mineral cut perpendicularly to the axis of the crystal, which is parallel to the line bisecting any pair of optic axes, and of the thickness one-seventh, one half, and five-sixths of an inch.

---

* [Prof. Lloyd also described these phænomena in L. and E. Phil. Mag., vol. ii. pp. 112, 207.—Edit.]

: one of one-seventh of an inch thickness does not give suf-
ent separation of the images for convenient use, and the
of five-sixths of an inch had its inconveniences, arising from
fact of crystals of arragonite being formed of twin crystals;
it is seldom that any component crystal is large enough
llow a ray of light to traverse the optic axis for any great
ance without entering the twin one. This was only in
ie places possible with the specimen referred to, so that
experiments about to be related were generally made with
crystal of half an inch thickness.

The experiment with sun-light was made as follows: a piece
infoil with a small hole in it, made with the point of a pin,
ig attached to one surface of the crystal, and the whole
a attached by means of wax to a moveable adjusting ap-
atus fixed to a stand, I placed the crystal in the sun-light
ected horizontally through a window by a mirror, with the
oil towards the incident light, so that the pencil entering
small hole would be in angular diameter 32′ nearly, or
sun's apparent diameter; then placing a piece of tissue-
er on the other surface, I saw generally upon it two round
ght spots; but when the crystal was moved until the light
ering it traversed the optic axis, or rather was symmetric-
refracted with regard to that axis, then there appeared
: round bright spot, with a dark ring, and then a broader
ght ring round it: now drawing the tissue-paper away
n the surface gradually, but keeping it parallel to it, the
ker ring gradually disappeared, and the section of the
cil became larger, the central spot becoming more and
re faint, and expanding into a conical pencil which blended
h the other conical pencil arising from the bright ring.
ese phænomena show that within the crystal the light had
n refracted in the form of a small solid cone, approxi-
ting to a cylinder, whose section was the bright spot seen
he second surface, and a larger hollow cone whose section
: the bright ring. After emergence into air we see that
bright ring expanded into a hollow cone; but the bright
t, gradually disappearing, gave rise to an indefinite number
conical surfaces of various angles.

These and other properties of the emergent pencil are
ch better examined by means of the eye-lens; and first,
cing the crystal in ordinary day-light coming through a
ll round aperture at some distance, with the second sur-
: in the focus of the eye-lens, we perceive the same ap-
rances as were seen on the tissue-paper at the second sur-
: of the crystal. Using now a triangular aperture, the
spots appeared triangular images of the aperture, and at

the optic axis the ring and spot possessed some trace of this triangular relation. The triangular appearance arose from the well-known optical property of a small hole, which gives on a screen beyond it an imperfect image of a bright object in front of it. In the experiment with the sun-light, although the central spot was round, yet it cannot be considered an image of the sun, in the same way as the two round spots generally seen.

The change from two spots to a ring and a spot was sudden, upon the direction of the light becoming that requisite for refraction symmetrical to the optic axis; and in experiments which will be related shortly, when a minute pencil of incident light was transmitted through a very minute aperture in the tinfoil, it required very nice adjustment to obtain the ring and spot. This shows that if a converging pencil were incident at the hole on the first surface, with its axis in the direction requisite for refraction symmetrical to the optic axis of the crystal, then no other ray but that in the axis of the pencil could be subject to the singular refraction in the crystal: but the other rays of the incident pencil giving each two rays within the crystal, these latter would be arranged around the optic axis, as the incident rays were around the axis of the pencil, and the appearances would be symmetrical; although introducing, in proportion to the magnitude of the pencil, confusion and indistinctness into the real phænomena of conical refraction. Professor Lloyd says, "The first-mentioned species of conical refraction, it has been observed, takes place *in air*, when a ray of common light is transmitted within the crystal in the direction of the line joining two opposite cusps of the wave. If we suppose such a ray to pass in both directions out of the crystal, it is evident that it must emerge similarly at both surfaces; consequently, the rays which are transmitted along this line within the crystal, and form a diverging cone at emergence at the second surface, must be incident in a converging cone at the first. Having, therefore, nearly ascertained the required direction by means of the system of rings in polarized light, I placed a lens of short focus at its focal distance from the first surface, and in such a position that the central part of the pencil might have an incidence nearly corresponding to the cusp-ray within," &c. And again, he says, "But to examine the emergent cone, it was necessary to exclude the light which passed through the crystal in all but one direction. For this purpose a plate of thin metal, having a minute aperture, was placed on the surface of the crystal next the eye, and the position of the aperture so adjusted, that the line connecting it with the luminous point on the first surface might be, as nearly as possible, in the direction of the cusp-ray. The exact adjustment to this direction was made by subsequent trial." After relating some preliminary experiments, he says again, "In all these experiments the emergent rays were received directly by the eye placed close to the aperture on the second surface. It was obviously desirable, however, to receive them on a screen, and thus to observe the section of the cone at different distances from its summit. After some trials, I effected this with the sun's light, the light of a lamp being too weak for the purpose. The emergent cone being made to fall on a screen of roughened glass, I was enabled to observe its sections at various distances, and therefore with all the advantages of enlargement. The light was sufficiently bright, and the appearance distinct, when the diameter of the section was between one and two inches."

This arrangement appears to have been continued throughout this series of experiments; for after discussing his results, he says, "Inasmuch as the cusp-ray, within the crystal, corresponds to a cone of rays without, it is evident that there must be a *converging* cone incident on the first surface, equal to that which diverges from the second."

We see in the preceding discussions, that Professor Lloyd, by experimenting with the view of verifying theoretical consequences, has fallen into two errors; the one an error of assumption, that a conical incident pencil of common light could be refracted as one ray within the crystal; and the other an error of omission, in not discovering that, however small the incident pencil may be, the refracted light consists of a larger cone and a smaller one, nearly a cylinder. The first error arose from applying the reasoning of geometrical optics, for uncrystallized media and common light, to the case of a double refracting medium and polarized light; for Professor Lloyd himself had discovered a peculiar state of polarization in the emergent pencil (he says, "Thus it appeared that all the rays of the cone are polarized in different planes"), and to have given the least chance of success the incident pencil ought to have been in the same state.

To investigate the form of the refracted light in a more severe manner, I placed an aperture made in a plate of metal, with a small pin, about one-fiftieth of an inch diameter, at seven inches from the flame of a lamp; and at sixteen inches on the other side of the aperture I placed the crystal with tinfoil attached to its first surface, and examining the light which passed through a minute hole, about one-thousandth of an inch in diameter, in the direction for refraction symmetrical to the optic axis, I found it on emergence giving the ring and central spot. In this experiment the angle of the incident cone was rather more than 4′.

In this experiment the light was reduced to a small pencil by apertures in thin metal plates, but I have seen the same phænomena when the incident pencil arose from a luminous point formed by a lens of short focus, at some distance from the crystal. The results of Professor Lloyd's method of experimenting were, that in the first instance he found the angle of the emergent cone about double what the calculation from theory gave, but by reducing the diameter of the aperture on the second surface this observed angle was reduced also; until with a very minute aperture an approximation to the calculated angle was obtained; and a hollow cone was seen, as required, in place of a solid cone, which appeared in the first instance.

It appears, that in order to obtain any tolerable approximation to the calculated quantities, an aperture was required to be applied to the second surface, so as to cut off all the ring and a large portion of the central spot. Such experiments cannot be considered as investigations of the refraction of biaxal crystals in the neighbourhood of the optic axes. The real nature of this refraction will be seen when we have examined Professor Lloyd's investigation in search of the conical refraction within the crystal giving a cylinder of rays in air.

He says, "The second kind of *conical refraction*, whose existence has been anticipated by Professor Hamilton, depends (it will be remembered) on the mathematical fact, that the wave-surface is touched in an infinite number of points, constituting a small *circle of contact*, by a single plane parallel to one of the circular sections of the surface of elasticity. It takes place when a *single external ray* falls upon a biaxal crystal in such a manner that one refracted ray may coincide with an optic axis. When this is the case, there will be a *cone of rays* within the crystal, determined by lines connecting the centre of the wave with the points of the periphery of the circle of contact. The angle of this cone is equal to

$$\tan^{-1}\frac{\sqrt{a^2-b^2}\,.\,\sqrt{b^2-c^2}}{b^2}$$

and its numerical value in the case of arragonite is 1° 55′, assuming the values of the three indices as determined for the ray E by Professor Rudberg (see Trans. R.I.A., vol. xvii. page 151.).

"As the rays constituting this cone will be refracted at emerice in a direction parallel to the incident ray, they will m a small cylinder of rays in air. This cylinder, it will seen, is in all cases extremely small; for the diameter of its tion made by the surface of emergence subtends an angle 1° 55′ only, at a distance equal to the thickness of the cry- l. Hence the experiments required to detect its existence l measure its magnitude, demand more care and precision in those already described.

" The incident light was that of a lamp placed at some di- nce; and in order to reduce as much as possible the breadth the incident beam, it was constrained to pass through two all apertures, the first of which was in a screen placed near flame, and the second perforated in a thin metallic plate oining to the first surface of the crystal. Under ordinary cumstances, it is obvious the incident ray will be divided o two within the crystal, and these will emerge parallel m the second surface. I was able to distinguish these two 's by the aid of a lens; and turning the crystal slowly, so to vary the incidence gradually, I at length observed that re was a position in which the two rays changed their re- ive places rapidly on any slight change of incidence, and peared at times to revolve round one another, as the inci- ice was altered. Being convinced that the ray was now ir the critical incidence, I changed the position of the cry- l, with respect to the incident ray, very slowly; and after ich care in the adjustment, I at last saw the two rays spread o a continuous circle, whose diameter was apparently equal their former interval.

" This phænomenon was exceedingly striking. It looked like small ring of gold viewed upon a dark ground; and the lden and almost magical change of the appearance from luminous points to a perfect luminous ring, contributed t a little to enhance the interest.

' The emergent light, in this experiment, being too faint to reflected from a screen, I repeated the experiment with sun's light, and received the emergent cylinder upon a all piece of silver paper. I could detect no sensible dif- ence in the magnitude of the circular sections at different tances from the crystal.

" When the adjustment was perfect, the light of the entire nulus was white, and of equal intensity throughout. But en there was a very slight deviation from the exact position, opposite quadrants of the circle appeared more faint than other two, and the two pairs were of complementary co- irs. The light of the circle was polarized, according to law which I had before observed in the other case of conical refraction. In this instance, however, the law was anticipated from theory by Professor Hamilton."

With the exception of the last sentence but one, this description gives a correct and clear account of the appearances, but under a condition which Professor Lloyd has omitted to notice. The luminous ring is seen, and seen perfectly only, when the lens is so placed in its distance from the crystal, that what he calls the two rays, are, in fact, the two virtual images of the luminous point on the first surface. The position of these virtual images within the crystal is found by the formulæ of geometrical optics, their distance from the second surface, when the incidence is nearly perpendicular, being equal to $\frac{\text{thickness of the plate}}{\text{refractive index}}$.

When the eye-lens has any other distance than that which gives the distinct images of the aperture, we find the appearances changed, and that the bright ring is not the section of a cylinder of equal diameter at all distances from the crystal, as Professor Lloyd supposes he has made out, by means of sunlight. That he should have overlooked phænomena so interesting, can be accounted for only by his devotion to his theory, which has thus robbed him of a fine discovery.

Whether we draw the eye-lens away from the crystal, or push it nearer, we find similar appearances, and we see that the ring was formed by the intersection of two cones (or more correctly of two series of cones), the one converging so as to have the bright spot, we formerly found on the second surface, for its vertex, and the other diverging, and having the ring, formerly found on the second surface, for its base. I say more correctly two series of cones, for we formerly found that the bright spot only gradually became fainter. The same appearances are seen in pushing the lens nearer the crystal, for gradually one portion of the light of the ring increases in diameter, whilst the other diminishes, and a bright centre is formed, which comes to its smallest magnitude, and then diverges again into a solid cone of rays, having a bright centre which gradually fades away. The phænomena show that the cones, the one virtual as being beyond the second surface of the crystal, measured from the eye, and the other real as being formed in the emergent light, are in reality a series of cones having their vertices on opposite sides of, and at different distances from, the ring. When the rays of either series of cones are collected into their smallest space forming the bright spot, this is evidently only an approximate effect; and when the incident pencil is very small, this spot is seen evidently outside the crystal, rather than on the second surface for one series.

iese results are certainly not in accordance with the theo- al investigations of Sir William Hamilton, whose dis- ons of the singular refractions near the optic axes, as in- ed by Fresnel's expression for the wave-surface, have referred to in the quotations from Professor Lloyd's r.

ie law of the polarization in the virtual ring and one of cones, as far as he investigated it, were observed by Pro- r Lloyd; but if my view of the mode of formation of the by the intersection of two series of conical surfaces be ect, then there must be a peculiarity in the polarization rto unsuspected. The ring, as stated by Professor Lloyd, larized in every part in a different plane; so that if we h a prism of calcareous spar, giving light polarized in plane, to our eye-lens, we see that one small portion of ring vanishes in every position of the prism, and that as urn the prism and eye-lens round the line of vision, the part in the ring moves through the double of the angle h the prism does, making a whole revolution whilst the n has made the half of one. Now if the ring be the in- ection of two series of cones, whilst one of these series verging and the other converging, the part which disap- s, in any one position of the prism, should be in the same or, measured from the optic axis as centre, in both series; after one has ceased to converge, the bright spot then ied should exhibit no polarization whatever, being com- ed of light polarized in all planes; and when it diverges n, the part which disappears should be in the directly op- te sector to that which disappears in the series of cones h has continued to diverge.

his is exactly what we see; but where the two series of es are diverging together, they soon overlap, and as they polarized in planes at right angles to each other at every it where they overlap, the light there exhibits no signs of rization; so that, for the internal series, it is only before as begun to overlap the other, that the polarization is evi- t. In this small space it is very easy to overlook it, and elieve I should not have discovered the law unless guided ny theoretical view, as to the formation of the ring.

t was before stated that the ring appeared at the position he virtual images within the crystal: now as the images e their distances from the surface slightly different, de- ding, as we have seen, on their respective refractive indices hat point, therefore the two portions of the ring will have erent positions of greatest distinctness; and this is easily n, when experimenting in a dark room, with a bright pencil

of light from a closed lamp, and when the incident pencil
very small; for then, when we desire to obtain the ring wi
maximum distinctness, we can see, that as one portion of
becomes more distinct the other becomes less so, on varyi
slowly the distance of the eye-lens.

A popular explanation of the formation of the ring, a
consequently of the two series of cones, may be thus give
any physical point being taken in the small hole on the fir
surface of the crystal, it will have two images polarized
planes at right angles to each other; these are two points
the extremities of some diameter of the ring; and every oth
point having similarly two images, the locus of all these imag
forms the ring. Speaking more correctly, if we call
and E_2 the two images of any physical point, then the loc
of E_1 forms one ring and the locus of E_2 the other, as befo
described.

The conclusion we come to, from the preceding discussion
is that the phænomena of conical refraction were not adequate
examined by Professor Lloyd. It may be that Sir Willia
Hamilton and Professor Lloyd have expected results fro
their experiments for which Fresnel's expression for the wav
surface in biaxal crystals gives no warrant. The cylindric
refraction in air, being merely the transition from a series
converging cones to a series of diverging ones, may not
separable from them by any mode of experimenting. M
present impression is that no such cylindrical refraction e
ists, but it is a point for future careful experimental re-e
amination, accompanied by an attentive comparison with th
properties of Fresnel's expression. The angle of the co
which has its vertex at the second surface of the crystal,
find to be about 3° 30′, or something less than this, conside
ing a point outside the crystal as the vertex; for a series
careful micrometrical measures with a compound microsco
of a magnifying power of twenty-six times, and a minute i
cident pencil of lamp light, gave the diameter of the rin
1-53·4th of an inch, and by finding the apparent position
the ring, within the crystal, we have the angle of the cone
above. Now the angle from Sir William Hamilton's calc
lations should be about 3°, a sufficiently near coincidence
produce an impression in the mind in favour of Fresnel's e
pression.

We saw, however, that one image of the sun, on the s
cond surface, in the first-mentioned experiment with sun-ligh
on approaching the position of the optic axis, expanded int
a ring, whilst the other image became an ill-defined point i
its centre: it is seen also that the two images leave the opti

axis in the same relation to it as that in which they approached it. From this we conclude, that the space round the optic axis within the ring is traversed by only one image, and therefore that there is only one sheet of the luminiferous surface in this place: the form of the two series of cones also favours this view. This indicates what may not inaptly be termed an eyelet-hole in one sheet, and a conical point in the other.

To determine the real nature of the refraction about the optic axes of biaxal crystals, will evidently require a good deal more of minute examination.

If any fellow-labourers should enter on the subject, I should be very glad to see their results in print, and am glad to be able to inform them that Messrs. Watkins and Hill have undertaken to fit up for sale crystals of arragonite, mounted in a mode so as to facilitate the examination of the refraction near the optic axes.

Queen's College, Cambridge, Feb. 13, 1841.

LVIII. *On the Voltaic Decomposition of Aqueous and Alcoholic Solutions. By* ARTHUR CONNEL, *Esq., F.R.S.Ed., Professor of Chemistry in the United College of St. Salvator's and St. Leonard's, St. Andrew's.*

[Continued from p. 249, and concluded.]

II. *Alcoholic Solutions.*

I SHALL content myself with simply referring to the experiments, by which I have shown that under voltaic agency water entering into the constitution of absolute alcohol is resolved into its elements, hydrogen being given off at the negative pole, and oxygen being engaged in producing secondary effects on the hydrocarbon of the alcohol, and to those demonstrating the extraordinary effect which the solution of the minutest quantities of alkalies, acids and saline bodies, have in promoting this action*.

This fact, that the water of absolute alcohol suffers decomposition under galvanic action, furnishes the key for solving all cases of the voltaic decomposition of alcoholic solutions, by assimilating them to those which occur in aqueous solutions. The analogy between the two cases is complete, with the difference merely, that in the former the appearances are less marked, from the smaller quantity of water present, the inferior conducting power of the solution, and the slight modifications of secondary action due to the presence of hyd carbon. In short, as respects voltaic agency, an alcoholic lution may be regarded as an aqueous one, in which a fore substance is present retarding the principal action, but affo ing an additional source of secondary agency.

---

* Edinburgh Transactions, vol. xiii.; Jameson's Journal, 1835; and London, Edinburgh and Dubl. Phil. Mag., Dec. 1841.

Accordingly, when the alcoholic solution of an ordin salt is acted on, the acid and alkali go to their pro poles, as in an aqueous solution, but much more slowly, the hydrogen of the water of the alcohol is evolved at negative pole, whilst the oxygen enters into second combination. When the base is of easy reduction, metal duced by hydrogen appears at the negative; whatever s stance is dissolved, if it is not of a nature to afford room fo secondary action with hydrogen, that element is given off the same proportion as from water, due regard being had some circumstances connected with the conducting powe the solution which have been formerly pointed out*. T fact was verified for alcoholic solutions of alkalies, ha salts, and oxyacid salts.

It was also found, by experiments similar to those w aqueous solutions of hydracids and haloid salts, that w iodine appears at the positive pole in alcoholic solution hydriodic acid or of iodides, it is due to secondary act Absolute alcohol was charged with dry hydriodic acid gas, placed in A, fig. 2, (p. 243) and water in B and C, A be made negative, and C positive by seventy pairs of four-i plates. Effervescence soon arose from both poles, but no dis loration was anywhere observed, nor acid in B or C until a the lapse of twenty minutes, when a slight brown discolora from liberated iodine commenced in C with acid react In half an hour the battery was reversed, when the hydri solution was instantly discoloured without elastic fluid f the pole in that solution, and with effervescence from other. This experiment would of course have had more a logy to those with water, if B and C had contained alcol but the feeble conducting power of that liquid, and the of interfering with the reactions if any substance was dissol in it, prevented its employment. The appearances are, h ever, best explained on the view that the water of the alco only suffers direct decomposition.

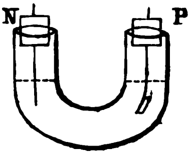
Analogous experiments led to the same view in regar iodides.

The appearances with a positive pole of zinc led to same conclusion. When a saturated solution of dry io of potassium in absolute alcohol was thus acted on by s of two-inch plates in the bent tube, fig. 3, oxide of zinc ı separated at *both poles* without appearance of iodine, or of effer- ence at the positive. The de- tion of oxide at that pole is in formity with the view of the direct omposition of water.

---

* Lond. Edinb. and Dubl. Phil. Mag., Dec. 1841.

Fig. 3.



Vith a similar solution of dry ride of lithium, oxide of zinc soon rated at the negative pole, with effervescence from that , but none from the positive; and it was somewhat uncer- whether any separation of oxide took place at the positive: little doubt could exist that the oxide originated, as in the of iodide of potassium, by the action of oxygen of the er of the alcohol on the zinc, and was subsequently dis- ed and transferred to the negative pole.

The principal condition of the deposition of oxide of zinc he positive pole, whether in aqueous or alcoholic solutions, ears to be a pretty rapid formation, from brisk action; the less powerful the acid, and the less its quantity drawn he positive side, the more of the oxide separates previous olution and transference.

With respect to pyroxylic solutions, I have made few ex- iments; because if the general rule holds good in regard alcohol, there can be little doubt that it will embrace pyr- lic spirit, since, as I formerly showed, the decomposition of water is much more readily effected than that of alcohol. ound, experimentally, that when a solution of dry iodide of assium in rectified pyroxylic spirit was placed in a tube A*, l water in a tube B, the two being connected by asbestus, l A made negative, and B positive by fifty pairs of two- h plates, although iodine soon appeared in the neighbour- od of the positive pole in B, yet it was accompanied by acid sing into the water of B; and after forty minutes' action se appearances continued the same, only more decided, l without any appearance of iodine elsewhere. There is le doubt that the nature of the action was just the same as aqueous and alcoholic solutions.

In the whole circumstances, although the evidence may not of quite so decided a character in some of the cases of al- iolic solutions as in regard to those in water, still I think ere need not be much hesitation in laying down as a still ore general proposition than that above stated, that "When utions of primary combinations of elementary substances, water and in those liquids, such as alcohol and pyroxylic spirit, which contain water as such as an essential constituen are submitted to voltaic agency, the dissolved substance is no directly decomposed by the current, but only the water of th solvent."

---

* Fig. 1. (p. 243.)

III. *Æthereal Solutions.*—Rectified æther gives no symptom of decomposition, nor of conducting power, under th influence of powerful currents, such as that from 200 pair of four-inch plates; and although $\frac{1}{10\cdot000}$th of potash has marked influence in promoting the voltaic decompostion c alcohol, the largest quantity of that alkali which æther is capable of dissolving has no effect in affording room for an galvanic agency. Neither do any of the ordinary substance soluble in æther, such as corrosive sublimate, chloride c platinum, or chromic acid, produce any such effect. Nor is a oxyacid salt, such as nitrate of uranium, when held in solutio by it, resolved into its constituents under galvanic action The conclusion drawn from all these experiments is, that æthe does not like alcohol contain water as a constituent*.

Up to this point, then, æther and æthereal solutions resist a voltaic action. I have since, however, found that when rectified æther is saturated with dry muriatic acid gas, and the submitted to moderate galvanic action, hydrogen, retainin some æthereal vapour mixed with it, is liberated at the negative pole, and no gas from the positive, but the liquid acquires a yellow colour from dissolved chlorine.

When dry hydriodic acid gas was conducted into æthe in a little Wolfe's apparatus, the liquid immediately separate into two layers, a lower dense and deep red, and an uppe slightly coloured, which under voltaic agency yielded ga from the negative pole.

In regard to the latter of these experiments, there can b no doubt that in saturating the æther with hydriodic acid ga decomposition took place; and although the nature of thi decomposition was not fully investigated, it seems probabl that the lower liquid consisted principally of iodized hydriodi acid, resulting from the combination of oxygen derived fron the æther with hydrogen of a portion of the hydriodic acid on which view water of course would be formed, and becom the subject of the subsequent voltaic action. During the saturation of æther with muriatic acid, no signs of decomposition were visible; but still it is not impossible that some internal changes may have taken place, and water resulted fron the action united to muriatic acid. Unless such a view b adopted, I should be inclined to hold that there really wa

---

* Edinburgh Transactions, vol. xiii. p. 331.

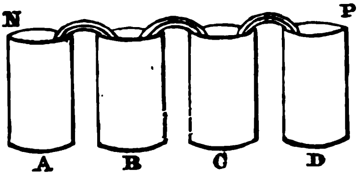
ect decomposition of the muriatic acid by the voltaic cur-
nt; for when I recollect that pure æther resisted very power-
voltaic currents, and that even potash, which has so won-
rful an effect in promoting the galvanic decomposition of
e water in absolute alcohol, did not make æther more sus-
ptible of electric agency, I cannot allow myself to suppose
at the decomposition in the case of an æthereal solution of
hydracid is that of water entering into the constitution
æther, but adhere to the original view, that æther con-
ns no water, and that alcohol consists of æther and of
ter.

IV. *On the state in which the Haloid Salts are dissolved by
ater and Alcohol.*—The question whether haloid salts are
ssolved by water as such, or decompose it and assume the
ite of hydracid salts, is one on which chemists are still di-
led. The action of voltaic electricity on such solutions
pears to me to decide the matter: during such action it is
equently difficult, particularly in the case of iodides, to ob-
rve any acid reaction at the positive pole, when both poles
e plunged directly into the solution, on account of the re-
cing action of oxygen on the acid formed; and even in
ose cases in which acid is observed, that circumstance will
t of itself prove the haloid to be dissolved as a hydracid
lt, because it might be held that acid is formed by secondary
tion at the negative pole, from whence it is drawn to the
sitive. In this way only, on the hypothesis of solution as
haloid, and direct voltaic decomposition of water alone, can
e separation of reduced metal at the negative pole be ac-
unted for. A doubt might also exist whether the acid re-
tion at the pole might not arise from an oxyacid formed by
condary action at the positive. All such objections are,
wever, obviated by placing the poles beyond the solution,
as to get quit of secondary actions; and if in such circum-
ances we can show that the acid and the base go to their
oper poles, and that this acid is a hydracid, we have, I con-
ive, sufficient evidence that the salt has been dissolved as a
dracid salt; for even laying aside for a moment the experi-
ents by which I have endeavoured to show that the haloids,
existing as such in water, are not directly decomposed,
t us take the different views of the nature of the galvanic
tion which suggest themselves when both poles are plunged
to the solution in the ordinary manner, and consider them
the supposition that haloids are dissolved as such. First,
t us suppose that one or other of the two substances, water
haloid, it matters not which, is decomposed, it is evident
at we cannot account for the production of acid where

secondary action is excluded. Next let us suppose that botl substances are decomposed, and that either the element going to the same pole unite on their journey, or by an interchange of elements the oxygen of water unites with th metal of the haloid, and the hydrogen of water with the electro-negative constituent of the haloid. The former of these alternations is contradicted by the fact, that the acid formed i a hydracid; and the latter, although it might account for th formation of acid and alkali, would not account for the liberation of the electro-negative constituent of the haloid a the positive pole, and of hydrogen in fixed and definite proportion at the negative, whatever be the strength of the solution. It appears to me then sufficient, in order to prove th aqueous solution of a haloid as a hydracid salt, to show th separation of the hydracid by voltaic action, under circumstances which exclude secondary action.

To exhibit this result the solution was placed in the tube B fig. 2, (p. 243) and distilled water in A and C, A being mad negative and B positive. When solutions of the chlorides o potassium and of calcium, and of the iodide of potassium wer treated in this way by fifty pairs of two-inch plates, acid and alkali were speedily detected at their proper poles in C and A and on the corresponding sides of the vessel B, and continue to increase whilst the action lasted. The acid collected in C was found in the case of the chlorides to be the muriatic In the case of the iodide the nature of the acid was somewha ambiguous with the above power, but when seventy pairs o four-inch plates were employed it was decidedly the iodic There appeared, however, little doubt that this latter aci had originated in an oxidating action at the positive pole o reduced hydriodic acid; and this view was confirmed by connecting the tube B, containing a solution of iodide of potassium, with two other vessels, C and D, containing distille water on the positive sides, as in fig. 4, and acting with sevent pairs of four-inch plates:—

Fig. 4.



when the acid produced in C was found to be the hydriodic, and that in D the iodic; in other words, the acid separate from B was the hydriodic, but on passing to the poles it wa reduced, and the iodine oxidated; thus both in the case of chlorides and of iodides, the acid separated proved to be the hydracid.

It must, however, always be remembered, that although such production can be readily shown in many cases of haloids, it does not necessarily follow that this would hold in all cases. We are of course best prepared to expect it in the case of haloids, of which the constituents have the strongest affinity for oxygen and hydrogen, such as the ordinary haloids of the bases of the alkalies and alkaline earths; and accordingly, I showed that it applied to chlorides and iodides of potassium and calcium. But further, it was found to hold good in regard to the ordinary haloids of the common metals, such as zinc. I was prepared, however, to consider it as doubtful what might be the result in regard to the noble metals. Accordingly, when a moderately strong solution of chloride of gold was placed in the tube B *, and connected by asbestus with the tubes A and C, which were filled with distilled water, A being made negative, and C positive by a power of fifty pairs of two-inch plates, no decided indications of the formation of acid were obtained during an hour's action; for although towards the end there was a slight acid reaction at the positive pole, it was not greater than distilled water itself might have yielded, and there was a trace of alkali at the negative. Before, however, deciding that chloride of gold in solution does not yield to voltaic action, it would be necessary to repeat the experiment with a more powerful current, because it may possibly only be a case of more difficult electric resolution. In such cases also atomic constitution may have a considerable, if not the principal influence, on the result.

Whenever we have obtained a decided instance of the formation of acid in the above circumstances, we may conclude, with every probability, that all haloids, of the same nature and atomic constitution, of metals, of equal or more powerful affinities, are in the same situation. Thus having verified the rule for chloride of zinc, we may conclude that all protochlorides of more electro-positive metals, such as manganese, cerium, magnesium, barium, potassium, &c., are dissolved as muriates. On the other hand, for the whole series of metals of less powerful affinities, as well as for all haloids of more complex atomic constitution, the matter will still require to be investigated, and I purpose making some further researches on the subject.

---

* Fig. 2. (p. 243.)

In regard to sal-ammoniac, I found that it was resolve
into acid and alkali in the above circumstances, a resu
showing that in solution at least it is simple muriate of am
monia, and cannot justly be regarded as chloride of ammoniu
The same reasoning above applied to the results with com
mon haloids, can be readily extended to the hypothetic
chloride of ammonium; and to complete the evidence on th
point, I found that a solution of muriate of ammonia yielde
the definite quantity of hydrogen from the negative pole.

The experiments with a positive zinc pole lead to the sam
result, at least when taken in conjunction with those showin
that the haloids, if viewed as existing as such in solution, a
not directly decomposed. The oxide of zinc which is di
solved and transferred, must have been taken up by aci
which had been previously drawn to the positive side.

The analogy of the action with a positive zinc pole in a
coholic solutions of haloid salts, as formerly described, lea
by similar reasoning to the view, that in moderately strong s
lutions of that description also, such as that of chloride of l
thium, iodide of potassium, and moderately saturated alc
holic solutions of chloride of calcium, the haloid decompos
the water of the alcohol, and exists in solution as an oxysal
Many phænomena of the voltaic action on such solutions wi
thus receive a more ready explanation than on the idea
these salts being dissolved as haloids; such as the appearan
of alkalies and earths at the negative pole, which will thus r
sult directly from the decomposition of a hydracid, instead
supposing the secondary action of the hydrogen and reactio
of the metal on water.

We cannot easily get the same evidence on this subject b
the method applied to aqueous solutions, of placing the pol
in water beyond the solution, because, from the inferior co
ducting power of the alcoholic solution, less acid will be s
parated if it truly exist in the liquid, and we cannot disti
guish whether it may not come from the point of junction
the alcoholic solution with the water in which the poles a
placed.

If alcohol dissolves haloid salts as hydracid salts, there c
be little doubt that pyroxylic spirit does the same: I incli
to think that the greater solvent powers of the latter fluid th
the former in regard to some substances, such as barytes, a
due to its greater absolute quantity of water, although n
greater atomic proportion.

V. *On the Conducting Powers of Solutions.*—Without goi
the length of holding that the additional conducting powe

stowed on water by dissolved substances is exactly propor- nal to the degree of chemical change under voltaic action re- ting from the dissolved body, there seems in every instance which increased conducting power is bestowed some chemical ange, or at least voltaic transference, attending the increase conduction. This chemical change may result either from e direct action of the current or from secondary agencies; d both circumstances lend their aid, where they occur, in gmenting conducting power.

In the case of salts the voltaic separation of acid and alkali once explains the result, and in many of such cases we have additional effect from secondary action at one or both les.

Acids alone in solution, as is now generally known, and as ave myself verified experimentally for sulphuric acid and e hydracids, undergo transference to their proper pole, ich circumstance appears to be the principal cause of their omoting conduction. In some instances secondary action the poles also contributes to the result.

To ascertain whether alkalies have a similar action by suf- ring transference, a moderately strong solution of caustic tash was placed in a tube B*, connected by asbestus moist- ed with distilled water, with two tubes, A and C, containing e latter fluid, A being made negative, and C positive by venty-two pairs of four-inch plates. The whole tubes ere covered with a close glass covering, a piece of turmeric per having been introduced into the liquids A and C be- een the asbestus and the poles. In a few minutes alkali was dicated at the negative pole, and went on increasing during lf an hour, whilst the test paper in C was not discoloured, owing that the effect in A was not owing to capillary action. he experiment was then stopped, when the water in A, al- ough not alkaline to test paper throughout, became de- ledly so by concentration, whilst that in C showed no alkali en after concentration.

In the experiments also already detailed, in which acid and kali were separately drawn to the poles in distilled water om saline solutions, the alkali usually reached the pole as on as the acid.

There can thus be no doubt, that by voltaic action, the alkali an aqueous solution is transferred to the negative pole.

Water coloured by bromine gives sensibly more efferves- nce under galvanic action, showing a superior conducting wer of the solution.

The manner in which such simple substances increase the

---

* Fig. 2. (p. 243.)

conducting power of water requires a little investigati
Chlorine, bromine, and iodine are generally admitted to
non-conductors themselves; and even if a little doubt m
exist as to iodine in a state of fusion, it is scarcely possible t
the minute quantity in an aqueous solution can operate
that way.

To ascertain whether such substances were capable
transference in solution, an aqueous solution of bromine, w
a little undissolved bromine at the bottom, to maintain a st
of saturation, was placed in the tube B, the arrangement bei
in all other respects the same as in the last-described expe
ment with a solution of potash ; and after an hour's action
seventy-two pairs of four-inch plates no discoloration fr
transference of bromine could be observed in the water eitl
of A or of C; and the latter had only a scarcely perceptil
smell of bromine, which I believe was due to the seconda
decomposition of a trace of hydrobromic acid drawn into
as both the liquids B and C showed some degree of acid
action.

An aqueous solution of iodine was then substituted in
for that of bromine, a little iodine being also left at the botto
and all other circumstances the same, and the battery
charged. After an hour's action there was no appearan
of iodine either in A or C.

From these experiments, it is obvious that neither of the
substances are transferred in solution by voltaic agency. V
must therefore look for some other explanation of the i
creased conducting power, and that which readily occurs is
secondary action at the negative pole, by the union of hydr
gen with the dissolved substance. To determine the accura
of this view, the current from fifty pairs of two-inch plates w
passed at the same time through a solution of bromine a
diluted sulphuric acid, and the hydrogen evolved from t
two negative poles collected, when after half an hour thirte
cubic inches were collected from the sulphuric solution, a
only a bubble, the size of a pea, from the bromine solutio
the difference had evidently combined with bromine.

When an aqueous solution of iodine, which had previous
been purified by sublimation, solution in alcohol and preci
tation by water, was substituted for that of bromine, the a
tion was much more feeble. In a quarter of an hour on
a small bubble of gas was collected from each negative po
and in two hours and a quarter ·1 cubic inch from the su
phuric solution, and ·077 from the iodic.

It is thus evident, that both in the case of bromine a
iodine the action is increased by the combination of the di
solved substance with hydrogen of the decomposed water, b

that, as is to be expected, this circumstance occurs to a much larger extent in the case of bromine than of iodine*.

On connecting the rules regulating the voltaic decomposition of solutions and the transference of substances held dissolved, we observe that no substance, when in a state of transference, suffers direct voltaic decomposition. Acids and alkalies suffer transference, but not direct decomposition. On the other hand, salts, whether oxyacid or hydracid, are not transferred, but are resolved into their constituent acid and alkali.

We cannot, however, say that every substance which is not transferred is directly decomposed. Thus we can hardly doubt that such compounds as bromide of iodine do not suffer voltaic transference, seeing that their constituent elements are not transferred; and we have further seen that this combination is not directly decomposed in solution. Probably also some cases of chlorides exist, in which, from peculiarity of atomic constitution, or other circumstances, there is neither transference nor direct decomposition.

Erratum in the former part of this paper in No. 117: page 247, line 44, *for* non-negative *read* now negative.

LIX. *On the Natural Arrangement of the Consonantal Sounds.* By H. Wedgwood, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

SEEING from your last (February) Number, p. 124, that you do not consider speculation on such a subject foreign to the plan of your publication, I am induced to send you a scheme of all the simple consonantal sounds, exhibiting, in a tabular form, a complete synopsis of their relations with each other.

The first point to be settled is the list of the sounds that are to be the subject of arrangement, which are far from coinciding with those represented by the consonants of the English alphabet. In our mode of writing many simple consonantal sounds are represented by a combination of letters, and conversely, some of our simple consonants are used to represent compound sounds, while others are used on different occasions as the representatives of more than one simple or compound sound.

---

* Long after these experiments were made and conclusions drawn, I observed that M. Becquerel had also found that bromine and iodine in solution unite with hydrogen under voltaic action.—*Institut, Juin*, 1840.

The letter C has in some situations the pronunciation of in others of S, and may therefore be considered superfluou a system of which the object is the arrangement of *sounds* alo The same may be said of Q, which is only used in combi tion with U, to represent the sound KW; and of X, wh is pronounced GS, or KS.

The sound of S in *leisure*, *pleasure*, or of Z in *azure*, i simple articulation, identical with the French pronunciat of the letter J, but having no exact representative in the glish alphabet, in which the sound of J or G soft is a co bination of the last-mentioned sound with a D immediat preceding.

The simple sounds expressed by double letters are th of SH; TH in *thin*, and TH in *then*, and NG in *bring*, *ha*

In addition to the foregoing, the guttural articulati GH, CH, which we do not possess in English, are also sim sounds. Of these, the former is very common in Dutch which it is the pronunciation both of G and CH) Gaelic; the latter in German and Lowland Scotch.

The following, then, will be found to be a complete lis the simple consonantal sounds arranged according to the gan employed in their pronunciation.

Gutturals: K, G, NG, CH, GH, H, pronounced by constriction of the upper part of the throat and base of tongue.

Labials: P, B, M, F, V, W, pronounced by the motion the lips.

Dentals: T, D, N, TH (in *thick*), TH (in *this*), L, p nounced by the pressure of the front of the tongue against teeth or front of the palate.

Palatals: S, Z, R, SH, J (French), Y, pronounced (a appears to me) by the pressure of the sides of the ton against the palate, leaving a passage for the air more or free through the middle.

The first relation of which we shall take notice betw sounds of the same organic class in the foregoing arran ment, is that which distinguishes the classes of Tenues Medials, as they are commonly called; for which, howe a more characteristic appellation would be spirants sonants respectively; inasmuch as the former class are p nounced by a mere expiration not falling within the musi scale; the latter, by a fundamental note of the vocal inst ment, the organic articulation being in other respects same in corresponding consonants of either class.

The consonants commonly arranged under the heads *Tenues* and *Medials*, are the following only :—

Tenues.	Medials.
K	G
P	B
T	D.

t could not, however, altogether escape notice, that the following sounds stood in precisely the same relation to each er :—

F	V
TH (in *thick*)	TH (in *this*)
CH (German)	GH (Gaelic)
S	Z
SH	J (French).

'he complete list of spirants and sonants will then be as ws :—

Spirants.		Sonants.
Gutturals	K CH (Germ.)	G GH (Gaelic), G (Dutch)
Labials...	P F, PH	B V
Dentals...	T TH (in *thick*)	D TH (in *this*)
Palatals...	S SH	Z J (French)

n casting our eye over the foregoing table, we can hardly to remark that the spirants of each organic class consist pair of consonantal sounds, of which the latter is, in every except the first (in which the K is replaced by an equint C), represented by the same letter with the former, the addition of an H subjoined; and there is an obvious lency to the same mode of expression in the sonant mn.

t is evident, then, that in the *spirant* column at least, a stant relation has been felt between the sounds of the ner and latter rank of every organic class, a relation ch may pretty accurately be expressed by calling the nds of the latter rank a thick pronunciation of those of former rank, although it is not easy to describe the menical arrangement of the organs of speech upon which a modification depends. There can be no question that same relation holds good between the consonants of each anic class in the sonant column. It is certain that V bears cisely the same relation to B, or J (French) to Z, that F rs to P, or SH to S.

n the ordinary arrangement of the mutes under the heads Tenues, Medials, and Aspirates, the character of sound designated under the name of *aspirate* is treated as a modif cation analogous to that which gives rise to the classes spirants and sonants, and thus the fact is overlooked that bo spirants and sonants of every organic class have their correl tives among the aspirates or ranks of thick pronunciation.

Tenues.	Medials.	Aspirates.
K	G	CH
P	B	PH
T	D	TH

The remaining consonantal sounds are N, G, H ; M, W N, L ; R, Y, which may be called liquids, from the facility wi which they coalesce with other consonants; or neutrals, fro not admitting of modification by spirant or sonant pronunci tion.

The tabular arrangement of the entire series may then b completed as follows:—

		Spirant or Tenuis.	Sonant or Medial.	Neutral Liquid.
Guttural	Pure or Clear	K	G.	NG
	Aspirate or Thick	CH (Germ.)	GH, G (Dutch)	H
Labial ...	Pure or Clear	P	B.	M
	Aspirate or Thick	F, PH	V, W (Germ.)	W
Dental ..	Pure or Clear	T	D.	N
	Aspirate or Thick	TH (in *thick*)	TH (in *this*)	L
Palatal..	Pure or Clear	S	Z, S (Germ.)	R
	Aspirate or Thick	SH	J (French.)	Y, J (Ger Italian

The characteristic of the sounds of the latter column a pears to me to be, that they are each produced by a st tionary condition of the vocal organs most nearly approachi to that to which the mouth must be reduced immediately b fore the pronunciation of the corresponding sonant or spirant

Hence the peculiar facility with which the sound of NG combined with that of G, M with B, N with D, L with T and R with S; and although we cannot illustrate, in the sa manner, the relation of H with GH, CH; of W with V a F, and of Y with J (French) and SH, yet it seems to me be of the same character, and certainly no one can doubt t intimate connexion of each of the last-mentioned groups sounds.

16, Gower Street. H. Wedgwood.

LX. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. Francis *and* H. Croft.

[Continued from p. 293.]

Benzoënitric and Cinnamonitric Acids.

IN the *Annalen der Pharmacie*, vol. xxx. p. 341, is a paper on Balsam of Peru, by Plantamour, in which a peculiar acid is mentioned, viz. the carbobenzoic; in vol. xxxiv. p. 297, is a treatise by Mulder on the benzoënitric acid; and in the *Journal für Praktischen Chemie*, vol. xxii. p. 193, one by Mitscherlich on cinnamonitric acid. These three papers are so intimately related that it is better not to separate them at all, but to consider them in one report.

When benzoic acid is boiled with excess of nitric acid, it dissolves and colours the fluid red; binoxide of nitrogen is given off. The development of gas ceases after boiling for several hours, and the solution becomes colourless. From the cold solution crystals similar to benzoic acid are deposited, the fluid eventually becomes one solid crystalline mass, from which the benzoënitric acid may be extracted by boiling water (Mulder). According to Mitscherlich, the evolution of binoxide of nitrogen is accidental; it arises from the action of nitric acid on the already formed benzoënitric acid; it is therefore not necessary to boil the fluid for any length of time.

The same acid is produced by the action of nitric acid on several substances which have been said to be converted into benzoic acid by nitric acid. Some substances first form benzoic acid, as is the case with cinnamic acid and oil of cinnamon when treated with dilute acid. Plantamour did not discover the nitrogen in this acid: he gave the formula $C^{15} H^{10} O^{3}$; Mulder and Mitscherlich's formula is $C^{14} H^{8} O^{4} + N^{2} O^{3}$, or $C^{14} H^{10} O^{8} N^{2} - H^{2} O$.

This acid separates from water as a crystalline mass, it is easily soluble in boiling water, and if there is not sufficient to dissolve it all, an oleaginous substance is formed which is heavier than water; benzoic acid has the same property. At 10° C. 400 parts of water dissolve one part of the acid, at 100° ten parts. It is easily soluble in alcohol and æther; melts at 127°, and begins to sublime at 110°; the pure acid sublimes unchanged; chlorine has no action on it. Benzoënitric acid is not easily decomposed by heated sulphuric acid; it becomes red, and then contains an acid which gives a soluble salt with baryta. The benzoënitrates are for the most part soluble in water and alcohol, crystallizable, explode by heating, and when gently warmed give off nitrobenzid. The potash salt crystalliz in acicular crystals, and, by heating gives much nitrobenzic the soda salt is deliquescent (Mulder), easily crystallizab (Mitscherlich). The ammonia salt easily loses ammonia an forms a bisalt. The lime salt is acicular, and contains tw atoms of water, which are driven off at 190°. The baryt salt loses four atoms of water at 100°. The strontia salt los two atoms and a half at 130°. When acid nitrobenzoate (ammonia is dropped into a solution of sulphate of zinc, a g latinous basic salt is formed, the filtered solution gives lame lular crystals, $C^{14} H^{6} N^{2} O^{7} + Zn O + 5$ Aq. The basic sa contains four atoms of base to one of acid. The manganes salt crystallizes with one atom of water. The copper salt a blue powder soluble in hot water; contains one atom (water. There are two salts of lead, a simple benzoënitra and a benzoënitrate, $5 (C^{14} H^{8} O^{7} N^{2} + Pb O) + Pb O$. Th silver salt is partly soluble in water, gives much nitrobenzi when carefully heated, &c. &c. (Mulder).

The cinnamonitric acid is formed by adding pulverized ci namic acid to strong nitric acid; the temperature must not ri above 60°. The cinnamic acid is at first dissolved, the mi ture soon becomes hot, and a crystalline substance separate it is washed with water, dissolved in and crystallized from a cohol. It is white with a trace of yellow, melts at 270° heated above 270° it boils and is decomposed. Almost ins luble in boiling water. At 20° is soluble in 327 parts of a cohol, and by this property may be easily separated from oth acids. Cinnamic acid is soluble in 4·2 parts, benzoic in 1·9 and benzoënitric in somewhat less than its own weight.

Cinnamonitric acid, when boiled with a small quantity (water, does *not* form the oily body mentioned above; somewhat soluble in boiling hydrochloric acid without de composition. Its salts with the alkalies are easily soluble; th other salts are either difficultly soluble or insoluble. Th potash and soda salts form verrucous crystals; when an exce of potash is added to the potash salt, prismatic crystals ar obtained. The ammonia salt decomposes like the benzoat Of all the others the magnesia salt is the most soluble. Thes salts explode.

The æther is formed by heating cinnamonitric acid wit twenty parts of alcohol and a little sulphuric acid for sever hours, at a temperature not above 80°. The acid is dissolve and the fluid when cooled deposits the æther in prismati crystals; the æther is not decomposed by ammonia. I melts at 136°, and boils at 300°, at which temperature it i partly decomposed.

Several acids of similar composition give crystallizable ers; the most beautiful is that of benzoënitric acid H^{10} O, C^{14} H^{8} N^{2} O^{7}). The following crystallize well: innitrate and anisinnitrate of æthyloxide, and the benzoëate of methyloxide.

Cinnamic acid may be very easily distinguished from ben- acid by the formation of the cinnamonitric acid. The for- a is $C^{18} H^{12} N^{2} O^{7}$, or $C^{18} H^{14} O^{3} + N^{2} O^{5} - H^{2} O$. The ombined acid is $C^{18} H^{14} N^{2} O^{9}$.

f more than one part of cinnamic acid is added to eight ts of nitric acid, the temperature rises above 60°, and ben- itric and another, as yet unexamined, acid are formed. crystallized acid (benzoënitric) contains one atom of er, according to Mulder; he calls it "acide nitroben- e:" benzoënitric is more applicable.

Cinnamic acid does not combine with sulphuric acid as zoic acid does. By distilling cinnamic acid with lime, onate of lime and carbon remain behind, and the product mixture of several substances; it smells like benzin, but uid under 0°; it perhaps contains some benzin, but is ainly a mixture. The same is the case with the products ined by passing camphor and oil of cinnamon through a hot tube, according to the experiments of H. C. (D'Ar- s fluid, which is said to be like benzin, but to boil at 140°, uld not obtain; the oil procured began to distil at 140°, the boiling point rose to 250°.—H. C.)

Ferric Acid.

M. Fremy has found that if a mixture of the per- le of iron and potash, or better one of peroxide of iron nitre and potash, or also peroxide of potassium be kept a time at a lively red heat, a brown mass is obtained, ch, when treated with water, affords a very beautiful violet tion. The combination contained in this solution can wise be formed in the moist way, by passing chlorine into ry concentrated solution of potash in which the hydrate he peroxide of iron is suspended. The combination pre- ed in either way is of a beautiful violet, very soluble in pure er, insoluble in alkaline water, in which it forms a brown cipitate, which redissolves in pure water with a purple co- . It appears to be less stable than the manganate of ash. Under certain circumstances (for instance, with time nuch water), it decomposes at the common temperature peroxide of iron, which falls to the ground, and into oxy- , which escapes, while all colour disappears. A tempera- of 100° produces the same decomposition instantaneously.

It is decomposed by all organic substances, from which c it is impossible to filter the solution. Hitherto all atte to isolate the combination have proved unsuccessful. W the red solution is treated with an acid and the potash ne saturated, peroxide of iron is thrown down and oxygen engaged.—*Comptes Rendus*, vol. xii. p. 23.

On the series of Bodies derived from the fuming liquor Cadet (*Alkarsin*).

In a paper entitled "Researches on the Cacodyl Ser *Annalen der Chemie und Pharmacie*, xxxvii. p. 1, Dr. Bu has described the products of the decomposition of alkar

The primary member of this series has the for $C^4 H^{12} As^2$, and therefore represents common alcohol which two atoms of oxygen are replaced by two of senic. This body may be taken as an excellent re sentative of the organic radicals. The arsenic in these c pounds cannot be completely oxidized by nitric acid; m over, it cannot be determined by means of the usual oxidi substances, or of oxide of copper, because explosions quently occur, and the separation of arsenic from copp difficult. The best substance is oxide of nickel, which be prepared by heating the sulphate to a white heat burning the compound with this oxide the arsenic is oxidized, but not so the carbon and hydrogen. The tub burning is connected with a Liebig's condenser, contai water; if the analysis has not been well conducted this w acquires a smell of alkarsin. The contents of the tub quite soluble in aqua regia; the solution is treated with phuric acid to drive off the nitric acid, evaporated to dry moistened and filtered: in this solution the arsenic ma determined in the usual method. The carbon and hydr may be determined by burning the compound with oxi copper or chromate of lead; if oxide of copper be us rather long tube must be employed, the anterior part of w is filled with chromate of lead or copper turnings. In paring these compounds all the vessels must be filled carbonic acid.

Oxide of cacodyl, alkarsin (vide *Annalen* xxiv.; also mas, *Annalen*, xxvii.). Formula $C^4 H^{12} As^2, O$. Specific vity of the vapour, found by experiment, 7·555, calculated, 7·

This body reacts perfectly neutral, but combines with a and in this respect is similar to some inorganic bodies, w are half base half acid. Phosphoric acid dissolves a l quantity of it, and forms a stinking thick fluid which doe crystallize. The alkarsin may be distilled over from

ıpound unchanged. Dilute cold nitric acid forms with
ırsin a thick fluid. Alkarsin is dissolved by digestion
concentrated hydrous sulphuric acid; the liquid on cool-
forms a mass of small white acicular crystals: they may
freed from the matrix by pressure between paper, deli-
sce in the air and react acid. Nitrate of cacodyloxide
s peculiar precipitates in metallic solutions, but their na-
is not as yet determined. With the hydracids alkarsin
ns water and a haloid salt. It extracts the oxygen from
oxides of mercury, silver, gold, &c.: even arsenic acid
indigo are reduced by it. Alkarsin may be used as a
for the presence of arsenic. If the substance obtained
Marsh's apparatus be boiled with water until dissolved,
a little potash and acetic acid added, and this solution
oorated to dryness, the mass when heated in a glass tube
s the horrible smell of alkarsin, if any arsenic be present:
mony has no such effect. If a drop of protochloride of
oe added to the heated substance, the characteristic smell
nloride of cacodyl is developed.

ulphuret of cacodyl, Cd S, is obtained by distilling chlo-
of cacodyl with hydrosulphuret of barium, Ba S, $\underline{H}$ S; hy-
ulphuric acid (sulphuretted hydrogen) is given off. The
llation with the hydrosulphuret must be repeated. The
r and excess of $\underline{H}$S may be removed by chloride of cal-
n and carbonate of lead; the fluid must then be carefully
ected from the air. The acid fluid which accompanies the
et's fluid gives a considerable quantity of this protosul-
ret when treated with the above-mentioned barium salt.
aqueous solution of cacodylic acid (alkargen) when
ted with hydrosulphuric acid also gives it. In an alcoholic
tion the higher sulphuret is produced.

d S is a clear æthereal liquid which does not fume in the
of a disagreeable penetrating odour, which reminds one
lkarsin and mercaptan. Does not become solid at
0° C.: distils over with water, although its boiling point
nuch higher than 100°. Easily inflammable in the air;
sulphur is perfectly oxidized by strong nitric acid. It is
ly insoluble in water, but communicates to it a strong
ll; may be mixed with æther and alcohol in all propor-
s; combines with sulphur into a crystalline sulphuret.
nium also forms a similar compound, which crystal-
out of æther in colourless laminæ. Iodine forms a cry-
ine substance. Oxygen converts it into alkargen, and an,
et, unexamined crystallized matter. Hydrochloric acid
ns Cd $\underline{Cl}$ + $\underline{H}$S. Sulphuric acid gives Cd O, $\dddot{S}$ + $\underline{H}$S,

&c. &c. Acetic acid does not decompose it. Formula C or $C^4 H^{12} As^2$, S. Specific gravity of vapour 7·72, calcul 8·39.

Protoseleniuret of cacodyl, $C^4 H^{12} As^2$, Se, or Cd Se: chloride of cacodyl is distilled several times with a s tion of seleniuret of sodium: it is a transparent yello fluid of very unpleasant smell; insoluble in water, solub æther and alcohol; absorbs oxygen and deposits colou crystals: burns in the air with a blue flame. Nitrate of s gives with Cd Se, Ag Se + Cd O, $\dddot{\underline{N}}$. Corrosive subli gives first black seleniuret of mercury, and then a white cipitate, viz. hydrargochloride of cacodyloxide. This c pound is soluble in hot water, and crystallizes from i cooling.

Protocyanide of cacodyl, Cd $\underline{Cy}$, is formed by disti concentrated hydrocyanic acid with alkarsin, or bette treating a strong solution of bicyanide of mercury with a sin. By distillation an oily fluid collects under the w and on cooling forms large beautiful prismatic crystals; t crystals are then distilled over baryta, &c. &c. Melts at 32 boils at about 140° C.; burns in the air with a reddish- flame. Soluble in æther and alcohol, but little in water. is the most poisonous of all the cacodyl compounds, and must be excessively careful in experimenting with it. A lution of silver gives cyanide of silver with Cd $\underline{Cy}$. Th trate of the dioxide of mercury is reduced by it, not s nitrate of the oxide. Sublimate gives the hydrargochl of cacodyloxide, &c. &c. Formula $C^4 H^{12} As^2, N^2 C^2$; cific gravity of the vapour 4·63, calculated, 4·547. The cific gravity of the vapour of cacodyl, $C^4 H^{12} As^2$, therefore be 7·281. We must reserve the conclusion of paper for our next.

LXI. *On the Isomorphism of Oxamethane and Chloroxamet* *By* M. F. de la Provostaye, *Professor to the Facul* *Rennes*[*].

AMONGST the numerous and important researche which the theory of substitutions has already given those of M. Malaguti upon chloroxalic æther[†] are wit doubt some of the most remarkable. We here see two plete series of products, all derived from each other wit destruction, and by simple combinations or transformati eries which constantly present, on one side hydrogen, and on he other chlorine which takes its place atom by atom. It ; certainly impossible to imagine a chemical isomorphism etter established and more perfectly characterized; neverieless I know not whether up to the present time it has been ossible to prove crystallographical isomorphism either for iese products, or for the products obtained by similar sub:itutions*. Most frequently indeed the two substances, or at east one of them, does not crystallize, and is altogether incapable f exact measurement. To any one remembering the beautiful esearches of M. Mitscherlich and the very important conequences which flow from them, it was, however, of the ighest interest to arrive at an accurate solution upon this oint. M. Malaguti's kindness has enabled me to give it. 'his chemist sent me some crystals of oxamethane and of iloroxamethane. The former had been obtained by the ooling of an æthereal alcoholic solution, the latter had been ormed in a solution simply æthereal. They were perfectly eautiful, and capable of being measured. Now the result of ieir examination is, that these two substances are isomorious. I here give the determinations on which this asserm is founded.

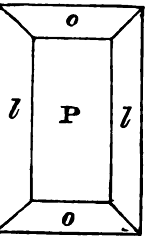
---

[*] From the *Annales de Chimie et de Physique*, vol. lxv. p. 322.

[†] An Abstract of this paper will appear in our next Number.—E

The chloroxamethane belongs to the rhombic (right rectanılar prismatic) system; it crystallizes in the form of a prism ith a rectangular base *l*, the angles of which are of 60° and 20°.

At each summit are two faces *o* forming the bevil, which intersect each other at an angle of 108° 54′ very nearly. We have then



Measured angles.

P : *l* = 120°

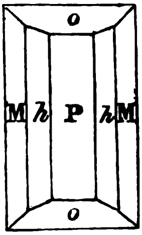
l : *l* on the other side = 120°

P : *o* = 125° 30′—35′.

Oxamethane crystallizes in excessively thin flexible laminæ, the measurement of which is rather difficult although the face are brilliant. The summits are almost alway altered; they are, however, found capable c measurement on very small crystals.

---

* M. Laurent has preferred a claim in the *Comptes Rendus de l'Institut,* the subject of this note. In a memoir presented to the Academy some ›nths ago, but which has not yet been published, he announced *that tain naphthalic combinations, in which chlorine takes the place of hydrogen, e isomorphous.* I am far from wishing to dispute the merit of these njectures; but as a fact of this nature can only be established by *meares,* which he had not taken, I still think I may say that oxamethane and loroxamethane present the first, and until now, the only example of isorphism, really proved amongst these two classes of substances.



Measured angles.

P : *o* = 125° 25′—35′
P : *h* = 151° 30′—50′
P : M = 132° 30′—45′

The result of these measurements is that oxamethane als belongs to the rhombic system. The angle of the two face which form the bevil at the summit is the same as the corre sponding angle of the crystals of chloroxamethane. As to th vertical planes, the angles are different; but it is easy to se that the two forms may be derived from the same fundament form. In fact, the calculated axes, by taking M for the pr mitive prism, are

Vertical axis	$a = 0{\cdot}715$
Horizontal axis turned towards the observer	$b = 1$
Horizontal axis directed from left to right	$c = 0{\cdot}924$.

By setting out from these axes, we have, for the notation c the faces and for the calculated value of the angles,

Notation.

Chloroxamethane.	Oxamethane.
$o = \overset{\vee}{P}\,\infty$,	$o = \overset{\vee}{P}\,\infty$,
$P = \infty\,\overset{\vee}{P}\,\infty$,	$P = \infty\,\overset{\vee}{P}\,\infty$,
$l = \infty\,\overset{\vee}{P}\,\frac{5}{8}$.	
	$M = \infty\,P$,
	$h = \infty\,\overset{\vee}{P}\,2$.

Calculated angles.

P : *l*	= 120°	P : M	= 132° 43′
l : *l* on the other side	= 120°	P : *h*	= 151° 34′
P : *o*	= 125° 33′	P : *o*	= 125° 33′

The agreement between the observed and the calculat values is such, that it is impossible that the isomorphism ca be doubtful.*

XII. *On the Composition of Chalk Rocks and Chalk Marl by invisible Organic Bodies: from the Observations of Dr. Ehrenberg*[*]. *By* THOMAS WEAVER, *Esq., F.R.S., F.G.S., M.R.I.A., &c. &c.*[†]

THE remarkable discoveries effected, and the new light
. thrown on geology by the indefatigable researches of Dr.
irenberg, during several years past, through the medium of
e microscope, particularly in respect of the Infusoria and
lythalamia tribes, highly instructive and interesting as they
ist be to all naturalists, are especially so to the geologist,
ice they open to him a large field of inquiry, eminently de-
rving of cultivation. To draw attention to this subject, which
volves no less than an investigation as to what extent minute
ganic bodies, invisible to the naked eye, may have contri-
ted to the production of all limestone formations, whether
an origin posterior or anterior to the epoch of the chalk,
scending thus in the series to the primary limestones, it ap-
ared to me that a sketch taken from a portion of the labours
Dr. Ehrenberg might be not only useful, but especially ac-
ptable to such geologists as may not be conversant with the
nguage of the original. I propose then, in the first instance,
advert briefly to the earlier researches of Dr. Ehrenberg
ncerning the Coral tribes in general, and those of the Red
a in particular[‡]; and in the second, to present such extracts
om the Memoir, the title of which stands at the head of this
per[§], as may answer the purpose of a general view.

At the instigation of the Royal Academy of Sciences of Ber-
[||], Dr. Ehrenberg and his friend, the late Dr. Hemprich,

---

[*] Communicated by the Author.

[†] With an Appendix touching the researches of M. Alcide d'Orbigny.

[‡] See in the Abhand. der König. Acad. d. Wissenschaften zu Berlin for
e year 1832:—

1. Contributions to the physiological knowledge of the Coral animals in
neral, and in particular of those of the Red Sea, with an attempt to clas-
them according to their physiological distinctions; read 3rd March,
31, with additions printed 1st Dec. 1833, pp. 225–380.

2. On the Nature and Structure of the Coral Banks of the Red Sea, read
nd March 1832; revised and printed in Feb. 1834, pp. 381–432.

[§] *Ueber die Bildung der Kreidefelsen und des Kreidemergels durch un-
htbare Organismen*, in the Transactions of the Royal Academy of Sciences
Berlin, for the year 1838, read 20th Dec. 1838, and 18th Feb. 1839,
. 59–149.

[||] See the Report read to the Academy by M. Alr. von Humboldt on the
avels of Doctors Ehrenberg and Hemprich through Egypt, Dongola, Syria,
abia, and the Eastern declivity of the highlands of Abyssinia, in the years
20–1825, conveying a clear idea of the arduous and extraordinary la-
urs of those gentlemen in all branches of Natural History: Berlin, 1826.
. Hemprich fell a sacrifice to his exertions in Abyssinia, on the 30th of
ne, 1825.

visited the Red Sea during a period of eighteen months, na
ly, nine months from the year 1823 to 1824, and an e
number in 1825, having been nearly twelve months of the
on board ship, in which interval they passed over nearly
whole extent of that sea, saw many of its islands and c
banks, and landed with a view to special examination on fo
eight different points of the two coasts; but the whole num
of islands and special points of the coast seen by them amo
to about 150, independently of the long coast of Sinai in A
bia, which they examined in continuity. In these labor
efforts, attended with extreme danger, they collected 110
cies of Coral animals, being nearly three times as many as
been found or described by all former observers, namely
Shaw, Forskål and Savigny, and later by Rüppel.

To determine the subjects of that collection with the gre
precision, it became necessary to undertake a review of
whole class of the Coral animals, and the more so as
Ehrenberg found that his own observations were freque
in collision with the systematic distinctions that have prev
up to the present time. In this review the author has e
cially compared the four most recent extensive systems, na
ly, of Schweigger in 1820, Rapp in 1829, Cuvier in 1830,
Blainville likewise in 1830, which may be said to embody
judgment of the present generation upon the labours of ea
periods, and to comprise the sum of existing knowledg
this department of natural history. He has in partic
turned his attention to the work of Blainville *, since it
tains the greatest number of new details, having been enri
by the latest manuscript observations and drawings of Q
and Gaimard, the result of their second voyage round
world with Capt. D'Urville. In these newer works, the
bours of Lamarck having been critically employed, the au
was relieved from the necessity of noticing them in a sp
manner.

The attempt to reconcile the observed discrepancies le
author to separate the Coral animals into two organic nat
groups, which are well marked and distinct from each o
and which he named *Anthozoa* (Flower-animals) and *Bry*
(Moss-animals). In the course of these researches the au
found that the whole group of the Anthozoa, which consi
the proper (single-mouthed) coral animals, and which
been gradually distributed under more than 158 generic na
including even heterogeneous animals and plants, might,
cording to his own observations of their correspondenc

---

* *Dictionnaire des Sciences Naturelles*, 1830.

affinity and relations of structure and development, be reduced to eighty-six genera, but which number might perhaps be still further diminished, as a few genera might be classed as subgenera. The Anthozoa he has divided into two orders, *Zoocorallia* (Animal-corals) and *Phytocorallia* (Plant-corals). In the Memoir is given a systematic description of the Orders, Tribes, Families, Genera and Species of the Anthozoa, while further details are reserved for the author's work, entitled, *Symbolæ Physicæ*. The subjoined Table will show the general arrangement, extending to the genera.

ANTHOZOA.

Ore ventriculoque distinctis, tubo cibario anoque discreto nullis, corpore intus radiatim lamelloso. (Vibratio nulla, gemmæ et spontanea divisio frequentissimæ.)

ORDO I.—ZOOCORALLIA.

Corpore aut omnino molli, aut Cephalopodum more intus lapidem generante (secernente nec excernente) hinc sæpe omnino libera et, præter formam, animalium characteres omnes perfectius servantia.

			Species.				
			Of the Red Sea.				
			Unproved.	Observed by himself.	Total living.	Fossil Genera.	Genera.
TRIBUS I.	Zoocorallia Polyactinia.	Familia I. ACTININA. Genera 9. Species living . 50. In the Red Sea 23.	...	16	29	...	1. Actinia.
			...	1	1	...	2. Metridium.
			...	1	1	...	3. Megalactis.
			1 ?	...	1 ?	...	4. Thalassianthus.
			...	1	10	...	5. Cribrina.
			...	1	3	...	6. Actinodendron.
			...	1	1	...	7. Epicladia.
			...	1	1	...	8. Heterodactyla.
			...	...	3	...	9. Lucernaria.
		Fam. II. ZOANTHINA. Genera 4. Fossil 2. Species living . 12. In the Red Sea 7.	...	2	3	...	10. Hughea.
			1	...	2	...	11. Zoanthus.
			1	1	4	...	12. Mammillifera.
			...	2	3	...	13. Palythoa.
			...	...	...	F.	Siphonia.
			...	...	...	F.	Lymnorea.
		Fam. III. FUNGINA. Genera 6. Fossil 5. Species living . 12. In the Red Sea 3.	...	1	3	F.	14. Fungia.
			...	1	5	...	15. Haliglossa.
			...	...	3	...	16. Polyphyllia.
			...	...	...	F.	17. Cyclolithas.
			1 ?	...	1 ?	F.	18. Turbinalia.
			...	...	...	F.	19. Trochopsis.
			...	...	...	F.	Diploctenium.

			Species.				
			Of the Red Sea.				
			Unproved.	Observed by himself.	Total living.	Fossil Genera.	Genera.
TRIBUS II.	Zoocorallia Octactinia.	Fam. IV. XENINA. Genera 3. Species living . 7. In the Red Sea 6.	...	3	3	...	20. Xenia.
			...	3	3	...	21. Anthelia.
			...	...	1	...	22. Rhizoxenia.
		Fam. V. TUBIPORINA. Genus 1. Species living . 3. In the Red Sea 1.	...	1	3	...	23. Tubipora.
		Fam. VI. HALCYONINA. Genera 6. Species living 28. In the Red Sea 13.	...	1	1	...	24. Halcyonium.
			...	5	12	...	25. Lobularia.
			...	2	2	...	26. Ammothea.
			...	2	4	...	27. Nephthya.
			...	3	8	...	28. Sympodium.
			...	...	1	...	29. Cliona.
		Fam. VII. PENNATULINA. (α) HALISCEPTRA. Genera 4. Species living . 7. In the Red Sea 0.	...	...	4	...	30. Veretillum.
			...	...	1	...	31. Pavonaria.
			...	...	1	...	32. Umbellularia.
			...	...	1	...	33. Scirpearia.
		(β) HALIPTERIA. Genera 3. Fossil 1. Species living . 10. In the Red Sea 0.	...	...	2	...	34. Renilla.
			...	...	3	F.?	35. Virgularia.
			...	...	5	...	36. Pennatula.
TRIBUS III.	Zoocorallia Oligactinia.	Fam. VIII. HYDRINA. Genera 2. Species living . 6. In the Red Sea 0.	...	...	4	...	37. Hydra.
			...	...	2	...	38. Coryna.
		Fam. IX. TUBULARINA. Genera 4. Species living . 12. In the Red Sea 0.	...	...	4	...	39. Syncoryna.
			...	...	3	...	40. Tubularia.
			...	...	4	...	41. Eudendrium.
			...	...	1	...	42. Pennaria.
		Fam. X. SERTULARINA. Genus 1. Species living . 4. In the Red Sea 1.	1 ?	...	4	...	43. Sertularia.
		ZOOCORALLIA...	5	49	151	8	

ORDO II.—PHYTOCORALLIA.

pore aut lapideam aut corneam materiam adglutinantem secernente, c dorso (solea) excernente ejusque ope semper adnato (Ostrearum more).

		Species.				
		Of the Red Sea.				
		Unproved.	Observed by himself.	Total living.	Fossil Genera.	Genera.
Polyactinia.	Familia XI. OCELLINA. Genera 14. Fossil 12. Species living 41. In the Red Sea 7.	...	...	2	...	44. Desmophyllum.
		...	...	3	...	45. Cyathina.
		...	1	1	F.	46. Stephanocora.
		...	...	3	F.	47. Monomyces.
		...	1	9	F.	48. Oculina.
		...	...	4	F.	49. Turbinaria.
		...	3	8	F.	50. Explanaria.
		...	...	6	F.	51. Cladocora.
		...	...	...	F.	52. Columnaria.
		...	1	1	F.	53. Strombodes.
		...	...	...	F.	54. Cyathophyllum.
		...	...	...	F.	55. Pterorrhiza.
		...	1	4	F.	56. Anthophyllum.
		...	...	...	F.	57. Stylina.
	Fam. XII. DÆDALINA. (α) ASTRÆINA. Genera 4. Fossil 3. Species living 28. In the Red Sea 15.	1	2	7	...	58. Caryophyllia.
		...	4	7	F.	59. Favia.
		...	8	14	F.	60. Astræa.
		...	...	...	F.	61. Favosites.
	(β) MÆANDRINA. Genera 7. Fossil 6. Species living 28. In the Red Sea 5.	...	2	7	F.	62. Mæandrina.
		...	2	12	F.	63. Manicina.
		...	...	1	...	64. Merulina.
		...	1	4	F.	65. Pavonia.
		...	...	2	F.	66. Agaricia.
		...	...	2	F.	67. Polyastra.
		...	...	...	F.	68. Monticularia.
Dodecactinia.	Fam. XIII. MADREPORINA. Genera 3. Fossil 3. Species living 41. In the Red Sea 24.	...	7	21	...	69. Heteropora.
		...	17	20	F.	70. Madrepora.
		...	...	...	F.	71. Catenipora.
		...	...	...	...	Pleurodictyum.
	Fam. XIV. MILLEPORINA. Genera 4. Fossil 2. Species living 23. In the Red Sea 11.	...	...	...	F.	72. Calamopora.
		...	4	6	...	73. Seriatopora.
		1	2	7	F.	74. Millepora.
		...	3	10	...	75. Pocillopora.
Octactinia.	Fam. XV. ISIDEA. Genera 4. Species living 10. In the Red Sea 1.	...	...	1	...	76. Corallium.
		...	...	4	...	77. Melitæa.
		...	1	3	...	78. Mopsea.
		...	...	2	...	79. Isis.
	Fam. XVI. GORGONINA. Genera 6. Fossil 1. Species living 63. In the Red Sea 3.	1?	...	3	...	80. Prymnoa.
		1?	...	10	...	81. Muricea.
		...	...	23	...	82. Eunicea.
		1?	...	4	...	83. Plexaura.
		...	...	12	F.?	84. Gorgonia.
		...	...	11	...	85. Pterogorgia.
Oligactinia.	Fam. XVII. ALLOPORINA. Genus 1. Species living 1. In the Red Sea 0.	...	...	1	...	86. Allopora.
	PHYTOCORALLIA	5	61	235	27	
	ZOOCORALLIA	5	49	151	8	
	ANTHOZOA	10	110	386	35	
	Of the Red Sea	120				

In the preceding Table we see that of the forty-three gene of Zoocorallia, there are eight which are found fossil; t living species amount to 151, of which fifty-four exist in t Red Sea, and forty-nine of these have been observed by t author, five remaining unproved. Of the forty-three gene of Phytocorallia there are twenty-seven which are found fo sil; the living species amount to 235, of which sixty-six exi in the Red Sea, and sixty-one of these have been observed the author, five remaining unproved. The general result i that out of eighty-six genera of Anthozoa, thirty-five occur the fossil state; and that of 386 known living species of A thozoa, 120 exist in the Red Sea, of which 110 species we observed by the author. The same Table also shows that the seventeen families of known Coral animals, thirteen exi in the Red Sea, while four are wholly wanting, namely, tho of Pennatulina, Hydrina, Tubularina and Alloporina. T total number of known living species comprised in each fami is also given, as well as the relative number actually existi in the Red Sea.

The 120 species of Anthozoa existing in the Red Sea th constitute nearly one third of the whole number of living sp cies, and being comprised in forty-four genera, the latter rath exceed one half of the number of known living genera.

Of the known living Corals there are eight genera peculi to the Red Sea, namely, *Megalactis*, *Thalassianthus*?, *Ep* *cladia*, *Heterodactyla*, *Anthelia*, *Ammothea*, *Stephanocora* an *Strombodes*. It appears also that eighty-eight species are pec liar to it, not having been hitherto observed anywhere else.

Among the genera of the Red Sea that of *Strombodes* excit peculiar interest, having previously been found only in the fo sil state. It affords a key to the structure of the remarkab Cyathophylla, differing from the view hitherto entertaine and rendering it quite clear that the internal central star the encased forms is not a young one, but the oldest or mo ther star, which is often surrounded by broad radiated mantl folds productive of buds.

It appears probable that the Red Sea and the part of th Mediterranean so nearly adjoining on the Libyan coast, pos sess only two forms out of the 120 species in common, namel *Actinia Tapetum* and *A. Mesembryanthemum*.

Of the Bryozoa group, Dr. Ehrenberg gave in the sam memoir, contained in the volume of the Transactions for 1832 only the more general results of his investigations, without en tering into detail; but the subject is resumed in his later me moir, inserted in the volume for 1838, in which he has pre sented a tabular view of the Bryozoa, distributed into Orders amilies and Genera, with their characteristics. According to is view the Bryozoa comprise four Orders, *Polythalamia*, *ymnocoræ*, *Thallopodia* and *Scleropodia*; the Polythalamia eing divided into *Monosomatia* (single-bodied), consisting of fty-six genera, and *Polysomatia* (many-bodied or polyparian), omposed of twenty-two genera, forming altogether seventy-ght genera of Polythalamia. The following Table exhibits e general arrangement.

BRYOZOA.

Animalia asphycta, tubo cibario simplici, sacciformi aut tubuliformi, vera corporis articulatione nulla aut sensim numerosiore, corporis forma gemmis aut novis articulis accedentibus sensim aucta, hinc indefinita, nunquam sponte dividua, omnia et singula verisimiliter periodice ovipara, ideoque hermaphrodita.

ORDO I.—POLYTHALAMIA.

Libere vagantia et loricata.

onosomatia.

Familia I. MILIOLINA.
Genera 2. ? Miliola, ? Gromia.

Familia II. NODOSARINA.
Gen. 11. Glandulina, Mucronina, Nodosaria, Orthocerina, Dentalina, Lingulina, Frondicularia, Rimulina, Vaginulina, Planularia, Marginulina.

Familia III. TEXTULARINA.
Gen. 6. Bigenerina, ? Dimorphina, Textularia, Grammostomum (*Vulvularia*), Polymorphina, Virgulina.

Familia IV. UVELLINA.
Gen. 11. Guttulina (et *Globulina*), Uvigerina, Bulimina, Valvulina, Rosalina, Clavulina, Globigerina, Pyrulina, Sphæroidina.

Familia V. ROTALINA.
Gen. 22. Operculina, Soldania, Planorbulina, Rotalia, Trochulina, ? Spirulina, Calcarina, Pleurotrema, Planulina, Discorbis, Omphalophacus, ? Gyroidina, Truncatulina, Lenticulina, Nonionina, Cristellaria, Siderolina, Dendritina, Robulina, Anomalina, Saracenaria, Cassidulina.

Familia VI. PLICATILIA.
Gen. 6. Biloculina, Spiroloculina, Triloculina, Articulina, Quinqueloculina, Adelosina.

olysomatia.

Familia VII. ASTERODISCINA.
Gen. 5. Asterodiscus, Lunulites, Orbitulites, Cupularia, Flustrella.

Familia VIII. SORITINA.
Gen. 2. Sorites, Amphisorus.

Familia IX. FRUMENTARINA.
Gen. 3. ? Dactylopora, ? Ovulites, ? Polytripe.

Familia X. HELICOSORINA.
Gen. 5. Peneroplis, Pavonina, Vertebralina, Orbiculina, ? Heterostegina.

Familia XI. HELICOTROCHINA.
Gen. 3. Polystomella, ? Amphistegina, ? Geopo
Familia XII. ALVEOLINEA.
Gen. 2. Melonia, Alveolina.
Familia XIII. FABULARINA.
Gen. 2. Fabularia, Coscinospira.

ORDO II.—GYMNOCORÆ.

Libere vagantes, nudæ.

Familia I. CRISTATELLINA.
Gen. 2. Cristatella, Zoobotryon.

ORDO III.—THALLOPODIA.

Stolonibus thallove membranaceo affixa, incrustantia nec adnata, sed loricata.

Familia I. HALCYONELLEA.
Gen. 8. Halcyonella, Vesicularia, Bowerbankia,
rella (= *Lagenella*) *, Valkeria, Step
dium, n. G., Dynamene, Halodactylu
Alcyonidium).
Familia II. CORNULARINA.
Gen. 1. ? Cornularia.
Familia III. ESCHARINA.
Gen. 5. Eschara, Melicertina (= *Melicerit*
Crisia, Acamarchis, Notamia.
Familia IV. CELLEPORINA.
Gen. 5. Cellepora, Flustra, Membranipora, B
phus, n. G., Apsendesia.
Familia V. AULOPORINA.
Gen. 1. Tubulipora.

ORDO IV.—SCLEROPODIA.

Stolonibus destituta, excreto fulcro axique anorganicis firmiter affixa eisque fruticulosa.

Familia I. MYRIOPORINA.
Gen. 9. Hornera, Idmonea, Retipora, Disticho
Myriopora, Tilesia, Cricopora, Cerio
Spiropora.
Familia II. ? ANTIPATHINA.
Gen. 1. Antipathes.

"The two last orders, the Thallopodia and Scleropo
the author observes, "are considerably richer in forms,
it would be very easy by an uncritical compilation to enl
greatly the number of names; but such confusion has
produced in names by Lamouroux and later writers, the s
body being often designated by many new names, that I
not venture to extend my judgment further at present. V

---

* The name *Lagenella* was appropriated to an infusorial form in
† *Melicerta* is already employed among the Radiaria, *Melicertum*
the Acalepha, *Melicerita* is not correct in language.
Perhaps hereafter it may be advisable to substitute *Textilaria* for
tularia, *Polystomatium* for Polystomella, *Cyclodiscus* for Discorbis, &c

has been advanced will suffice to show clearly the position of the Polythalamia, such as it appears to me, in the animal kingdom."

On Chalk and Chalk Marl.

The memoir on the chalk and chalk marl is distributed under the following heads:—

1. Historical Introduction, pp. 59—68.
2. New method of observing, pp. 68—70.
3. On calcareous-shelled organisms, invisible to the naked eye, as the principal constituents of writing chalk, pp. 70—74.
4. On Chalk Marl and its relations to Chalk, and to the Flints of the Chalk, pp. 74—87.
5. On the compact limestone of Upper Egypt and Arabia, as formed by the Polythalamian calcareous animalcules of the White Chalk of Europe, pp. 87—90.
6. On the principal organic calcareous forms which constitute all chalk, and the local differences, pp. 90—95.
7. Preliminary view of new researches respecting living Polythalamia, and their relation to the formation of the sand of Sea Downs, pp. 96—106.
8. Application of the preceding observations to the systematic distinctions of Polythalamia, with a tabular view of the Bryozoa, according to their orders, families and genera, with their characteristics, pp. 107—121.

   (N.B. Of this tabular view I have given a transcript above.)
9. On the geographical distribution of living Polythalamia on the African and Asiatic coasts of the Mediterranean, and in the Red Sea, with a tabular view of the genera and species, pp. 121—127.
10. A concise Diagnosis of the new families, genera and species,
    1. Of the siliceous Infusoria of the chalk marl, containing thirty-one new species, of which seventeen species belong to five new genera, and fourteen species to five former-known genera, pp. 128—130.
    2. Of the calcareous-shelled Polythalamian animalcules of the chalk and sea sand, sixty-seven new species, beside two new species from the Jura (Oolite) limestone, pp. 130—135.
11. A summary view of the conclusions drawn from the preceding expositions, pp. 135—139.
12. Explanation of the Plates, pp. 140—147.
13. A tabular view of the organic bodies invisible to the naked eye, which form the chief constituents of chalk, chalk marl, the compact limestone of Egypt and Arabia, and the nummulitic limestone of the Pyramids of Geza or Gyzeh.

The reader being thus put in possession of the general sco of the work, I now proceed to exhibit in full the conclusic to which the author has been led (as indicated under the he of No. 11), to which I shall subjoin further extracts taken fr different portions of the Memoir, for the purpose of gene illustration.

Conclusions.

1. Many, and probably all, *White Chalk Rocks* are the pr duce of microscopic coral-animalcules, which are mostly qu invisible to the naked eye, possessing calcareous shells of to $\frac{1}{288}$ line in magnitude, and of which much more than o million are well preserved in each cubic inch, that is, mu more than ten millions in one pound of chalk *.

2. The *Chalk Marls* of the Mediterranean Basin are t produce of microscopic Infusoria possessing siliceous shells cases, mostly quite invisible to the naked eye, intermingl with a small proportion of the calcareous animalcules of t chalk.

3. The peculiar state of aggregation in *White Chalk* do not arise from a precipitate of lime previously held in soluti in the water of the sea, nor is it the result of the accumulati of the small animalcules, but it proceeds from a disintegrati of the assembled microscopic organisms into much minu inorganic calcareous particles; the reunion of which into r gular, elliptical, granular laminæ, is caused by a peculiar cr stalloid process, which may be compared to crystallization, b is of a coarser nature, and essentially different from it. T best writing chalk is that in which this process has been dev loped to the greatest extent.

4. The compact limestone rocks also which bound the N in the whole of Upper Egypt and extend far into the Saha or Desert, being neither white nor of a staining quality, well as the West Asiatic compact limestone rocks in the nor of Arabia, are, in the mass, composed of the coral animalcu of the European chalk. This affords a new insight into t ancient history of the formation of Libya from Syene to t

---

* It is to be understood that I speak only of such Polythalamia as well preserved, wholly disregarding their fragments. Of the well-preser there are contained in one fourth part of a cubic line, or in one twelfth a grain of chalk, frequently 150 to 200 in number, equal to 600–800 each cubic line, or 1800–2400 in each grain, and from 1,036,000 1,382,400 in each cubic inch; and hence in one pound of chalk the nu ber far exceeds ten millions.

The larger Polythalamia and Bryozoa of the chalk are best obtained fr the sediment produced by brushing the chalk under water; the entir microscopic forms remain long suspended in water.

as, and of Arabia from Sinai to Lebanon, thus opening a
ge field to organic distribution.

5. Many of the chalk-like formations bordering on the Me-
rranean in Sicily, Barbary and Greece, really belong to
period of the European chalk formation, as proved by
ir organic contents, although commonly held to be differ-
from the chalk, and considered as tertiary *.

6. The chalk beds of the South of Europe, around the ba-
of the Mediterranean, are distinguished from those of the
th and east of Europe by numerous well-preserved chalk
malcules, and less numerous inorganic laminæ; while in
north and east of Europe these relations are reversed †.

. In the South of Europe the beds of marl which alternate
h the chalk consist of siliceous shells of Infusoria, and flints
wanting; while in the North of Europe beds of flint al-
nate with the chalk, and marls with Infusoria are wanting.
is exchange of character tends to explain the peculiar re-
on of flint to chalk, indicating that the pulverulent sili-
us particles of Infusoria have been converted into compact
lules of flint.

. It has been lately remarked that the chalk which con-
s flints is deficient in numerous siliceous Infusoria, when
pared with the Bilin slaty Tripel or polishing slate (*Po-
chiefer*) containing semi-opal; but this deficiency now dis-
ears, and a rich substitute takes its place, the Infusoria in
North of Europe having been employed in the formation
flints; while in the south, remaining unchanged, they are
served in the Infusoria marls.

. The chalk animalcules resemble most those of the sea-
d and the Miliolites, which, up to the present day, have
n ranged among the Mollusks with the Cephalopods; but
her of these are either Cephalopods or Mollusks, nor even
usoria (as asserted by a late observer); but they are Bry-
a, animals of Moss-corals, which are most nearly related
Flustra and Eschara.

0. The sea downs of some, and probably of most coasts,
still in course of formation by living Bryozoa, which,
ugh very small, resembling grains of sand, are yet, for the
st part, larger than the chalk animalcules, and a large pro-

In Sicily, however, there occur many breccias of chalk, which have
red a subsequent change, and may be referred to the tertiary epoch.

Thus in the white and yellow soft writing chalk of the North of Europe
inorganic crystalloid portions sometimes equal or rather exceed in mass
organic remains; but in the South of Europe, in Sicily, these organisms
their fragments are greatly predominant, consisting, as it appears, ex-
ively of well-preserved Polythalamia.

portion of the sand of the Libyan Desert has been prove consist of such grains. It is only in Nubia above Syene the desert sand becomes a pure detritus of granite*.

11. In the various countries of the earth in which occur w and earthy, as well as coloured and compact rocks, comp of microscopic calcareous animalcules, the genera and sp of these animalcules present so striking an agreement those of the white chalk of Rügen, that they may wel deemed characteristic of one and the same period of ge gical formation. It cannot be asserted for a certainty the same forms have been observed any where else†.

12. In the beds subjacent to and more ancient than chalk, namely, in those of the Oolite or Jura limestone mation, we have also clear evidence of the existence of microscopic Polythalamia. These, however, are such as not hitherto been found anywhere in the chalk.

13. The early assertion that *all* limestone was the pro of animals‡, though resting on no sufficient foundation, therefore justly held in slight regard by modern geolog yet now deserves every attention, since it clearly appears a limestone formation widely extended on the surface of earth is composed of microscopic animals, visibly conve in a gradual manner into inorganic chalk and compact l stone. If similar phænomena appear also in the Jura l stone formation, and should become still further confir these considerations (combined with the long-known exist of coarser corals and shells in both formations) tend to s how necessary it is, when examining the composition of any siderable portion of the solid mass of the earth, to streng our natural senses by artificial means, in order to obtain stinct knowledge of the extent to which organic life may contributed to its production.

14. The extreme minuteness of the chalk animalcules is kingly proved by this, that even in the finest levigated wh multitudes of them are still present, and may be applied out suffering change to the most varied technical purp Thus in the chalk coating given to painted chambers, pa or even glazed visiting-cards (when not coated with white

---

* On these very interesting and not easily developed relations, I at a future day, to be able to make a more special communication.

† If I have applied the same name in some cases both to animalcu the chalk and to forms existing in the present sea-sand, or in recent beds, it has arisen partly from my being unacquainted with the original of the latter, and partly from my desire not to create unnecessary per ity by the adoption of new names. It should be observed that they a stinguished by marks of interrogation. All those which I could really pare were different.

‡ By Linnæus in 1745 and 1748, and Buffon in 1749.

lone), may be seen a pretty mosaic of well-preserved, moss-coral animalcules, but which are invisible to the naked eye. And thus our natural vision receives from such a surface the mpression of the purest white, little deeming that it contains he bodies of millions of self-existing beings, of varied and beautiful forms, more or less closely crowded together (as in Plate IV., where the subjects are magnified 300 times).

Explanation of the Plates and Tabular View.

The Memoir is accompanied by four Plates*, presented with he view of facilitating a comparison between the organic reations of minute fossil bodies invisible to the naked eye, and hose of still living bodies visible to the naked eye.

Thus the first three Plates exhibit recent small bodies naturally visible, with which the naturally invisible forms of the ourth Plate may be readily associated.

The first three Plates serve also to elucidate the true nature of the Polythalamia (hitherto mistaken), showing their greater affinity to the Bryozoa (Flustra) than to all other animal orms, and in particular the great difference there is between hem and Cephalopods and Infusoria. They represent partly he unfolded, soft, external parts of living subjects, and partly ead, naked bodies, artificially divested of their calcareous hell, and not hitherto figured.

Lastly, these first three Plates serve to convey a view, according to some of their principal divisions, of the structure of the whole group of forms occurring in Polythalamia, and n particular to illustrate their frequent assemblage in families, or Polyparies, as they are termed. Plate I. contains simple orms; Plates II. and III. composite or family forms, Polyparies; of which Plate II. contains family forms assembled in ingle rows, and Plate III. family forms arranged in many rows.

If, as already observed, we examine a wall or paper whitened with finely levigated chalk, or a glazed visiting-card not coated with white lead alone, but also with chalk, they would appear, when magnified 300 times, more or less rich in subjects, as represented in Plate IV.

Plate I. contains *simple recent Polythalamia* from the sea-sand of Rimini. Fig. 1. *Rotalia Beccarii*; the shell only was known, but the figures show also the form of the animal when leprived of its shell by an acid, the form of both being the same. Fig. 2. *Marginulina Raphanus* (*Nodosaria Raphanus, Nautilus Raphanus priorum*), also very common at Rimini and other Italian coasts, and which had hitherto been erroneously ranked with Orthocera.

Plate II. contains *Polyparies of recent Polythalamia assem-*

---

* These plates do not accompany Mr. Weaver's paper.

bled in single rows, from the Red Sea and the Mediterranea The two subjects represented in this Plate were collected l me in the year 1823, and it is peculiarly interesting, throug my newly-discovered method of observing*, to have bee able to see in several divisions of the internal body tl remains of the siliceous Infusoria, of which they had made repast fifteen years before. Fig. 1. *Peneroplis planatus*, d'O bigny, *Nautilus planatus* of Fichtel and Moll, from the Re Sea. The shells of this animalcule were hitherto only know but the soft organic animal form which they inclose is here als represented. Fig. 2. *Coscinospira Hemprichii*, a form fro the Red Sea, also found in the Libyan part of the Medite ranean, and which was formerly erroneously placed adjoinin the *Spirula* of the Cephalopods, and more recently as co nected, through *Lituolites nautiloides*, with *Spirolina*.

Plate III. contains *Polyparies of recent Polythalamia asse bled in many rows*. This Plate contains the only living a malcule of the Polythalamia group, hitherto so far observ as to admit of its classification. The three forms given this Plate, constructed of many rows of animalcules, may l distinctly associated with the Flustra and Eschara of tl Bryozoa, to which, through the well-known *Lunulites* a *Orbitulites* (hitherto ranked with coral animals), they approx mate in a convincing manner. Fig. 1. *Orbiculus numismal* from the sea-sand of the Antilles Isles. Fig. 2. *Sorites o biculus* = *Nautilus orbiculus*, Forskål, *Nummulina* (*Assilin nitida*, d'Orbigny, ? from the Red Sea. The same speci lives also in the Mediterranean. In a part magnified 3 times we see the animalcule with eight feelers protruding fro its cell. In some of the cells may be seen distinct shells siliceous Infusoria; in others appear oviform globules. Fig. *Amphisorus Hemprichii* closely resembles the *Sorites*; but has cells on both sides bearing single animalcules, and hen the discs are twice as thick as in *Sorites*. If we compare *Sorites* with *Flustra*, we may place *Amphisorus* by the side of *Eschara*, but, being both free moving bodies, they are different from them.

---

* The new method of observing is the following:—Place a drop of wa upon a lamina of mica, and put into it of scraped chalk as much as w cover the fine point of a knife, spreading it out and leaving it to rest a f seconds; then withdraw the finest particles which are suspended in t water, together with most of the water, and let the remainder become p fectly dry. Cover this remainder so spread out with *Canadian balsam*, t turpentine of the *Pinus* (*Abies*) *balsamea*, and hold it over a lamp until becomes slightly fluid without froth. A preparation thus made seld fails, and when magnified 300 times in diameter we see that the mass the chalk is chiefly composed of minute well-preserved organisms. In t preparation all the cells of the Polythalamia appear at first black with white central spot, which is caused by the air contained in the cells, whic as is well known, appear under water as annular black bodies; but by grees the balsam penetrates into all the single cells, the black rings of t air vesicles disappear, and we recognize all the small cells of the Polyth lamian animals, often presenting a very pretty appearance.

Plate IV. contains the *invisible animalcules of the chalk and chalk marl*, displayed in twelve specimens of rock; 1 to 9 being portions from the chalk, and 10 to 12 from the chalk marl, magnified 300 times. In these specimens the calcareous Polythalamia amount to sixteen species, and the siliceous Infusoria to twelve species, with siliceous spicula of sponges. The twelve localities from which these specimens of the rock masses were derived are the following:—No. 1 to 5, *writing chalk*; namely, 1. from Puszkary, in Poland, opposite Grodno, from the shore of the Memel; 2. from Jütland, in Denmark; 3. from the island of Rügen in Pomerania; 4. from Gravesend, on the Thames; 5. from Meudon, near Paris; *firmer* writing chalk, No. 6, from Cattolica in Sicily; *compact, not writing* chalk, No. 7, from the Mokattum hills near Cairo; and No. 8, from the Catacombs of Thebes in Upper Egypt; *compact gray limestone*, No. 9, from the mountain mass of Hamam Faraūn in Sinai, Arabia; *chalk marl*, No. 10, from Oran in Africa; No. 11, from Caltasinetta in Sicily; No. 12, from Greece.

In the *general table* indicated above, under the head of No. 13 of the contents of the memoir, a list is given of the principal forms of the invisible organic bodies which constitute the rocks from which the twelve above-mentioned specimens were taken, as well as the chalk of Brighton, the chalk marl of Zante in the Ionian Islands, and the nummulite limestone of the Pyramids of Geza in Egypt. From this it results that the principal forms in these rocks consist of twenty-five species of calcareous-shelled Polythalamia, thirty-nine species of siliceous-shelled Infusoria, seven species of soft-shelled Infusoria of the flints, and five species of siliceous plants.

The twenty-five species of calcareous-shelled Polythalamia, belonging to eight genera, are the following:—

Flustrella *concentrica*; Globigerina *bulloides*?, G. *helicina*?; Planulina *sicula*, P. **turgida*; Robulina *cretacea*; Rosalina **foveolata*, R. *globularis*?, R. **lævigata*, R. *pertusa*; Rotalia **globulosa*, R. *ocellata*, R. *ornata*, R. *perforata*, R. *scabra*, R. *stigma*; Textularia *aciculata*?, T. **aspera*, T. *brevis*, T. **dilatata*, T. **globulosa*, T. *perforata*, T. *spinosa*, T. **striata*; Turbinulina *italica*? Quinqueloculina? from Benisuef, is doubtful. N.B. *Textularia globulosa*, when in fragments, is not easily distinguished from *Rotalia globulosa*; and in like manner the fragments of *Textularia perforata* may be confounded with *Rotalia perforata*.

The thirty-nine species of siliceous-shelled Infusoria belor to fourteen genera, and are as follow:—

Actinocyclus *ternarius*, A. **quaternarius*, A. **quinarius*, *senarius*, A. *septenarius*, A. *octonarius*, A. *denarius*; Coccon ma *Cretæ*; Cornutella *clathrata*; Coscinodiscus *Argus*, *centralis*, C. *lineatus*, C. **minor*, C. **Patina*; Denticella *Fr gilaria*, D. *tridens*; Dictyocha *Fibula*, D. *Navicula*, D. *po actis*, D. *speculum*, D. *stella*, D. *triangula*; Eunotia *zebr* Fragilaria *rhabdosoma*, F. *striolata*?; Gallionella *aurichalca* G. *sulcata*; Haliomma *Medusa*, H. *crenatum*; Lithocam *lineata*, L. *Radicula*, L. *solitaria*; Navicula *africana*, N. *E cillum*, N. *eurysoma*, N. *ventricosa*, N. *sicula*; Pyxidicı *prisca*; Synedra *ulna*.

The seven species of soft-shelled Infusoria of the flints b long to three genera, and are the following:—Chætophy *Pyritæ*; Peridinium *pyrophorum*†; Xanthidium *bulbosum*, *furcatum*, X. *hirsutum*, X. *ramosum*, X. *tubiferum*.

The five species of siliceous plants belong to two gene namely, Spongia (*Tethya*?) *aciculosa*, S. *cancellata*, S. **C brum*, S. *binodis*; Spongilla (*Tethya*?) *lacustris*‡.

Of these principal forms the before-mentioned rocks parta in the proportions as stated below: namely,

	Species of Calcareous Polythalamia.	Species of Infusoria.		Species o Siliceou Plants.
		Siliceous, in Chalk.	Soft-shelled in Flints.	
The *Chalk* of				
Puszkary contains	6			
Rügen	7	1		
Jütland	6			
Gravesend	6	3	5	
Brighton	7	1	4	
Meudon	9	.....	2	
Cattolica	9			
The *Chalk Marl* of		Siliceous Infusoria.		
Caltasinetta	7	27	......	4
Oran	2	18	......	1
Zante	5	8	......	2
Greece	3	9	......	1
The *Compact Chalk* of				
Egypt	8			
Arabia	6			
The *Nummulite Limestone* of The Pyramids of Geza ...	6	Containing 4 species of N mulite, the largest of wl is one inch in diameter.		

---

† *Peridinium delitiense* has hitherto been found only in flint pet near Delitzsch, yet accompanied with forms that are common in the f of the chalk.

‡ In the preceding lists, the species which are marked with an asteri are those which most frequently occur, forming the masses of the ro The *Rotalia globulosa* occurs in all the localities.

On the Chalk Marl, and its relations to the Chalk and its Flints.

The whole coast of Oran in Africa appears to belong to he chalk formation, composing the plain east of the town, nd extending thence to the Atlas. The marl brought from hence as tertiary by M. Rozet in great quantities I had an pportunity of examining in Paris, and I found not only Po-irschiefer and an Infusoria conglomerate, but calcareous ani-nalcules of the same species as occur in the chalk of Poland, Rügen, Denmark, and Paris, and which there mainly contri-ute to its mass. It thus appeared that the so-called tertiary ormation of the coast of Barbary might, without much hazard, e brought into a nearer connexion with the chalk. In his lescription of this tract, M. Rozet states*, "The tertiary ormation is extensively developed in Oran, forming the soil f the large plain on the east of the town, and on the south o the Atlas. It forms also the sea-coast to an extent of 3000 netres between Mers el Kebir and Cape Falcon, and the vhole soil of the adjacent plain. The lower bed is a blue narl, like that which we found at Algiers and within the Atlas. t appears destitute of organic remains. The second or upper leposit consists of marly and calcareous beds in alternation, orming a thickness of 30 to 40 metres. In the plain these eds are apparently horizontal, as well as in the elevated plain f the Rammra hill; but in the hills south-west of the town f Kasba they are, on an extent of two hours march, inclined o the north, at an angle sometimes exceeding 30°. The beds f limestone are white and chalk-like, yellowish and coarse ranular, usually forming the lower part, succeeded by others lternating with yellow marls, which are often slaty and harged with sand, and between them are found layers of streæ and other shells. Among them two beds are distin-uished, each one metre in thickness, composed of very white inely-laminated marl, containing numerous well-preserved mpressions of fishes, so that in a cubic mass of one foot we eldom fail to find three or four fishes. In these beds of marl hus enclosing the fishes, other organic remains do not appear; ut in the calcareous and sandy beds which intervene, occur ayers of large oysters mingled with grypheæ. The upper art of this deposit is composed of a calcareous breccia, which s exhibited at the surface in the soil of the whole plain on he south-west of Oran."

This exact description of the position and thickness of the vhite marl with impressions of fishes, has a reference to the

---

* Rozet, *Voyage dans la Régence d'Alger*, Paris, 1833. tome 1. chap. v. p. 56, 63.

Infusoria conglomerate of Oran, to which I have already adverted. It is probably what formed the Tripel of the earlier periods of Italy. When M. Rozet speaks (at p. 28–30) of the great extent of the tertiary tract near Algiers as similar in its relations to those of Oran, I cannot agree with him. On the contrary, forming my judgment by the organic remains, I consider the desert tract near Algiers as really composed of a tertiary formation, which reposes on chalk. This opinion is founded on my observation, that the tract in Libya, extending from Alexandria to Siwa, is composed of tertiary beds, while from Cairo to Geza the chalk formation occurs, which terminates at the granite of Syene, but is far spread into the Desert. The valley of Siwa appears to form the northern boundary of the chalk in Eastern Libya.

In the South of Italy, at Caltasinetta and its neighbourhood, the relations had been correctly seized by our late friend Frederick Hoffmann, from whose diary I have been favoured with an extract by M. von Dechen. He represents the series of strata which occupy the greater part of Sicily as composed of limestones, sandstones, clays, and marls; the lower members being probably referable to the Jura formation, succeeded by such as clearly belong to the chalk, and many beds of which perfectly resemble the hard chalk of the north-west of Germany (Teutoburger Wald). Among the marls are white chalk-like thinly laminated masses, analogous to Tripel, designated by Hoffmann as *white chalk marl*, and which especially occur in the southern part of the island. The beds of the chalk formation usually dip 20° to 30°, while the strike is nearly constant, from 15° to 45° S. of E. and N. of W., parallel to the south coast. The tertiary beds which succeed the chalk are composed of loose sand, friable sandstone, testaceous breccias, clays and limestones. They cover the chalk unconformably, resting on the truncated edges of the latter. The chalk beds are upon the whole poor in organic remains, and these are seldom distinct; there occur Hippurites, Nummulites, Lenticulites, and in a few places indistinct Ammonites and Belemnites, while the tertiary beds are quite filled with innumerable Mollusks, of which nine-tenths are still living in the Mediterranean. This distinction is so striking that it scarcely required the difference of relative position in order to draw a correct line between the two formations. Even had so circumspect a geologist as Frederick Hoffmann not correctly seized and pronounced with decision on these local relations, the numerous microscopic siliceous Infusoria with calcareous Polythalamia which I have found in the chalk marl would have led to the same conclusion.

If we compare Hoffmann's description of this portion of icily with that given by Rozet of the coast near Oran, we nnot avoid recognizing a similarity of relations; and the inly laminated marly beds with impressions of fishes, etween Caltasinetta and Castrogiovanni, which Hoffmann fers with certainty to the chalk formation, correspond to the milar beds which occur near Oran, but which were said to e tertiary. And the parallel is confirmed by the microopic siliceous Infusoria and calcareous animalcules which I ave discovered in both.

The genera and species of the siliceous Infusoria in Sicily e so similar to those of Oran and Zante, that of thirty-six ecies, four occur in all the three countries, three in Calsinetta and Zante, seven in Caltasinetta and Oran, while in l of them the *Coscinodiscus Patina* is greatly predominant. f all these siliceous animals, not a single species has been und in the chalk of the North of Europe, nor even in the nts. On the other hand, the calcareous-shelled animalcules, hich in the South of Europe accompany the siliceous anials, comprise about one half of the same species that are und in the North, yet exceeding them in quantity.

From the examination of the organic constituents of the alk marl we learn the hitherto unknown fact, that numeous swarms of microscopic Infusoria were in existence within e period of the secondary formation of the earth's surface, iefly belonging to such as possess siliceous cases or shells, d which for the greater part are members of such sections the Bacillaria family as had previously appeared to be conned to the tertiary or newest formations.

Of the thirty-nine or forty species of siliceous Infusoria ccurring in the chalk formation, thirty-four or thirty-five ave not hitherto been found in the recent state; but it is rearkable that the remaining five or six species so closely esemble existing species of the present day, that they present o peculiar character by which they could be distinguished om them, and hence the application of new names appeared admissible. They are, *Eunotia zebra*, *Fragilaria rhabosoma*, *F. striolata ?*, *Gallionella aurichalca*, *Navicula ventriosa*, *Synedra ulna*[*].

In the chalk itself only four out of the thirty-nine or forty ecies of siliceous Infusoria have hitherto been met with, namely, *Fragilaria rhabdosoma*, *Fragilaria striolata ?*, *Gall nella aurichalca*, and *Pyxidicula prisca*. They are very ra and found only in the vicinity of the beds of flint.

---

[*] The indifference shown to climate by Infusoria, and the peculiarity of eir organic development, seem to render it possible that they might be ore readily preserved through many catastrophes of the earth than other rms. By the faculty which they possess of spontaneous division, a single dividual can, under very favourable circumstances, be multiplied in the urse of a few hours to the extent of millions.

On the Composition of the Compact Limestone of Upper Eg and Arabia by the invisible Animalcules of the White Ch of Europe.

Both the nummulite limestone of the pyramids of Geza the left bank of the Nile, and the same kind of rock on right bank near Cairo, contain numerous microscopic a malcules of the chalk, which serve as a cement to the Nu mulites. I had often examined microscopically specim which I had brought from thence, but I did not succeed separating and rendering visible the different elements w equal clearness, until I applied my newly-acquired practi which was much facilitated by immersing these stones a lon time in water. The same result attended the examination the other calcareous rock masses of Upper Egypt and Ara showing that the animalcules of the chalk occupy in a s prising manner a wide extent of country in Libya.

Nummulite limestone, wherever occurring, has been m usually referred to the tertiary period, although perhaps of belonging to the chalk. In Egypt it possesses no great tent. On the right bank of the Nile it is deposited only the small hills near Cairo, and on the left bank, as it appea in a tract extending from Siout to the declivity of the co pact limestone, which latter constitutes the mass of the ro that line the course of the Nile in Upper Egypt. It for the foundation and principal material of the Pyramids. Nor ward it is directly bordered by the slimy delta of the Nile, productive soil of Egypt. Between the Oasis of Jupiter A mon and the Mediterranean, is a wide elevated plateau or ta land of rock, among whose numerous organic remains known tertiary forms. The whole of Upper Egypt, as fa Syene, has a similar character. In 1828, though assu that its limestone rocks were more ancient than the terti period, yet, from want of distinct fossils, I was doub whether they might not be referred to the Jura formati On the south, and not far from Syene, this limestone is cumbent on sandstone (Quadersandstein ?), and the two rep on granite and the primary rocks connected therewith. I g these views in 1828 in the geologically coloured map wh accompanied the first section of the first volume of my Trav in Egypt, Libya, Nubia, and Dongola.

It now results, from the microscopic examination which taken place, that the whole of the limestones of Benisu

ut and Thebes, on the western bank of the Nile, and of
iro and Kineh (including the gray marl near Kineh), on
eastern bank, and which inclose the Nile at an elevation
frequently 100 to 300 feet above its level, extending along
river full sixty German miles in length, are, like the
mmulite limestone, composed of an inconceivable accumu-
ion of microscopic calcareous-shelled animalcules, which
of precisely the same genera and species as those which
nstitute the chalk of Europe. The table-land formed by
ese calcareous rocks extends far westward into the Desert,
d it is perhaps principally composed of them.

A new and unexpected light is thus thrown on these exten-
e regions. The phænomena apparent in Egypt may be
nnected with those of Western Africa. It has been already
own that the same animalcules constitute the territory of
an, stretching far along the foot of the Atlas; and when
consider the equality of surface which prevails in the plain
the Great Desert, or Sahara, of the North of Africa, and
mpare it with what I have myself seen along the whole ex-
nt of its eastern border, as well as on a large portion of its
rthern, we may be well permitted to think of a similarity
composition.

But these distinct indications of a similar organic influence
tend not merely to the west but also to the east of Cairo,
panded into Asia. The specimens collected by Dr. Hemp-
h and myself from Hamam Faraûn, and Tor in the Sinaian
rtion of Arabia, which I had formerly considered as ash-
ay marl and yellowish-gray limestone of the tertiary epoch,
ere now proved, by the new method of examination, to con-
t of quite the same microscopic chalk animalcules as con-
tute the hilly masses of Upper Egypt. And from hence
is formation appears to be continued eastward far into the
terior of the Great Desert plain, trending toward Palestine;
t on the Arabian coast of the Red Sea we did not find it
rther south than Tor, which locality alone, among all the
ints of the east, yielded flints similar to those which occur
the European chalk.

We have here to remark on the absence of siliceous animals
this limestone and marl formation, while the so-called
gyptian pebbles and jaspers occupy the same position in
rizontal layers as the flints in the North of Europe, appear-
g as their substitute. But in these jaspers the organic sili-
ous elements are no longer to be distinctly found by reason
their intermixture with other substances, and their conse-
uent opacity, giving rise to dendritic and other delineations.
seems as if the solution and conversion of the organic into

the inorganic in the Egyptian pebbles (*Cailloux d'Egyp* is throughout more perfect than it is in many flints, althou the constituent elements of both kinds of stone are very probably quite the same.

On the principal Organic Calcareous Forms which compos the mass of all Chalk.

From what has been already stated, it is evident that production of the calcareous mass of the chalk is not to attributed, as formerly conceived, to the larger organic bodi but to the minuter, and in the greatest measure to such are invisible, consisting of eight genera of Polythalamia w twenty-five species, and excluding all such as may be dist guished by the naked eye, that is, exceeding $\frac{1}{24}$th of a line magnitude; the latter, however, are comparatively rare. is possible that several other, and perhaps many species of same genera, may yet be discovered in the chalk, as well other genera, since the investigations hitherto made co only be applied to a minimum of its substance; yet, as th were conducted by me on chalk from many regions, it d not appear probable that other sections of the animal ki dom will be found to have taken so great a share in the for ation of chalk as the Polythalamia, the principal prevaili forms of which I have indicated.

From the preceding it is also apparent that the chalk roc of all countries agree in their constituent organic forms n only according to the zoological class, but also in genera, a for the most part in species likewise; this character being n confined to the white tender writing chalk of Europe, but e tending also to the compact limestone rocks of the North Africa and the West of Asia. Particularly striking is t characteristic persistence of single forms through all the different and widely-separated countries. Thus in all of the are to be found *Rotalia globulosa*, with *Textularia globulos* *T. aciculata ?*, and *T. striata*, as well as *Planulina turgid* thus giving a common character to all these rock formation and this character becomes the more important, when we co sider that these forms are the most numerous, and in fact a the chief constituents of the chalk[*].

If now the question be asked whether the forms which occ in such masses in chalk belong to it exclusively, and are hen to be considered characteristic of that formation, I am almo disposed to reply in the affirmative. The analogous for which occur in sea-sand, tertiary sand, and indeed in all modern formations, are viewed for the most part as different and larger species, although of the same genera; and it does not appear that any of these forms can be referred with perfect certainty to such as are now living in the sea.

---

[*] The Polythalamian forms which Mr. Lonsdale noticed in the Engl chalk in 1837 as visible to the naked eye, and amounting to 1000 in o pound of the chalk, and which, with Mr. Lyell, he has named *Lenticul*

To the theory of the formation of limestone, the observation is important, that these organic deeply-seated relations are not peculiar to the chalk formation. The tertiary calcareous beds consist, in like manner with the chalk, of multitudes of such Polythalamian animals, which compose in many quarters sandy sea-downs of great extent; and even in the sandy desert of Libya we can recognize distinct Polythalamia. On the other hand, having succeeded in discovering microscopic Polythalamia in the compact flints of the Jura limestone from Cracow, which are of decidedly different forms from those of the chalk, the calcareous animals being *Nodosaria urceolata*, n. sp., and *Soldania elegans*, n. sp., and the siliceous *Pyxidicula prisca?*, with *fragments of soft sponges*, it becomes apparent that such invisible organic bodies were also present in the formation of the Jura limestone.

[To be continued.]

LXIII. *Notice of an undescribed Native Subsulphate of Iron from Chili. By* JOHN PRIDEAUX, *Esq.*[*]

THIS specimen, of which I have not found any description, was brought to Sir Charles Lemon's Mining School by Edward Hookham, one of the students, having been sent from Chili to Captain N. Vivian, by his son, without any geological reference.

Form, mammillary, or curved lamellar about one-sixth of an inch thick. *Structure*, fibrous parallel, transverse to the laminæ; *fracture* corresponding; fibres crystalline, but too minute to be easily definable. *Brittle* in mass, fibres rather flexible. H 2·5; specific gravity below 2·5.

and *Discorbis*[†], appear, judging by the figures, to be referable to *Rotalia ornata* and *R. globulosa*, including perhaps fragments of *Textularia globulosa*.

I may here remark, that my continued researches on the Polythalamia of the chalk have convinced me, that very frequently in the earthy coating of flints, which is partly calcareous and partly siliceous, the original calcareous-shelled animal forms have exchanged their lime for silex, without undergoing any alteration in figure, so that while some are readily dissolved by an acid, others remain insoluble; but in the chalk itself all similar forms are immediately dissolved.

[*] Communicated by the Author.

[†] Dr. Buckland's Bridgewater Treatise, 2nd Edition, vol. i. p. 448. 1837. Lyell's Elements of Geology, 1838.

Lustre shining, silky on the fibrous face; dull on tha the laminæ. *Transparency* of the laminæ 0, fibres tra cent. *Colour* pale greenish gray; externally yellowish, f adhering sulphur. *Taste* slightly acid and astringent; *odorous.*

Solubility. In cold water little or none; but the fibres asunder: on applying heat, the colour changes to orange, the hot water dissolves a portion, acquiring its taste. In riatic acid swells like sponge, assuming a rich orange col and soon dissolves, except a little residue of sulphur and e

By dry heat, in tube closed at one end, the fibres sepa and become orange-coloured, giving off much water a little sulphur: by increased heat, sulphuric acid is driven and the residue left red. In open tube, much the same: charcoal, before the blowpipe, exhales the odour of sulp and shrinks exceedingly, leaving a residue of oxide of iro

By analysis,

			Oxygen.
Peroxide of iron	31	=	9·5
Sulphuric acid	26	=	15·5
Water	33	=	29·
Sulphur, earth and loss	10		
	100		

the acid containing 1·5, and the water three times the ox of the base, formula $2\ Fe_2\ O_3 + 3\ S\ O_3 + 18\ Aq$, contai half as much acid as the persulphate of iron, and a little cess, to which the sapidity is probably due.

Its aspect and composition suggest the name Fibroferr

J. Pridea

LXIV. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

May 27, 1840.—THE memoir "On the Classification and Distrib of the Older Rocks of the North of Germany," by Prof. Sedgwick and Mr. Murchison, commenced at the pre meeting, was concluded[*].

In an introduction of considerable length, the authors enter historical review of the different steps by which they had been during the former year, to place nearly all the older slates of De shire, and a considerable part of the slate rocks of Cornwall, group intermediate between the carboniferous and Silurian syst and therefore coeval with the old red sandstone of Herefords To the vast group of slate rocks, so defined, they proposed the of Devonian System; and their leading object in visiting Belg the Rhenish provinces, the Hartz, &c., was to ascertain wheth any of those countries there was a group of strata (no matter of mineralogical character) with Devonian fossils, and in a pos

---

[*] See L. E. and D. Phil. Mag., vol. xvii. p. 508.—Edit.

termediate between the carboniferous and Silurian systems. Should
ch a group exist on the continent, then would the Devonian sy-
em be established, not merely on plausible arguments derived from
suite of fossils, but also on the more direct evidence of natural
ctions.

With these views the authors endeavoured (1.) to ascertain the
tural descending order of the formations on the right bank of
e Rhine, between the Westphalian coal-field and the chain of the
aunus. (2.). To ascertain the same order in Belgium, and among
e ancient rocks on the left bank of the Rhine, north of the Hunds-
ck.

In the course of the summer they also made (with similar objects)
veral traverses through the Hartz, and one long traverse from the
huringerwald to the north flank of the Fichtelgebirge, in the hope
bringing into relation with their previous observations, the country
hich has become so celebrated from the labours of Count Munster.

The authors follow this order in the descriptive parts of their
per. But before commencing their detailed sections, they explain
some length the method of determining the order of superposition
nong rocks, like those of Belgium and the Rhenish provinces,
hich are not only much contorted, but often in a reversed position.
his order of superposition can be made out only by sections, which
e of two kinds, *vertical* and *horizontal*. *Vertical sections*, where
e strata are not inverted, not only indicate the natural group of
rata, but their true order of superposition, both of which may often
e ascertained on a single line of traverse. But *horizontal sections*,
owing the intersection of successive groups of strata with the ac-
al surface of the country (and represented by the colours of a
eological map), can only be examined by following the lines of
rike. The colours of such a section (if derived from strata origin-
lly conformable) must show the masses, however contorted, in their
ue juxtaposition. Hence we may define from the horizontal sec-
ons of a country a true consecutive geological series; and if the
lative age of any two contiguous terms be known, the relative age
f all the other members of the section may be inferred with cer-
inty, though the formations be in an inverted position, as seen on
e line of any one vertical section. It was by this laborious method
f "horizontal sections," or, to use his language, by determining the
mmetry of position of the several formations, that Professor Du-
ont first disentangled the perplexing phænomena of the Belgian
rovinces[*]. The authors, after acknowledging the great value of
is principle, state that they endeavoured never to lose sight of it
estimating the value and interpreting the meaning of the many
ertical sections they examined in their long traverses through the
rovinces they describe.

§ 1. *Coal-fields of Westphalia, &c.*—The authors commence their
escending sections, on the right bank of the Rhine, with the pro-
uctive coal-field, which occupies an irregular triangular area,
ounded towards the north by greensand and cretaceous deposits,

---

[*] See L. E. and D. Phil. Mag., vol. xvii. p. 303.—Edit.

towards the south-east by older formations, afterwards to be
scribed, and towards the south-west by an irregular line, skir
the low country near the Rhine, and passing near Mulheim, Ketw
Werder, and thence to a point a few miles north-east of Elberf
In its lithological character and fossil contents it is not to b
stinguished from the coal-fields of England. It is affected by n
anticlinal and synclinal lines, which have brought a lower and
productive portion to the surface, and thrown the productive
tions into a number of irregular troughs, ranging in the directio
the strike, east-north-east.

The lower and unproductive coal-field is composed in par
coarse grits, well exposed on the banks of the Ruhr, between
decke and Schwerte, and of yellowish and light-coloured sandst
and grits, with thin seams of coal and impressions of plants; and
strata are underlaid by dark gray micaceous slates and thin-bed
hard sandstones, of great thickness, marked by many obscure imp
sions of small plants. The lowest member of this series contains n
dark pyritous shale (*Alaun Schiefer* of the Germans), and repose
the upper calcareous zone of Westphalia (mountain limesto
Several sections are described which confirm this order of
perposition. The authors then state that this lower division o
coal-field is greatly expanded towards the north-east; that it is li
logically almost identical with the great *culm-field* of Devon,
resemble it also in its numerous impressions of small plants.
the *Flötzlehrer Sandstein* of the German geologists, and had
regarded by them as the highest member of the graywacke se
but in the recently published map of Von Dechen, it is placed
the parallel of the millstone grit of England.

§ 2. *Carboniferous limestone of Westphalia* (*Berg-Kalk*), *K*
Schiefer, and bituminous limestone, &c.—The authors next describe
limestone which commences at Cromford, near Ratingen, and ra
east-north-east to Velbert. Thence deflecting to the valley of
grath, north of Tonnesheide, it is cut off, and does not form a
tinuous band (as represented in all the German maps), with a lo
limestone, which commences a few miles further south, and ra
through Metman to Elberfeldt. Near Cromford the limeston
thick-bedded, and in its structure and fossils resembles the g
scar-limestone of England. For proofs the authors give severa
tailed sections, and quote published lists of fossils. In its rang
the east it becomes more cherty, and abounds in casts of the st
of Encrinites, so as to resemble the screw-stones of Derbyshire.
several places (*e. g.* Isenbugel, Velbert, &c.) the connexion of
limestone with the upper series is well exposed. The upper bed
limestone pass into dark flat-bedded flinty slate, which is overlaid
psammite and shale, with thin courses of flinty slate, and these
under the lower members of the coal-field. Again, there is at
bert a clear proof that the limestones, screw-stones, and flinty s
dip under the alum-slates of the neighbourhood.

Following the strike of the county still further to the east,
limestone range loses its mineral character; but a large grou

ıta (dipping under the alum-slates above-mentioned, and resting dark shales, like those which form the base of the limestone) ocy the exact place of the carboniferous limestone in the transverse tions. The group is characterized by dark flinty slate (*Kiesel hiefer*) and dark and often fetid thin-bedded limestone, and so sely resembles the culm-limestone series of Devonshire, that the cription of one formation might almost serve for the other. Like culm-limestone, it also contains many Goniatites and Possidoniæ; l among the latter, the *Possidonia Becheri* of Devon. It wants, vever, the numerous species of mountain limestone fossils of the s above-noticed, a fact which the authors explain by a reference change in all the physical conditions of the deposit. This group, owing all the sinuosities of a most contorted country, and somees doubled back upon itself for many miles together, may be ed by its *Kiesel Schiefer* and Possidonia schists, and sometimes its black fetid limestones, to the eastern limit of the chain of older ks near Bleiwasche and Stadtberge.

3. *Devonian System.*—The authors next describe the rocks imliately inferior to the carboniferous groups. The mountain limeıe of Cromfort, above-described, rests on dark-coloured shale, but descending section is much obscured by overlying deposit. In long range of the same series, from Elberfeldt to Menden, there many clear transverse sections, exhibiting in greater or less perion the following *descending order.*—(1.) Immediately under the er limestone shales are many reddish bands, with calcareous conions, in which the Possidonia and some of the species of the erior groups are still found. (2ndly.) These are succeeded by ell-marked range of psammites and coarse flagstone. (3rdly.) m beneath the psammites rise a series of shales, and bands of nmite of dark colour, with here and there thin courses of inferior estone, in which we find flattened Goniatites, and shells of a cies different from those of the overlying formations, among which ecially is noticed the *Terebratula aspera* of Schlotheim. These therefore, considered as forming a part of an inferior system, the first and second groups of the section may be regarded as le up of beds of passage between the carboniferous system and which is below it. The sequence here given is compared with highest beds of the Devonian series, immediately under the culm sures, and with the yellow sandstones of Ireland described by Griffith.

3 *a. Lower Limestone of Westphalia, &c.*—This limestone rises nediately from below the third group of the preceding section. range (from the neighbourhood of Ratingen, in the valley of the ne, to the confines of Hessia) is described in detail. Its changes nineral structure—its separation here and there into two zones—contraction in one place and its great expansion in another—its rmous flexures and occasional inversions of position—its re-apance at Warstein and Attendorn, in consequence of such flex,—all these phænomena are noticed in their turn. As a whole, ıs so great a resemblance to the limestone of South Devon, that

through large tracts of Westphalia the two could not, by a serie land specimens, be distinguished from one another. The fossil this limestone are very abundant, and several sections are give detail, to show their local distribution. Among the most charac istic and abundant in these sections the following are enumerat *Stromatopora polymorpha*, *S. concentrica*, *Favosites ramosa*, *Fa sites polymorpha*, *F. spongites*, *F. Gothlandica*, *Strygocephalus B tim*, *Gypidium*, *Terebratula aspera*, *Turritella coronata*, *T. bilin* (Schlotheim), *Buccinum spinosum* (Sowerby), &c. &c. From these facts, it is inferred, that this lower limestone of Westphalia true Devonian limestone, exactly or very nearly on a parallel the great limestone of South Devon.

Local and detailed lists are added, and detailed sections are gi connecting the whole series both with the upper and lower for tions, especially one from the Possidonia schists and black limesto near Schelke, through the Devonian limestone, and to the lo formations exposed on the banks of the Lenne, towards Altena. this section there is no ambiguity, and the defective evidence in sections of Devonshire, when we endeavour to connect the cu measures with the South Devon limestone, is here amply suppli

The authors then describe in detail the sections at Paffrath, Bensberg, on the right bank of the Rhine, near Cologne, where same Devonian limestone occurs, with a magnificent series of fos its position is, however, reversed, as it seems to dip under the li stone near Bensberg, which is referred to the upper part of the lurian system.

To the same geological epoch the authors also refer the con cated metalliferous deposit of Dillenburgh, and the limestones of Lahn in the country of Nassau. At the former place the great tortions and the extraordinary intrusions of trappean rocks make relations difficult; but, considered on a great scale, the vast fo iferous and calcareous group reposes on rocks considered of the lurian age: it contains a true Devonian group of fossils, and upper portion at Herbon is surmounted by a Possidonia schist, fectly identical with that of Westphalia. The limestones of Lahn at Dietz, Weolburg, Wetzlar, &c., are still more unequivo Devonian; and though the alternating masses of limestone and s are of enormous thickness (rivalling in that respect the whole s of limestones and slates in South Devon), and the sections c obscure, yet in descending the Lahn from Dietz to Nassau and Ems, they had a proof that the calcareous system is underlai Silurian rocks. The appearance of these Devonian deposits the eastern limit of the old rocks, on the right bank of the Rhi accounted for by enormous undulations, which have repeated again, in three or four great parallel troughs, the formations w appear in their true place in Westphalia, on the northern limit o same ancient formations.

§ 4. *Silurian System.*—The authors next describe the grea ries of rocks which rise from beneath the lower Westphalia l stone, and state that in the long range from Elberfeldt to Ise

descending order is unequivocal. The passage downwards is
netimes effected by flagstones, with bands of shale, containing
n calcareous courses. In other places, the shales are more abun-
nt, occasionally becoming much indurated; and in the range to-
rds the north-east (for example, near Meschede) this group be-
nes greatly expanded, and contains many quarries of roofing-slate,
h a true oblique cleavage. This part of the series is compared
h the shales under the Eifel limestone, and with the Wissenbach
tes, which underlie the Devonian limestone series of Dillenburg.
e great difference in the development of this group produces a
at difference in the fossils, but on the whole, they are regarded
forming a passage between the Devonian and Silurian types. A
of fossils is subjoined, and the authors regard the numerous Go-
tites as rather connecting the group with the overlying Devonian
ks; while the trilobites and orthoceratites, &c., some of which
nnot perhaps be distinguished from known Silurian fossils, seem
link it to the Silurian system.

Below the preceding comes a group of vast thickness, composed
earthy schistose beds, passing on one hand into shale, on the other
o coarse slate, and alternating indefinitely with bands of psammite,
netimes passing into coarse arenaceous flagstone, sometimes into
ck beds of sandstone. Nearly throughout are occasional obscure
getable impressions, and in the upper part are courses of lime-
ne and calcareous bands, with innumerable impressions of fossils.
the lower part, the limestone bands seem gradually to disappear,
d the whole passes into a formation of graywacke and graywacke
te, in some rare instances producing a good roofing-slate. For many
ies south of the undisturbed range of the lower Westphalia lime-
ne, the prevailing dip is about north-north-west. The country
und Siegen is regarded as a kind of dome of elevation, composed
the lower part of this series; for still further south the dip is re-
sed to the south-south-east; and in a traverse from Siegen to the
unus, across the strike (a distance of about fifty miles), the same
is continued, with very few interruptions. Considering their
h inclination, this fact seems to give an almost incredible thick-
s to the deposits in question. But the vertical sections do not
e the order of superposition; for at Dillenburg, and on the Lahn,
o great Devonian troughs are brought in among the older strata,
hout any general change of dip; and if we accepted the vertical
tions as the sole proofs of superposition, we must place the Devon-
and a part of the carboniferous series under the chain of the
unus. The authors therefore endeavoured to apply the method of
ofessor Dumont, and found their results confirmed by the sections
the lower Lahn.

Many other local details are given, and the authors having deter-
ned the geometrical position of the great mineral masses, next at-
npt to define their age from their fossils. In the arenaceous and
careous group under the lower Westphalia limestone, many spe-
s of the genus Pterinea (Goldfuss), Homalonotus, Orthis, &c. &c.,
gin to prevail. Along with these are forms at present unknown

in England, *e.g.* Hysterolites of Schlotheim, and two species of
thyris,—*D. macroptera* and *D. microptera* (Goldfuss). The
groups of fossils are found on the banks of the Rhine; and
quarry near Unkel are many fossils of the genus Orthis, a
which were *O. pecten*, *O. flabellula*, *O. rugosa*. Along with
was *Terebratula Stricklandii*, and the group was considered ch
teristic of the lower Silurian rocks of England.

On a review of the whole evidence, the authors place thi
succession of strata in the Silurian system, without professi
separate the several parts into distinct groups, on a parallel wit
several groups of the Silurian system of England. This is fo
den by the absence of distinct calcareous bands, and also b
great vertical range of some of the fossil species, which are f
almost from the highest beds to the lowest of the whole s
Several lists of fossils are then given, in confirmation of these
ral views; and it is thence concluded, that the great sequen
coarse earthy schists, calcareous bands, arenaceous flagstones, p
mites, &c., are the representatives of the upper Silurian system
that the lowest quartzose, graywacke, flagstone, roofing-slate
which in some places have no fossils, and in others have num
repetitions of a few species of the genus Orthis, belong to the
Silurian, or upper part of the Cambrian Systems.

Part II. *Older formations on the left bank of the Rhine.–*
Hartz.—Upper Franconia, &c.

§ 1. The authors commence with a short description of the
sical region extending from the coal-field of Belgium to the s
eastern flank of the Ardennes, and then in like manner descril
country between the same coal-field and the limestone of the
They afterwards discuss, at some length, the methods used by
fessor Dumont to determine the superposition of the natural gr
and partly from considerations derived from the symmetric
rangement of the mineral masses, and partly from the direc
dence of sections, especially in the Eifel country, show tha
geological sequence has been correctly determined. So far a
ing the views of Professor Dumont, the descending order i
provinces above-mentioned is as follows:—

(1.) Coal country.......... *Terrain Houillier.*
(2.) Anthraxiferous country. *Terrain Anthraxifère.*
(3.) Slate country......... *Terrain Ardoisier.*

The second of these divisions is subdivided into four n
groups or systems, viz. Upper calcareous system; Upper qu
schistose system; Lower calcareous system; Lower quartzose
stose system.

The slate country is also divided into three groups,—U
Middle, and Lower.

The order being assumed as fixed, the next question is as t
British equivalents of the successive divisions or subordinat
stems.

Respecting the Belgian coal-field, there is no doubt: it is o
same horizon with the great coal-fields of England. Throu

considerable part of its south-eastern boundary it is inverted, so as to dip under the older formations; but on a part of its northern boundary the older formations emerge in their regular order.

The upper limestone of the second division is undoubted mountain limestone. The only question, then, is respecting the equivalents of the three lower divisions of the *Terrain Anthraxifère*, which are, by Professor Dumont, respectively classed with the Ludlow rock, Wenlock limestone, and Caradoc sandstone formations. This classification is not accepted by the authors, for reasons stated in detail.

The *upper quartzo-schistose system* is separable at two parts very different from one another: the higher, often characterized by an open-grained yellowish psammite; the lower (with many variations of structure, and with occasional subordinate calcareous bands) abounding in a dull greenish-gray earthy schist, not unlike the "mudstone" of the Ludlow rocks. But the higher grits and psammites pass insensibly into the bottom beds of the upper limestone (mountain limestone), and contain a series of fossils so near the carboniferous type, that it is difficult to draw a line between the two deposits; and the lower earthy schists do not contain (among the specimens brought away by the authors) one single species found in the Ludlow rock.

The *lower limestone* of the second division is then described in detail, both as seen in Belgium and the Eifel. The authors dwell some time on the remarkable association of the Eifel dolomites with volcanic rocks of different ages: but they contend that the dislocation and contortions of the older strata, and their changes of mineral structure, are not generally due to the more recent igneous eruptions. A comparison of the lists of fossils from the Eifel and lower Belgian limestone, show that they belong to a group identical with that of the lower limestone of Westphalia and the limestone of Paffrath, and that they present the closest analogies with the fossils derived from the limestones of South Devon; some of the most abundant species, both of shells and corals, being identical in all the localities. Hence the authors conclude, that the second and third members of the *Terrain anthraxifère* of Professor Dumont form a part of the Devonian system, and not a part of the Silurian system.

Lower quartzo-schistose system.—In Belgium it is harder and more quartzose than the upper division, and also of more varied mineral structure; and in its upper portion contains some thick beds of conglomerate, which, from their mineral structure and the supposed analogies of the lower limestone with the mountain limestone of England, have been classed with the old red sandstone. Without attributing any value whatever to these conglomerates, as terms of comparison with English formations, and regarding them only as mineral accidents, the authors place them near the base of the Devonian system, and consequently near the lower limit of the old red sandstone.

In the Eifel, the system is better developed and more fossiliferous, and exhibits the following descending order: (1.) Calcareous sh forming the base of and passing into, the limestone; (2.) Indu shales, alternating with sandstone and flagstone, occasionally reddish colour; (3.) Sandstone, flagstone, arenaceous slate, qu ite, &c., gradually passing into a slate formation. The author fer to various lists of fossils, and conclude that, though several cies are in common with those of the overlying Devonian sy yet that as a group they are distinct: 1st. Because the carbonif species disappear; 2ndly. Because some of the most characte species of the lower limestone (such as the *Strygocephalus Bu* &c.) are wanting; 3rdly. Because new (and Silurian) types l to abound; more especially shales of the genus *Pterinea*, se species of *Orthis*, the *Homalonotus Knightii*, *Calymene Blumenb* &c. They further remark, that the species of Silurian fossils v appear in the Eifel lists are mostly derived from the lower sl which are regarded as beds of passage. Along with known rian fossils there occur also (as remarked in deposits unde Westphalian limestone, Part I.) many other fossils, *Delthyri croptera* and *D. macroptera*, &c., in great abundance. The l quartzo-schistose rocks of Professor Dumont are therefore p in the Silurian system, but without any attempt to subdivide i distinct portions analogous to those of England. And as the no well-defined separation between this system and the over Devonian, still less is there any well-defined separation betwe lower limits and the central slate rocks of the Ardennes.

The slate country of the Ardennes is subdivided into three g of slate rocks,—Upper, Middle, and Lower. All the fossil tained by the authors from the upper group are of Silurian t From the middle and lower groups they obtained no fossils: b all the groups are linked together, and the upper is placed l fossils in the lower part of the Silurian system, they assign th lower groups to the upper Cambrian system. They then ent some mineralogical details connected with the structure of the of the Ardennes; and among the crystalline beds of the l group (which they regard as only an altered portion of that v is next superior), point out some examples of slates derived fr cleavage transverse to the beds, and intersected by a true se cleavage plane, a rare phænomenon among the slates of Eng but noticed by the authors among some rocks on the south co Devon and the north coast of Cornwall.

§ 2. *Formations between the Eifel and the Hundsruck.—Left of the Rhine, &c.*—Crossing the strike of the beds from the Ei the Moselle, by several distinct traverses, the authors met wit same series of deposits in descending order: viz. 1st. Calca shales; 2nd. Arenaceous flagstones and shale; the upper part quently exhibiting a reddish tinge, and with portions more or calcareous, and the lower part passing into a great formati arenaceous flagstone, indurated slate and coarse slate, and occa ally of fine quartzite. The series is here and there highly f ferous, containing several species of Pterinæa, Delthyris, Orthis casionally presenting obscure impressions of plants, and casts of a rge Homalonotus, of a Silurian species. The sequence, deter- ined more by the symmetrical position of the great mineral masses an by direct superposition, as seen in vertical sections, gradually sses into rocks of a more decidedly slaty structure, and almost ithout fossils. Passing to the right bank of the Moselle, and in e same way making traverses through the chain of the Hunds- ck (which is elevated on the line of strike, *i. e.* east-north-east), ey again had an ascending series, and thence concluded that the hole chain was only a portion of the great system under the Eifel nestone in an altered form. The Silurian fossils discovered among e crystalline quartzites and schists of the chain (*e. g.* one or two ecies of Orthis, a large winged Delthyris, &c.), confirmed this ew. Hence also the chain of the Taunus, which is the physical olongation of the Hundsruck, must be referred to a similar place the general series; a conclusion at which the authors also arrived om an examination of the sections on the right bank of the Rhine, ough obscured by the enormous development of masses of con- mporaneous trap (*Schaalstein*).

The authors then give some details respecting the trappean and lcanic rocks on both banks of the Rhine, and conclude, that the artzite and chlorite slates, &c. of the Hundsruck and Taunus are it altered forms of a great Silurian group under the Eifel lime- one; and that the causes which at an ancient epoch dislocated, ntorted, and mineralized the strata, have perhaps not yet entirely ased, and that the hot springs of Wisbaden and bubbling fountains Nassau, may be referred to their last expiring efforts.

On a review of all the facts stated in this and the preceding Part their communication, the authors conclude, (1.) That from the rboniferous deposits of Westphalia and Belgium, to the lowest ssiliferous deposits of the Rhenish provinces, there is a great and interrupted series of formations, which are in general accordance ith the British series, though the subordinate groups do not admit direct comparison; (2.) That, considered in a broad point of ew, the natural successive groups of strata and the natural suc- ssive groups of fossils, are in general accordance; but as the oundaries of the physical groups are ill defined, so also the bound- ies of the fossil groups are ill defined, and pass into or overlap one other; (3.) That as there are no great mineralogical interruptions, oducing a discontinuity and a want of conformity among the de- sits, so also there seems to be no want of continuity among the oups of the great palæozoic series of animal forms. If the ex- eme terms be compared together, all the objects are dissimilar; it if the proximate fossil groups be put side by side, there are any points of resemblance, and many of specific agreement; (4.) hat the Devonian system is, therefore, a natural system, not merely ade out, as in England, by a plausible interpretation of fossil evi- ence, but defined, in the Rhenish provinces, both by its group of ssils and its place in a true descending section. And as the old d sandstone of Herefordshire passes on the one hand into the carboniferous limestone, and on the other into the upper Silurian roc without interruption, it follows, that the Devonian system, as abo defined, is contemporaneous with, and the representative of, this c red sandstone.

§ 3. *Chain of the Hartz.—Fichtelgebirge, &c.*—The general stri of this chain is nearly the same as in the provinces before describ (*i. e.* east-north-east), and therefore almost at right angles to t prevailing direction of the chain, as laid down on a geological ma The mineral structure and the fossils are also nearly the same, a the numerous contortions throw the same difficulties in the way determining the true order of superposition; and these difficult are greatly increased by protruding masses of granite, which ha not only altered the structure of all the neighbouring rocks, l literally broken the chain into fragments, several of which a thrown into a reversed position.

The igneous rocks of the region are stated to be of four kin (1.) Trappean rocks in beds and protruding masses, nearly on t line of strike; (2.) Granite, sending veins into the older slates a trappean rocks; (3.) Quartziferous porphyry in masses and dyk identical in structure, and apparently in relation with the Elvans Cornwall; (4.) Trappean rocks (*melaphyre*, &c.), associated with *rothe todte liegende* and coal-measures on the south-eastern skirts the chain.

Silurian fossils are found in several parts of the Hartz, but the auth saw no rocks which they could compare with the central slates of Ardennes, or the oldest slates of the Rhine; but they give two s tions which ascend into a higher system. The first is from Hu genstein to the neighbourhood of Clausthal, and appears to give following ascending order:

(1.) Devonian limestone, well characterized by its fossils.

(2.) A series of psammites and shales, with one or two species Possidonia.

(3.) A series of coarse sandstones and grits, surmounted shales and psammites highly charged with plants, and minera gically resembling the Devon culm-beds.

Plants are, however, found below the Devonian limestone, a even thin bands of culm; and the section is obscure: but if the terpretation given to it be correct, a part of the country near Cla thal rises into the carboniferous series.

Their next section commences with the limestone of Ebinger (on the south side of the Brocken mountain). The limest abounds with Devonian corals and other fossils, and some parts it cannot be distinguished from the lower limestone of Westpha Other parts of it are pierced with trappean rocks and are overlaid ferriferous deposits; in which respects, as well as in its fossils, it strictly analogous to the Devonian limestones of Dillenburg and Lahn. Again, the ferriferous bands are overlaid by black sh containing *Kiesel schiefer*, and (if we are not misinformed) conta ing Possidonia schist. The analogy presented with the upperm part of the Devonian series in Westphalia seems, therefore, perf

From these facts the authors conclude, that the older rocks of the Hartz are chiefly Silurian and Devonian, with a few traces of the lower carboniferous.

If the great contortions and strike of the Rhenish provinces were produced contemporaneously with those of the Hartz, then must the great derangements of the Hartz have taken place *after* the deposit of the Belgian and Westphalian coal-fields. But the principal dislocations of the Hartz must have taken place *before* the deposit of the red conglomerates, sandstones, coal-beds and trappean masses, which rest on its eastern flank. Hence the authors conclude, that none of these red conglomerates are of the date of the old red sandstone; and that the coal-beds belong to the highest part of the carboniferous series, where it passes into the base of the new red sandstone.

Lastly, The authors describe a hasty traverse, made from the Thuringerwald through the forest of Upper Franconia, and thence to the north flank of the Fichtelgebirge. On the northern limits of the section (the strike and many accidents of position remaining as before) were rocks with a true slaty cleavage, which might (at least mineralogically) be compared with the upper slates of the Ardennes; and further south, the analogy was confirmed by bands of limestone, with stems of Encrinites, but with very few other fossils. Still further south occur a few impressions of plants, and the whole system appears to be at length overlaid by a series of limestones and schists, some of which are very rich in fossils. One of these zones of limestone (the lowest according to Count Munster) rests on calcareous slates, containing a *cardiola* of the upper Ludlow rock. It is in this zone that the Clymenia is most abundant. Goniatites, Orthoceratites, &c., are abundant in a higher zone; and the series is overlaid by a limestone with many species of true carboniferous productæ, &c., and identified with the mountain limestone. From these facts the authors conclude, that the fossiliferous region near Hoff (south of the Fichtelgebirge) belongs to the Devonian system, with the exception of the highest beds, which are carboniferous.

Such are the results arrived at by the authors, which seem to be in general accordance with one another, and to bear out the classification they proposed for the older British formations.

LONDON ELECTRICAL SOCIETY.

April 20, 1841.—The Society held their first meeting of this Session. After the election of members had been announced, the Chairman stated that Walter Hawkins, Esq. had presented the Society with a dried specimen of the *Gymnotus Electricus*. The Secretary read a letter he had received from J. P. Gassiot, Esq., F.R.S., in which was a description of a voltameter containing five pairs of electrodes, so arranged as to enable the experimenter to use one or more pairs at pleasure. A communication was received from George Mackrell, Esq., detailing experiments wherein the five terminal wires of a voltaic battery were ignited at the surface of acid water, forming a part of the circuit. The negative wire was heated to a greater degree than the positive. A paper from Thomas Pollock, Esq. was read, scribing the deflections of the galvanometer, dependent on the cha of colour, &c. produced in certain liquids by the application of to one arm of a V tube containing them. The author conclu that Dr. Black's law of capacity for heat will enable us to exp these electrical phænomena.

These papers will be printed in the "Proceedings" of the Soci the first part of which will appear on the first of July. The Soc then adjourned to Tuesday, May 18th.

CHEMICAL SOCIETY OF LONDON.

On the 23rd of February, 1841, a meeting was held (in the roon the Society of Arts, the use of which had been kindly granted fo purpose,) of gentlemen desirous of uniting themselves for the pose of forming a Chemical Society. At that meeting the follov gentlemen were appointed as a Provisional Committee for carry this object into effect.

Messrs. A. Aikin, W. T. Brande, H. J. Brooke, J. T. Coo J. Cumming, J. F. Daniell, T. Everitt, T. Graham, W. R. Gr H. Hennell, G. Lowe, R. Porrett, R. Warington.

Mr. Warington, by whom the requisite preliminary arrangem had been made, was appointed Secretary to the Provisional C mittee.

A number of gentlemen engaged in the practice or pursuit of mistry having been invited by the Provisional Committee to bec original members, the first meeting of the Society took place, als the rooms of the Society of Arts, on the 30th of March, Profe Graham in the Chair.—The minutes of the previous meeting, on the 23rd of February, having been read and confirmed, the foll ing report of the Provisional Committee was brought up and ad ed, with amendments, for the present government of the Society.

AMENDED REPORT OF THE PROVISIONAL COMMITTEE.

Name and Objects of the Society.—That the Society be designa the CHEMICAL SOCIETY OF LONDON.

That this Society is instituted for the advancement of Chemi and those branches of science immediately connected with it.

For this purpose periodical meetings of its members shall be for the communication and discussion of discoveries and observati relating to such subjects; an account of which shall be published fr time to time, by the Society, in the form of Proceedings or Trans tions.

That the formation of a library of books relating to its pro subjects, of a museum of chemical preparations and standard inst ments, and the establishment of a laboratory of research, are also terior objects of the Society.

Constitution of the Society.—That the Society consist of Ordin Members, Foreign Members, and Associates. The government the Society to be vested in the ordinary members only.

That all persons who have received the circular of the provisio committee inviting them "to join the Society as original members and to support it with their active co-operation," and who have given their assent so to do, on or before the 30th March, 1841, are the first ordinary members of the Society.

That the ordinary members do elect out of their own body, by ballot, a President, four Vice-Presidents, a Treasurer, two Secretaries, and a Council of twelve persons, four of whom may be non-resident, by whom the business of the Society shall be conducted.

That after the appointment of officers and a council, every candidate for admission be proposed according to a form of recommendation agreed upon, subscribed by three members of the Society, to one, at least, of whom, he shall be personally known; and such certificate shall be read and suspended in the Society's rooms or place of meeting for three ordinary meetings.—The method of voting for the election of members to be by ballot.—The ballot to take place at the meeting at which the certificate is read for the fourth time.—The election shall not be valid unless twelve or more members ballot. When three-fourths, or more, of the members balloting, shall be in favour of the candidate, he shall be elected a member; but when fewer than three-fourths of the members balloting, shall be in favour of the candidate, he shall not be elected a member.

That the Annual Contribution to be paid by ordinary members, resident within twenty miles of London, be two pounds, due on each successive 30th of March, and payable in advance for the current year. That for ordinary members resident beyond twenty miles of London, the Annual Contribution shall be one pound.

That no admission fee shall be required of those members who join the Society during the first yearly session.

That Foreign Members and Associates shall be recommended to the Society by the Council, and shall not be required to contribute to the funds of the Society.

That Foreign Members shall be balloted at the next meeting after that at which they are recommended for election.

That Associate Members be elected for a period of three years; that they be thereafter eligible as ordinary members, or may be re-elected associate members.

That the Ordinary Meetings of the Society be held on the Tuesday of every second week, at 8 o'clock in the evening,—from the beginning of November until the end of May.

That the rooms of the Society of Arts, John-street, Adelphi, be the place of meeting for the Society, until more convenient and fitting accommodation can be found.

The following gentlemen were then elected as Officers and Council for the ensuing year:—

President: Professor T. Graham.—*Vice-Presidents*: W. T. Brande, Esq.; J. T. Cooper, Esq.; Professor J. F. Daniell.—*Treasurer*: Arthur Aikin, Esq.—*Secretaries*: E. F. Teschmacher, Esq., Robert Warington, Esq.—*Council*: Dr. T. Clarke; Professor J. Cumming; Dr. C. Daubeny; T. Everitt, Esq.; T. Griffiths, Esq.; W. R. Grove,

Esq.; H. Hennell, Esq.; G. Lowe, Esq.; Professor W. H. Miller
W. H. Pepys, Esq.; R. Porrett, Esq.; Dr. G. O. Rees.

The names of the gentlemen who had become members, i
number seventy-five, were then read.

The Society then adjourned until Tuesday, the 13th of April.

LXV. *Notices respecting New Books.*

A Collection of Letters illustrative of the Progress of Science
England from the Reign of Queen Elizabeth to that of Charles th
Second. Edited by JAMES ORCHARD HALLIWELL, Esq., F.R.S
F.S.A., &c. 8vo. [Printed for the Historical Society of Science.]

MANY of our readers are probably aware that a society has r
cently been formed, on the joint-stock principle of the Camde
and a few other societies, for the purpose of printing documents co
nected with the history of the sciences, and forming by these mea
a medium for the circulation of such information on that subject
could scarcely be given to the world in any other way. The wo
whose title we have given above is the first offspring of this newl
established body; and although it is not distinguished by any ve
striking novelty or the produce of any great change in our previo
knowledge of the history of science at the period to which it refe
yet it not only offers very comprehensive views of the real state
English science in the sixteenth and seventeenth centuries, but al
affords us some very curious biographical information respecting o
men of science during that time. It must above all be remembere
that our only chance for obtaining works of this class is depende
on the present or some similar plan, for there is no public encourag
ment to print them; no publisher of course would undertake the
publication, and the chance of a private person being reimbursed
any sum he may have been hardy enough to have risked in the sa
way is very small. Every lover of such researches will, therefore,
think agree with us in commending any method by which we c
possess ourselves of a series of publications of this nature in an us
ful form, and at a comparatively trifling expense.

The "Historical Society of Science" has commenced its labours
the publication of a miscellaneous collection of letters on scienti
subjects, most of them hitherto inedited, from original manuscri
preserved in various English libraries. They are arranged in chr
nological order, without any regard to the different subjects on whi
they treat. The earliest letter in the volume is one from Eden
chemist, to Lord Burghley, written on August 1st, 1562, in whic
among other twaddle, he describes an operation by which he made
whole quantity of silver! The next is a curious relic of early E
glish science,—a letter from Thomas Digges to Lord Burghley, dat
1574, on astronomical subjects. Then we have mechanical inve
tions by Ralph Rabbards, a long letter from Dr. Dee to Lord Burg
ley on the method of discovering hidden treasures, and one fr

H. Cole to the same nobleman on some "grene ore" found at Richmond. We must not omit to mention, as belonging to this early period, a very curious letter from Tycho Brahe to Thomas Savelle, dated 1590, in which he sends his remembrances to Dr. Dee, "quem in patriam feliciter reversum audivi," and likewise to Thomas Digges. This last-mentioned document is very curious, because it shows that a greater intercourse was in all probability then maintained between the scientific men of this and other countries than is usually admitted to have been the case.

It is unnecessary, however, to continue an enumeration of all the letters here printed, few of which are without their value, and almost all are curious illustrations of the science of the period. We may call the attention of our readers more particularly to a letter from Briggs to Pell at p. 55, which affords a good illustration of the early history of logarithms in this country, and to the correspondence of Sir Charles Cavendish, who was one of the most munificent patrons of science this country has ever possessed. As a specimen of the others, we will here insert the following letter from Mr. Sawtell, a name previously unknown to us in science, addressed to Lilly, the celebrated astrologer, which gives an account of a strange tide at Weymouth, in July, 1666.

"Waymouth, August 6th, 1666.

"Mr. Lilly,—I wrote to you the 18th of the former month concerning the wonderfull motion of the tide as it was carefully here observed for 4 hours time, viz.. July 17th, from about 10 in the morning untill 2 in the afternoone. I also wrote the same to the Post-office in London; notwithstanding which, it was put into the printed intelligence in one manner, and in the Gazette in another manner, and neyther of them aneere the truth, and for what eyther reason or policie I cannot imagine, but made me to be derided here, until I shewed a coppy to many of what I wrote them to London; since which they reply if they print one false that is so wonderfull, how many us believe the rest, &c. Sir, I intreated you by my letter to have given me a few lines in answer that you had received mine, but to this morning I have not received any, which makes me doubt that you have not received mine, or else that you doubted the truth, or that it was not of any worth. If it came not to your hand, pray be pleased by a few lines to give me notice and I shall give you (if you desire it) a very true, sure, and punctuall account of it, or in any particular thing or accident that you shall require of it. If you doubted the truth I'll assure you that to what I wrote you ther is not a title false, I am very confident, viz. that the sea did ebb and flow seven times in four hours time; with the rest of particulars writen you, to which a clowde, as it were, of witnesses will appeare; that there were many more ebbings and flowings it is probable to be true by the report of many; which say that the sea did soe all the morning, and that it did soe likewise againe in the evening, but they say that this was not so violent; but this 4 houres time that I gave you account off was as I have said, carefully observed by so many and that of the chiefest mariners, merchants, and other gentlemen, as well as other sorts of people, both men and weomen, one

friend calling and sending for another, that to me it seemes an
surdity in the least to doubt the truth; the chiefe occation of ob
vation being the extreame violence of it, one wherof I heard b
in a house, being talking with one, we had much pitty for a k
there riding in the road, we thinking of a very great and sud
storme of wind, but looking out of doores found noe wind att alle,
saw the sea at a distance full of ravelling waves with much no
If not of worth to you, pray take the will for the deed. I ayme
the best. The thing to all persons of this town was and sti
wonderfull, and I thought it worthy of publique note, to w
end I sent it to London preferring you before Mr. Gadbury,
otherwise had had the account. However, Sir, if you will not a
me no other account of it, you let me begg you to give me a few l
that you have received mine, that I may know that it came to y
hand; and if by writing so presumptuously to you, strangers to
other, I have offended you, pray pardon me; it is but the se
offence; I hope I then shall not committ the third: yet in the m
while shall remaine, Sir,

"Your friend in what I may,
"CHRISTOPHER SAWTEL

"*For Mr. William Lilly, Astrologer, at the*
corner-house, over against Strand-bridge,
these in London. Post paid, 3d."

Independent of its more weighty and important attractions, t
is much in the volume that will be found to be interesting to
general reader; and, what is very unusual with works of a scie
nature, some parts of it are really entertaining. It only remain
us to say, that the editor appears to have performed his task
fully and judiciously.

LXVI. *Intelligence and Miscellaneous Articles.*

ORTHODOX GEOGRAPHY.

"*Azimghur School.*—The Local Committee brought to our n
a prejudice which existed amongst some of the Hindus at this p
An unwillingness existed amongst some of the pupils to receive
graphical instruction according to the English system, from an
that the Christian, as well as the Hindu religion, had certain *pe*
geographical tenets.

"We recommended to the Local Committee to act with cau
until the jealousy should subside, which we doubted not it
would, and we suggested that geographical instruction shou
given orally with the aid of maps. We instanced the readiness
which geography had been studied in the Sanscrit College fro
English books, and pointed out that learned Brahmins did not
tate to study the Hindu astronomy, according to the Sidhantas, th
they are as opposed to the conceits of the Puranas as is Euro
geography."—*Report of the Committee of Public Instruction o*
Presidency of Fort William, Bengal.

CUMYLE—OIL OF CUMMIN.

The following are some additional particulars relative to the substances obtained from oil of cummin, by MM. Gerhardt and Cahours, referred to in our last, p. 282.

Cumyle (unknown radicle)........ $C^{40}\ H^{22}\ O^{2}$
Hydruret of cumyle (oil of cummin) $C^{40}\ H^{22}\ O^{2} + H^{2}$

If the hydruret of cumyle be gradually dropped on fused hydra of potash, hydrogen is evolved, and every drop is converted into te minate of potash. The effect is so rapidly produced, that by this method two pounds of cuminate of potash may be obtained in an hour.

The analysis of the product obtained by distilling cuminic acid with excess of barytes, and the density of its vapour, lead to the formula $C^{36}\ H^{24}$, which represents four volumes of it. It is therefore formed in the same manner as benzen. In fact, we have $C^{40}\ H^{24}\ O^{4} - C^{4}\ O^{4} = C^{36}\ B^{24}$, the alkaline base employed effecting the separation of carbonic acid.

This product, to which the authors have given the name of *cumen*, possesses striking analogies with benzen: thus it dissolves in Nordhausen sulphuric acid, and produces an acid corresponding to the sulphobenzidic acid of Mitscherlich.—*L'Institut*, No. 389.

METEOROLOGICAL OBSERVATIONS FOR MARCH 1841.

Chiswick.—March 1. Cloudy. 2. Frosty: rain. 3. Fine: cloudy. 4. Clear and fine: rain. 5. Overcast: slight rain. 6. Clear and very fine. 7, 8. Very fine. 9—12. Foggy in the morning: very fine throughout the day: evening clear. 13. Slight haze: foggy. 14. Foggy: very fine: dense fog at night. 15, 16. Foggy: very fine. 17. Cloudy and showery. 18. Cloudy. 19. Overcast: showery. 20. Fine: stormy with rain. 21. Very fine: slight rain at night. 22. Rain: fine. 23. Fine. 24—26. Very fine. 27. Showery: clear. 28—30. Cloudy and fine. 31. Clear: fine but windy: rain at night.

Boston.—March 1. Cloudy: rain P.M. 2. Fine: rain P.M. 3, 4. Fine. 5. Fine: rain A.M. and P.M. 6. Fine. 7. Fine: beautiful morning. 8. Foggy. 9—13. Fine. 14. Cloudy. 15. Fine: three o'clock P.M. thermometer 65°. 16. Fine: two o'clock P.M. thermometer 65°. 17. Cloudy. 18—21. Windy. 22. Rain: 23. Windy. 24, 25. Fine. 26. Fine: rain P.M. 27. Cloudy. 28. Fine. 29. Cloudy: rain early A.M.: rain P.M. 30. Fine: rain P.M. 31. Fine.

N.B. This is the warmest month of March since March 1830.

Applegarth Manse, Dumfries-shire.—March 1. Cold and clear: snow on hills melting. 2. Rain all day. 3. Slight frost. 4. Frost harder: snow. 5. Heavy rain: snow gone. 6. Fine A.M.: rain P.M. 7. Fine throughout. 8. Fine spring day: rain A.M. 9. Fine spring day: fair. 10. Growing day: fog P.M. 11. Beautiful day. 12. Fine A.M.: raw fog P.M. 13, 14. Fine throughout. 15. Fog A.M.: cleared up. 16. Fine throughout. 17, 18. Fine A.M.: rain P.M. 19. Rain and hail. 20. Heavy showers. 21, 22. Rain P.M. 23. Cloudy and threatening. 24, 25. Showery and foggy. 26. Showers A.M.: cleared and fine. 27. Showers A.M. 28, 29. Showers P.M. 30. Fair A.M.: rain and wind P.M. 31. Boisterous and wet.

Sun shone out 23 days. Rain fell 18 days. Frost 2 days. Snow 1 day. Fog 3 days.

Wind East 1 day. South-east 4 days. South-south-east 1 day. South 4 days. South-south-west 1 day. South-west 13 days. West-south-west 2 days. West 3 days. North-north-west 1 day.

Variable 1 day. Calm 11 days. Moderate 8 days. Brisk 5 days. Strong breeze 5 days. Boisterous 2 days.

Mean temperature of the month	44°·07
Mean temperature of March 1840	39 ·35
Mean temperature of spring-water	45 ·60
Mean temperature of spring-water, March 1840	42 ·60

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. March.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8¾ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·614	29·693	29·585	29·24	29·46	29·57	39·3	42·6	36·9	41	27	37	45	33	SE.	S.	calm	NNW.	...	·10	...	...	36
2.	29·688	29·685	29·393	29·30	29·35	29·10	37·4	43·2	35·0	49	37	34	45	32	SW.	SW.	W.	SW.	·025	·30	·08	...	37
3.	29·324	29·665	29·315	28·89	29·24	29·63	42·4	46·7	37·3	50	29	42	45	37½	NW.	SW.	W.	SW.	·283	·02	·23	...	39
4.	29·912	29·904	29·818	29·48	29·75	29·60	36·8	48·3	35·2	46	33	36	41	26½	NW.	W.	NW.	SW.	...	·08	...	...	36
5.	29·876	29·857	29·299	29·39	29·42	29·10	42·0	45·4	36·2	50	36	39	41½	32	S.	S.	calm	SW.	·083	·22	...	...	40
6.	29·764	29·960	29·723	29·26	29·58	29·68	43·2	50·3	40·5	52	41	41	48½	33½	NW.	NW.	calm	SW.	·158	...	·32	...	41
○ 7.	30·080	30·255	30·047	29·53	29·77	30·12	51·7	52·5	42·6	61	42	49	51	41	W.	SW.	calm	W.	...	...	...	0·77	43
8.	30·414	30·369	30·355	29·85	30·18	30·24	50·5	59·2	49·6	61	35	46	52½	41	SW.	SW.	calm	SW.	...	...	...	...	45
9.	30·444	30·392	30·385	29·90	30·21	30·24	44·5	60·3	43·4	60	30	44	51	38½	SSW.	SW.	calm	SW.	...	...	...	...	46
10.	30·490	30·462	30·426	29·93	30·30	30·35	46·7	56·5	43·3	63	27	44	49	41½	S.	S.	calm	WSW.	...	...	...	...	45
11.	30·512	30·473	30·401	29·97	30·33	30·27	45·2	58·7	40·0	64	30	44	56	39	S.	SE.	calm	var.	...	...	...	...	44
12.	30·400	30·366	30·273	29·85	30·25	30·28	46·6	59·3	42·2	67	31	41·5	53½	34½	E.	SW.	S.	SE.	...	...	...	...	46
13.	30·398	30·376	30·331	29·87	30·30	30·30	46·6	61·0	44·6	59	37	46	56	35	N.	E.	E.	SE.	...	...	...	...	47
14.	30·388	30·351	30·192	29·94	30·18	30·11	40·2	56·7	37·7	57	33	36·5	53½	37	E.	W.	calm	SW.	...	...	...	0·04	45
☾ 15.	30·176	30·152	30·005	29·70	30·04	29·94	42·8	53·7	39·6	66	30	43	56½	42½	NE.	W.	W.	W.	...	...	...	...	40
16.	29·942	29·921	29·729	29·40	29·75	29·48	49·7	60·6	41·6	65	35	53	62	41½	S.	S.	W.	SE.	...	...	...	...	47
17.	29·630	29·603	29·577	29·08	29·35	29·30	51·3	61·7	49·2	56	41	51·5	49	47	S.	S.	S.	SW.	...	·01	...	...	49
18.	29·444	29·525	29·404	28·98	29·11	29·15	51·7	56·7	46·8	56	40	49	52	43	E var.	SW.	S.	SW.	...	...	...	...	48
19.	29·628	29·713	29·598	29·14	29·20	29·25	49·7	56·0	44·2	54	34	46·5	51½	43	SSW var.	SW.	S.	WSW.	...	·01	...	...	48
20.	30·650	29·647	29·565	29·24	29·39	29·32	50·3	54·0	44·3	57	39	47	50	39½	E.	SW.	SE.	E.	·022	·13	...	...	46
21.	29·672	29·625	29·482	29·22	29·32	29·14	49·5	56·4	43·6	54	48	48	51	39	S.	S.	SSW.	S.	·133	·12	...	1·20	46
22.	29·378	29·596	29·362	28·84	29·01	29·27	51·9	54·3	48·7	58	36	52	50	45	S var.	SW.	W.	S.	·036	·02	·12	...	50
● 23.	29·878	30·128	29·803	29·29	29·44	29·78	49·3	56·6	43·2	59	43	47	50½	39	SSW.	W.	W.	SW.	·025	...	...	...	48
24.	30·270	30·222	29·192	29·70	29·94	29·94	51·4	58·3	46·6	58	30	50	48½	44½	SE.	SW.	W.	SW.	...	...	...	...	46
25.	30·160	30·123	29·952	29·60	29·85	29·70	51·8	55·7	44·3	61	32	47	48½	44½	SSE.	S.	calm	S.	...	...	...	...	45
26.	29·728	29·698	29·593	29·21	29·55	29·40	55·5	59·0	47·0	65	39	52	48½	42	S.	S.	calm	SE.	...	·02	...	...	49
27.	29·778	29·863	29·721	29·21	29·48	29·63	51·5	64·2	45·0	58	27	48	52	39½	S.	SW.	calm	W.	·036	·08	·26	...	48
28.	29·970	29·914	29·897	29·43	29·60	29·65	45·0	59·7	41·2	54	40	46	47	43	SSW.	S.	SW.	SSW.	·105	...	...	1·36	44
29.	29·938	29·922	29·787	29·37	29·66	29·47	51·2	56·6	44·8	58	40	51·5	53	42½	S.	SW.	calm	SSE.	...	·10	·02	...	48
☽ 30.	29·912	29·915	29·836	29·34	29·60	29·35	48·2	57·0	43·0	55	41	47	49	39½	W.	W.	calm	S.	·072	·08	·10	...	48
31.	29·678	29·638	29·497	29·06	29·02	29·20	49·0	55·0	44·7	56	41	47	49	40	S var.	W.	W.	SW.	·069	·03	·07	...	46
Mean.	29·940	29·968	29·856	29·42	29·665	29·753	47·1	55·4	42·7	57·09	35·61	45·3	50·1	39·2					Sum. 1·047	1·32	1·20	3·37	Mean. 45

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

JUNE 1841.

LXVII. *On some of the Products of the Action of Nitric Acid on Castor Oil.* *By* THOMAS GEORGE TILLEY[*].

THE action of nitric acid on the fat acids is a subject that has been lately investigated by Laurent[†] and Bromeis[‡], and the interesting results obtained by them having called the attention of chemists to the subject, I was induced, at the suggestion of Professor Liebig, to examine in his laboratory the products of the oxidation of the oil of *Ricinus Communis*, differing as it does in such a remarkable degree from the other fixed oils.

One part of this oil was accordingly mixed with twice its weight of nitric acid diluted with an equal bulk of water, and subjected to a gentle heat in a retort. The parts which distilled over were preserved. After some time the action became very violent, and gas was formed in such quantities as to expel the contents of the retort forcibly from its mouth. The retort must then be removed from the fire, when the action gradually subsides. When again placed on the fire, protected by a sand-bath, the action is not so violent. The process of oxidation must be continued for some days, more or less, according to the strength of the nitric acid employed. When the quantity of nitrous acid fumes decreases, the retort is removed from the fire. In the receiver are found nitric acid, water, and a peculiar volatile oily acid, which new compound will form the subject of the following pages. If the

---

[*] Read before the Chemical Society, April 27, 1841. Communicated by the Society.

[†] *Ann. de Chim. et de Phys.*, lxvi.

[‡] *Annalen der Pharmacie*, xxxv. p. 86. [Noticed in L., E. and D. Phil. Mag. for February, pres. vol. p. 115.]

fatty mass which remains in the retort be mixed with w
and distilled, an additional quantity of the acid will be
tained.

It must be separated from the acid fluid on which it fl
mixed with water and redistilled, which process must be
peated several times. After this it must be dried by stan
over melted phosphoric acid, chloride of calcium not b
adapted to the purpose, it being a little soluble in the aci

The acid procured in this way is perfectly colourless
transparent, having an agreeable aromatic smell and a s
stimulating taste. It is sparingly soluble in water, impa
to that fluid its peculiar smell. It is soluble in nitric
alcohol and æther. When raised to the temperature of 148
it begins to boil, and a small part distils over; but if kep
some time at that temperature, it becomes black and is
composed, giving off empyreumatic products, so that it ca
be distilled alone. It burns with a clear flame, giving
smoke. It does not become solid though cooled to — 17
To this body I have given the name of *Œnanthylic Acid*,
reasons which will be stated in another part.

To determine its composition the acid was burned
oxide of copper.

1st. 0·3305 substance gave 0·7810 carbonic acid, and 0·
water.

2nd. 0·4295 substance gave 0·9905 carbonic acid, and 0·
water.

This gives in 100 parts,

	1st.	2nd.
Carbon	65·34	65·33
Hydrogen......	10·83	10·60
Oxygen.........	23·83	24·07
	100·00	100·00

Œnanthylic Æther.—This æther is formed by dissol
the acid in strong alcohol, and passing a stream of hy
chloric acid gas through the solution. To the mixtu
then added carbonate of potash, in order to neutralize all
acid; it is then distilled. The æther passes over into th
ceiver, and may be freed from any alcohol that it may co
by agitation with water. It must, lastly, be distilled over
ride of calcium in a stream of carbonic acid gas, as it is
composed by the oxygen of atmospheric air, at the temp
ture of its boiling point.

The œnanthylic æther so obtained is a colourless
lighter than water, having a peculiar and agreeable smel
unlike that of nitrobenzide. It has a sweet and some

pungent taste, leaving a disagreeable sensation on the palate. It is soluble in æther and alcohol, and burns with a clear blue flame, giving out no smoke. It is solid at the temperature produced by a mixture of snow and salt, and is then crystalline.

Burnt with oxide of copper, 0·2195 substance gave 0·5485 carbonic acid, and 0·3195 water; making in 100 parts,

Carbon	68·57	
Hydrogen	11·57	
Oxygen	19·86	
		100·00

and leading to the following composition:

			Per cent.
18 atoms	Carbon	= 1375·80	68·71
18 „	Hydrogen	= 224·63	11·22
4 „	Oxygen	= 400·00	20·07
		2000·43	100·00

Now supposing this æther to be composed of one atom oxide of æthyl and one atom of anhydrous acid, we have for the composition of the latter $C^{14} H^{13} O^{3}$, and adding to this one atom of water to form the hydrate, we have, in 100 parts,

			Per cent.
14 atoms	Carbon	= 1070·09	65·05
14 „	Hydrogen	= 174·71	10·62
4 „	Oxygen	= 400·00	24·38
		1644·80	100·00

which calculated result agrees with the numbers found by actual experiment given at page 418.

Œnanthylate of Silver.—This salt may be easily prepared by precipitating a neutral ammoniacal solution of the acid by nitrate of silver; it falls in the form of a white flocculent powder.

1st. 0·7165 of this salt gave ·3265 silver, which is equivalent to 0·3509 oxide of silver. This gives per cent.,

Oxide of silver	48·89	
Œnanthylic acid	51·11	
		100·00

making the atomic weight of the anhydrous acid 1517·0, the calculated result being 1532·33.

By burning the salt dried *in vacuo* over sulphuric acid, with oxide of copper, 0·7350 gave 0·9360 carbonic acid, and 0·3675 water.

2nd. 0·8785 gave 1·1300 carbonic acid, and 0·4505 water. This gives in 100 parts,

	1st.	2nd.
Carbon	35·20	35·56
Hydrogen	5·55	5·68
Oxygen	10·27	9·78
Oxide of silver	48·89	48·98
	100	100

and leads to the following theoretical composition:

		Per cent.
14 atoms Carbon	1070·090	35·86
13 ,, Hydrogen	162·233	5·43
3 ,, Oxygen	300·000	10·07
1 ,, Oxide of silver	1451·61	48·64
Atomic weight	2983·384	100·00

The formula for this salt is then $C^{14} H^{13} O^{3} + Ag O$.

Œnanthylate of Baryta.—This salt is formed by boiling carbonate of baryta with an alcoholic solution of œnanthylic acid till the fluid no longer possesses acid reaction. The solution must be filtered while hot, and on cooling deposits the salt in beautiful pearly scales, which are insoluble in æther, but soluble in water and alcohol.

0·3520 of this salt gave 0·1735 carbonate of baryta, or 0·1346 baryta, the atomic weight of the acid, calculated from this, being 1545·54. It gives in 100 parts,

38·23	oxide of barium
61·77	œnanthylic acid
100·00	

1st. 0·4395 of the same salt, burned with oxide of copper, gave 0·6645 carbonic acid, and 0·2740 water.

2nd. 0·3460 salt gave 0·5200 carbonic acid, and 2190 water.

After adding to the carbon that which remained, combined as carbonic acid, with the baryta in the combustion tube, we obtain in 100 parts,

Carbon	44·84	44·58
Hydrogen	6·91	6·87
Oxygen	10·02	10·32
Baryta	38·23	38·23
	100·00	100·00

which gives the following theoretical numbers:

			Per cent.
14 atoms	Carbon..........	=1070·09	42·98
13 „	Hydrogen.......	= 162·23	6·51
3 „	Oxygen..........	= 300·00	12·07
1 „	Oxide of barium	= 956·88	38·44
		2479·20	100·00

It will be seen by the preceding numbers that an excess of carbon is found; but this may be accounted for by the high temperature required to burn the last particles of carbon, by which some of the carbonic acid might have been driven off from the carbonate of baryta in the combustion tube.

The formula for this salt is one atom œnanthylic acid and one atom oxide of barium, or $C^{14} H^{13} O^{3} + Ba O$.

Œnanthylate of Potash is obtained by neutralizing the carbonate of that alkali by œnanthylic acid; it does not crystallize, but, on evaporation, assumes the form of a thick transparent jelly.

The *Copper Salt* crystallizes in beautiful needles of a rich green colour, soluble in alcohol, and sparingly so in water.

The *Œnanthylate of Strontian* appears in the form of bright pearly scales, very much resembling the analogous salt of baryta.

It will at once be perceived by the analyses which have been given, that the composition of anhydrous œnanthylic acid is $C^{14} H^{13} O^{3}$, and it enters as such into the following combinations:

Œnanthylate of water......... $C^{14} H^{13} O^{3} + H^{2} O$
„ oxide of æthyl. $C^{14} H^{13} O^{3} + C^{4} H^{5} O$
„ „ barium $C^{14} H^{13} O^{3} + Ba O$
„ „ silver . $C^{14} H^{13} O^{3} + Ag O$.

Now it was discovered some time ago that wine owes its peculiar smell to a certain acid in combination with oxide of æthyl. To this acid Liebig and Pelouze, its discoverers, gave the name œnanthic acid (flower of wine). Œnanthic acid has the following composition, $C^{14} H^{13} O^{2} + aq$.

The analogy between this acid and the one described in this paper will at once be seen, and the composition of the two acids would lead us to suppose that they are oxides of the same radical, which may for the present be considered as composed of $C^{14} H^{13}$; while the two acids are respectively

$$R + 2O$$
$$R + 3O.$$

On these grounds I have given the higher oxide the name *Œnanthylic acid*, and propose for the lower oxide, that of

Liebig and Pelouze*, the name *Œnanthylous acid.* It is improbable that œnanthylic acid may be identical with azoleinic acid of Laurent†; but he did not obtain it in a st of purity. When the œnanthylate of silver is distilled, th pass into the receiver an oil and a solid body, neither of wh possesses acid properties. The solid body is soluble in alcohol, and on being allowed to cool, it crystallizes in be tiful needles.

Suberic acid is another product of the oxidation of cas oil; it remains in the retort mixed with oxalic acid: it m be purified by repeated crystallizations, and boiling with ni acid.

Thus obtained and dried at 100° C., 0·2710 substance g ·5490 carbonic acid, and 0·1990 water, or, in 100 parts,

	Found.	Atoms.	Calculated.
Carbon . . .	55·97	8	55·64
Hydrogen .	8·15	7	8·03
Oxygen . . .	35·78	4	36·33
	100·00		100·00

giving the formula $C^8 H^6 O^3 + aq.$

The other acids found by Laurent did not appear to h been formed; but this is not affirmed with positive certai as the examination was not proceeded with. A trace of lip acid, however, may be obtained during the evaporation of fluid parts, pressed from the suberic acid.

LXVIII. *On Bleaching Salts.* *By* M. Detmer, *Esq.*‡

A SHORT time ago a notice was published by M. Mi on the Bleaching Salts of Chlorine, in which a new v was offered of the constitution of these compounds. They h for some time past generally been considered as compou or mixtures of a metallic chloride with a hypochlorite metallic oxide; bleaching powder or the chloride of lime, instance, as consisting of chloride of calcium and hypochlo of lime, in single equivalents, the acid of the last salt cont ing one atom of oxygen to one atom of chlorine. The r tion of chlorine upon lime supposed, may be very sim stated: two atoms of lime take up two of chlorine; one a only of the lime is decomposed, of which the calcium oxygen respectively unite with an atom each of chlorine, fo ing chloride of calcium and hypochlorous acid. The hypochlorous acid combines with the other atom of lime.

---

* *Ann. de Chim. et de Phys.*, t. lxiii. 118.

† *Ann. de Chim. et de Phys.*, t. lxvi. 172.

‡ Read before the Chemical Society, April 27, 1841. Communic by the Society.

Starting from the composition of chlorochromic and chlorosulphuric acids, which are represented by Walter and Regnault as chromic and sulphuric acids in which the third proportion of oxygen is replaced by chlorine ($Cr\,O_2 + Cl$ and $S\,O_2 + Cl$), Millon supposes that the bleaching chlorides have a similar relation to the peroxides of their metals. The peroxide of calcium being $Ca\,O_2$, or $Ca\,O + O$, bleaching powder is $Ca\,O + Cl$, or the peroxide of calcium, with chlorine substituted for its second proportion of oxygen. In support of this view Millon adduces observations of his own on the composition of the bleaching compounds of chlorine with different metallic oxides, such as oxides of lead and protoxide of iron, as well as potash, soda and lime, in which the proportion of chlorine was found to vary, but to correspond with the excess of oxygen above one equivalent in the peroxides of the same metals. In particular, potash was found to absorb two equivalents of chlorine, and soda only one, the peroxide of potassium being $K\,O + 2\,O$, while the peroxide of sodium is $Na\,O + O$.

The attention of the author was particularly directed to ascertain the accuracy of the latter statement. A solution of carbonate of soda was charged with chlorine gas till it acquired a yellow colour and retained not a trace of carbonic acid. The solution was then briskly agitated with air, by which the excess of free chlorine escaped. In analyzing the solution afterwards, one portion of it was treated with a few drops of ammonia, and the chlorine afterwards precipitated by nitrate of silver; another portion was evaporated to dryness for the sodium, which was obtained in the state of chloride of sodium.

In four experiments the liquids charged with chlorine contained chlorine and sodium in the following proportions, in 100 parts:—

Sodium	47·88	45·26	46·81	44·76
Chlorine	52·12	54·74	53·19	55·24,

while, if the bleaching chloride of soda contained 1 eq. of chlorine to 1 eq. of soda, its composition would be

1 eq. sodium	46·91
1 eq. chlorine	53·09
	100·00

The results correspond as closely as could be expected with this theoretical statement. There can be no doubt then that the chloride of soda contains one of chlorine to one of soda. This is the result required by Millon's theory, the peroxide of sodium containing, according to him, one of oxygen and one of soda; but it is equally consistent with Balard's theory that the salt is a mixture of single equivalents of chloride of sodium and hypochlorite of soda. To determine the quantity of chlorine which water dissolves, a stream of the gas was sent through water at 59° for five hours. One hundred grammes of water were found to take up 0·663 gramme of chlorine; or 200 cubic inches of water dissolved 207 cubic inches of gas. The chlorine was estimated by converting it into hydrochloric acid by the addition of a few drops of ammonia, slightly acidulating afterwards by nitric acid, and precipitating by nitrate of silver. A solution of 2·58 chloride of potassium in 38·96 water was found to dissolve less chlorine than pure water, in the proportion of 180 to 257. Chlorine gas being allowed to stream through a solution of 9·245 grammes carbonate of potash in 96·495 grammes of water, till saturation, the solution lost all its carbonic acid and took up 6·631 grammes of chlorine. Here 1 eq. of potash = 590 has taken up 656 chlorine, which is very nearly $1\frac{1}{2}$ eq. of chlorine = 663. But when the quantity of free chlorine in the liquid is deducted, the latter is found to contain only 1·34 equivalents of chlorine to 1 eq. of potash. In two other experiments, in which the liquid was agitated with air after being saturated with chlorine, to allow the excess of gas to escape, there were found to 1 eq. of potash 1·44 and 1·43 equivalents of chlorine. The carbonate of potash, therefore, without doubt, takes up more than a single equivalent of chlorine. But the quantity of chlorine combined with the potash is still greatly short of two equivalents, the proportion required by M. Millon's theory; the peroxide of potassium containing two oxygen to one potash, or $K\,O_3$. The conclusion therefore is inadmissible, that the chloride of potash is analogous in constitution to the peroxide of potassium.

It remains to account for the property which potash is found to possess of taking up more chlorine than is necessary to convert it into chloride of potassium and hypochlorite of potash. On transmitting chlorine through carbonate of potash, a stage in the absorption is very observable, at which the liquid becomes all at once of a yellow colour. This happens when what remains of the potash is entirely converted into bicarbonate of potash. The suddenness of the appearance of the yellow colour appears to be due to a reaction of the carbonic acid upon the hypochlorite of potash in solution, by which hypochlorous acid is set free and tinges the liquid. By the continued application of chlorine to the bicarbonate of potash, it is converted into a mixture of chloride of potassium, hypochlorite of potash, and free hypochlorous acid. By the ultimate action of the chlorine all the bicarbonate of potash is decomposed, the carbonic acid entirely expelled, and a portion of hypochlorous acid remains free in solution.

This formation of free hypochlorous acid does not occur with carbonate of soda, owing to the much weaker affinity which that base has for carbonic acid, and its forming a much less stable bicarbonate than potash does. The free carbonic acid cannot therefore react upon the hypochlorite of soda, and liberate hypochlorous acid as the free carbonic acid does upon the hypochlorite of potash. The same formation of free hypochlorous acid occurs in a more striking degree when chlorine is sent through a solution of acetate of potash; that solution, it is well known, absorbs a large quantity of gas, and acquires the strong yellow colour, the odour, and all the other properties of free hypochlorous acid. It is here evident, that by the action of chlorine upon acetate of potash, chloride of potassium is formed, with the binacetate of potash, free hypochlorous acid, and the hypochlorite of potash. If the large absorption of chlorine by carbonate of potash is due to carbonic acid, it follows that caustic potash should not absorb any excess of chlorine, but that the property should be confined to the carbonate. Accordingly, in two experiments, the proportion of chlorine absorbed by caustic potash was found to be as nearly as possible a single equivalent. In one experiment 449·1 chlorine, in the other 424·8 chlorine were taken up, instead of 442·6 chlorine, by a single equivalent or 589·9 of potash. Caustic potash, therefore, dissolves no more chlorine than caustic soda. There appears therefore to be no reason to abandon the old theory, that the bleaching solutions of chlorine in alkalies and alkaline earths contain a chloride and hypochlorite, for these bleaching compounds certainly do not correspond with metallic peroxides, as has been lately maintained.

LXIX. *On a linear Method of Eliminating between double, treble, and other Systems of Algebraic Equations.* *By* J. J. Sylvester, *F.R.S., Professor of Natural Philosophy in University College, London.*

Part I.—Binary Systems.

Let U and V be two integer complete homogeneous functions of x and y, one of the mth, the other of the nth degree; and let it be required to express the condition of the coexistence of the two equations $U = 0$ $V = 0$ by means of the equation $C = 0$, where C is free from all appearances of x or y.

This equation, according to the system of notation developed in a preceding paper, and which has been since adopted and sanctioned by the high authority of M. Cauchy, I call the final derivative: the quantity C is designated the final derivee: and it is our present object to show how this may be obtained in a *prime* form, that is to say, divested of irrelevant factors: in this state it must consist of terms, each containing $m + n$ letters, of which n belong to the coefficients of V, and m to those of U.

Of course in applying this rule it is to be understood that every combination of powers in U or V has a single letter prefixed for its coefficient, and that in the final derivee powers are represented by repetitions of the same character.

Every term in U or V being of the form $C\,x^p . y^q$, $x^p . y^q$ is called an argument, c its prefix.

Assume two integer positive numbers r and r', and also two others s and s', such that $r + r' = n - 1$ $s + s' = m - 1$, and form from $U = 0$ $V = 0$ two new equations,

$$x^r . y^{r'} . U = 0 \quad x^s . y^{s'} . V = 0.$$

Such equations are termed the Augmentatives of the two given ones respectively; also $x^r . y^{r'}$. U and its fellow are termed the Augmentees of U and V.

r and r' are termed the indices of augmentation belonging to U, s and s' the same belonging to V.

Finally, it will be useful hereafter to call the given polynomials U and V themselves the proposees, and the given equations which assert their nullity, the propositive equations, or, briefly, the propositives.

Now as many augmentees of either proposee can be formed as there are ways of stowing away between two lockers (vacancies admissible) a number of things equal to the index of the other*; hence we shall have n augmentees of U, and m of V: thus there will be $m + n$ augmentatives each of the degree $m + n - 1$, and the number of arguments is clearly $m + n$ also, so that they can be eliminated linearly, and the final derivee thus found, containing $m + n$ letters (properly aggregated) in each term, will be in its prime form, that is, incapable of further reduction, and void of irrelevant factors.

It is worthy of remark, that the final derivee obtained by arranging in square battalion the prefixes of the augmentees, permuting the rows or columns, and reading off diagonal products, affected each with the proper sign (according to the well known rule of Duality), will not only be free from factorial irrelevancy, but also of linear redundancy, which latter term I use to signify the reappearance of the same combination of prefixes, sometimes with positive and sometimes with negative signs: furthermore, it follows obviously from the nature of the process that no numerical quantity in the final derivee will be greater than the higher of the indices of the two given polynomials.

---

* "Tot Augmenta utriusvis ex æquationibus propositis formari possunt quot modi sint inter duo receptacula (utrivis vel ambobus omnino vacare licet) rerum, quarum numerus indicem alterius æquat, distributionem faciendi."

Part II.—TERNARY SYSTEMS.

CASE A.—Indices all equal.

Method 1.

Let there be now three proposees, U, V, W, integer complete homogeneous functions of x, y, z, each of the degree n: let $r + r' + r'' = n - 1$, $s + s' + s'' = n - 1$, $t + t' + t'' = n - 1$,

$$x^r . y^{r'} . z^{r''} . \mathrm{U}, \quad x^s . y^{s'} . z^{s''} . \mathrm{V}, \quad x^t . y^{t'} . z^{t''} . \mathrm{W}$$

will, as above, be called the augmentees of U, V, W, and every other part of the notation previously described is to be preserved.

$$\text{Suppose now } \mathrm{U} = 0, \ \mathrm{V} = 0, \ \mathrm{W} = 0,$$

we shall have as many augmentative equations formed from each proposee as there are ways of stowing away n things between *three* lockers (vacancies admissible)*, i. e. $n . \frac{n+1}{2}$ of each kind; in all, therefore, $3 . \frac{n . n + 1}{2}$, and every one of these will be of the degree $2n - 1$, so that the number of arguments to be eliminated is equal to the number of ways of stowing away $2n - 1$ things between three lockers (empty ones counting), i. e. $\frac{2n . (2n + 1)}{2}$.

As yet, then, we have not *enough* equations for eliminating these linearly.

Make, however, $\alpha + \beta + \gamma = n + 1$,

and write $\mathrm{U} = x^\alpha . \mathrm{F} + y^\beta . \mathrm{F}' + z^\gamma . \mathrm{F}'' = 0$,

$$\mathrm{V} = x^\alpha . \mathrm{G} + y^\beta . \mathrm{G}' + z^\gamma . \mathrm{G}'' = 0,$$

$$\mathrm{W} = x^\alpha . \mathrm{H} + y^\beta . \mathrm{H}' + z^\gamma . \mathrm{H}'' = 0,$$

it will always be possible to make the multipliers of x^α, y^β, z^γ integer functions: for if we look to any argument in U, V, or W, it is of the form $x^a . y^b . z^c$, and one of the letters a, b, c must be *not less* than its correspondent α, β, γ, for otherwise $a + b + c$ would be not greater than $\alpha + \beta + \gamma - 3$, i. e. n would be not greater than $(n + 1) - 3$, or $n - 2$, which is absurd: if now any one, as (a), be equal to or greater than α, it may be made to supply an integer part to the multiplier of x^{α}.

---

* See for Latin translation the preceding note.

Here it may be asked what is to be done with such terms as $K . x^{a} . y^{b} . z^{c}$, when two letters a, b are each not less than their correspondents α, β: the answer is, such terms may be made to enter under the multiplier of x^{α}, or of x^{β}, or to supply a part to both in any proportion at pleasure*.

From the equations above we get, by linear elimination,

$$F . G' . H'' + G . H' . F'' + H F' . G''$$
$$- G . F' . H'' - H . G' . F'' - F . H' . G'' = 0.$$

This may be denoted thus: $\Pi(\alpha, \beta, \gamma) = 0$, which equation I call a *secondary* derivative, and the left side of it a *secondary* derivee; α, β, γ may likewise be termed the indices of derivation (as r, s, t, etc. are of augmentation).

Now since $\alpha + \beta + \gamma = n + 1$, it is clear then the index of $\Pi(\alpha, \beta, \gamma)$ is always $n + n + n - (n + 1)$; i.e. $2n - 1$.

1st. Let any two of the indices of derivation be taken zero, then it is easily seen that all the terms in $\Pi(\alpha, \beta, \gamma)$ vanish, and consequently the secondary derivative equations obtained upon this hypothesis become mere identities, and are of no use.

2nd. Let any *one* of them become zero.

It is manifest, from the doctrine of simple equations, that

$$\Pi(\alpha, \beta, \gamma) \text{ may be made equal to} \left\{ \lambda . U + \mu . V + \nu . W \right\} . \frac{1}{x^{\alpha}},$$
$$\text{or} \left\{ \lambda' . U + \mu' . V + \nu' . W \right\} . \frac{1}{x^{\beta}},$$
$$\text{or} \left\{ \lambda'' . U + \mu'' . V + \nu'' . W \right\} . \frac{1}{x^{\gamma}},$$

upon the understanding that

$\lambda = G' . H'' - G'' . H'$, $\mu = H' . F'' - H'' . F'$, $\nu = F' . G'' - F'' . G'$,
$\lambda' = G'' . H - G . H''$, $\mu' = H'' . F - H . F''$, $\nu' = F'' . G - F . G''$,
$\lambda'' = G . H' - G' . H$, $\mu'' = H . F' - H' . F$, $\nu'' = F . G' - F' . G$.

The three rows of coefficients will be respectively of the degrees $(n - \beta) + (n - \gamma)$, $(n - \gamma) + (n - \alpha)$, $(n - \alpha) + (n - \beta)$.

Thus if any one of the indices α, β, γ be zero, $\Pi(\alpha, \beta, \gamma)$ becomes identical with $\lambda^{?} . U + \mu^{?} . V + \nu^{?} . W$, where the multipliers of U, V, W are of $2n - (\alpha + \beta + \gamma)$ dimensions, i.e. of $(n - 1)$ dimensions, and may accordingly be put under the form

$$\Sigma A . x^{r} . y^{r'} . z^{r''} . U + \Sigma . B . x^{s} . y^{s'} . z^{s''} . V + \Sigma . C x^{t} . y^{t'} . z^{t''} . W,$$

---

* The prefixes of any such terms (say K) may be conceived as made up of two parts, an arbitrary constant, as e and $(K - e)$; e will disappear spontaneously from the final derivee.

that is to say, becomes a *linear function* of the augmentatives, and therefore if combined with them in the process of linear elimination would *give rise* to the identity $0 = 0$.

Hence we must reject all such secondary derivatives as have zero for one of the indices of derivation. But all others, it may be shown, will be *linearly* independent of one another, and of the augmentees previously found. Hence, besides $3 . \frac{n . n + 1}{2}$ equations of augment of the degree $2n - 1$, we shall have of the same degree so many equations of derivation as there are ways of stowing away between three lockers $(n + 1)$ things, under the condition that no locker shall ever be left *empty*, i. e. $\frac{n . (n - 1)}{2}$.*

Thus, then, in all we have $n . \frac{n - 1}{2} + 3 . \frac{n . n + 1}{2} = \frac{2n(2n + 1)}{2}$ equations, which is exactly equal to the number of arguments to be eliminated. Hence the final derivee can be obtained by the usual explicit rule of permutation, and moreover will be *its lowest form*, for it will contain in each term $\frac{n . (n + 1)}{2}$ prefixes belonging to the augmentatives of U, and a like number pertaining to those of V and of U, as well as $n . \frac{n - 1}{2}$ belonging to the secondary derivatives, each prefix in any one of which is triliteral, containing a prefix drawn out of those belonging to each of the proposees.

Thus every member containing $n . \frac{n + 1}{2} + n . \frac{n - 1}{2}$, i. e. n^2 of the original prefixes belonging to U, V, W, singly and respectively, the final derivee evolved by this process will be in its lowest terms; as was to be proved.

Case A.—Indices all equal.

Method 2.

It is remarkable that we may vary the method just given by making $r + r' + r'' = n - 2$, $s + s' + s'' = n - 2$, $t + t' + t'' = n - 2$. The augmentatives will thus be of the degree $2n - 2$.

Furthermore, we must make $\alpha + \beta + \gamma = n + 2$. It will still be possible to satisfy by integer multipliers the equations

$$U = x^{\alpha} . F + y^{\beta} . F' + z^{\gamma} . F'',$$
$$V = x^{\alpha} . G + y^{\beta} . G' + z^{\gamma} . G'',$$
$$W = x^{\alpha} . H + y^{\beta} . H' + z^{\gamma} . H''.$$

---

* Vide page 426 for the Latin version.

[these it will be useful in future to term the *equations*, x^α, y^β, z^γ being the *arguments*, and F, G, H, etc. the *factors* of decomposition] for otherwise calling the indices of x, y, z in any original argument a, b, c, their sum or n would be not greater than $(n + 2) - 3$, i. e. $(n - 1,)$ which is absurd.

For the same reasons as in the last case no index of augmentation must be made zero: the degree of each will be $(n - \alpha) + (n - \beta) + (n - \gamma,)$ i.e. $(2n - 2,)$ and their number $\frac{(n+1)n}{2}$; the number of augmentatives will be $3 . \frac{(n-1)n}{2}$ linearly uninvolved, each of the degree $2n - 2$, and therefore containing $\frac{(2n-1)2n}{2}$ arguments.

Now $\frac{(n+1)n}{2} + \frac{3.(n-1)n}{2} = \frac{(2n-1)2n}{2}$.

Hence the final derivee may be found, and it will be in its *lowest terms*, for every member will contain $\frac{3.(n-1)n}{2}$ letters due to the augmentative, and $\frac{3.(n+1)n}{2}$ due to the partial derivative equations; in all then there will be $3n^2$ letters in each term.

This second method being applied to three quadratic equations of the most general form, leads to the problem of eliminating between six simple equations which lies within the limits of practical feasibility, and it is my intention to register the final derivee upon the pages of some one of our scientific Transactions as a standing monument for the guidance of hereafter coming explorers*.

Scholium to Case (A).

If we attempt to carry forward these processes to quaternary systems, it becomes necessary to make

$$\left.\begin{array}{r} \alpha + \beta + \gamma + \delta = (r-2)n + 1 \\ \text{or else } \alpha + \beta + \gamma + \delta = (r-2)n + 2 \end{array}\right\}, \text{where } r \text{ is the number}$$

of proposees.

Now if the factors in the equations of decomposition are all integer, one of the indices of derivation must be not greater than the corresponding index in any of the original arguments, which may easily be shown to be always impossible for a system of equations, *complete* in *all* their terms, whenever their number r is greater than three, if $\alpha+\beta+\gamma+\delta=(r-2)\,n+2$; but if $\alpha+\beta+\gamma+\delta=(r-2)\,n+1$ only possible for the case of $n=2$, and for that case alone.

---

* Elimination between *two* quadratics leads to a final derivee made up of *seven* terms only; the final derivee of *three* quadratics is made up of at least several *thousand*; nay, I believe I may safely say, several *myriads* of terms!

Particular method applicable to *four Quadratics.*

Let $U=0$, $V=0$, $W=0$, $Z=0$, be four quadratic equations existing between x, y, z, t:

$$\begin{array}{llll} \text{Make } x.U=0 & x.V=0 & x.W=0 & x.Z=0 \\ \quad\;\; y.U=0 & y.V=0 & y.W=0 & y.Z=0 \\ \quad\;\; z.U=0 & z.V=0 & z.W=0 & z.Z=0 \\ \quad\;\; t.U=0 & t.V=0 & t.W=0 & t.Z=0. \end{array}$$

$$\begin{aligned} \text{Also write } U &= x^2.F+y.F'+z.F''+t.F'''=0 \\ V &= x^2.G+y.G'+z.G''+t.G'''=0 \\ W &= x^2.H+y.H'+z.H''+t.H'''=0 \\ Z &= x^2.K+y.K'+z.K''+t.K'''=0. \end{aligned}$$

By eliminating linearly we get
$\Sigma\{F\Sigma G'.(H''K'''-H'''.K'')\}=0$, which will be of the third degree, since the factors represented by the *unmarked* letters F, G, H, K are of zero, and all the rest of *unit* dimensions.

Similarly we may obtain other equations, so that besides the *sixteen* augmentatives already written down, we have four secondary derivatives, namely,

$$\Pi\,(2\,111)=0 \quad \Pi\,(1\,2\,11)=0 \quad \Pi\,(11\,21)=0 \quad \Pi\,(111\,2)=0.$$

Thus we have *twenty* equations and as many arguments to eliminate, since a perfect cubic function of four letters contains twenty terms.

The final derivee will contain $16+4\cdot4$ letters, i. e. 32, 8 or 2^3 belonging to each system of original prefixes in each member, and will therefore be in its lowest terms: for one of the canons of form teaches us, *à priori*, that every member of the derivee deduced from any number of assumed equations must contain in each member as many prefixes belonging to one equation of the system as there are units in the product of the indices of all the rest taken together.

Corollary to Case (A).

Either of the two methods given as applicable to this case enables us to determine integer values of X, Y, Z, which shall satisfy the equation

$$X.U+Y.V+ZW=F.x^p.y^q.z^r,$$

where F is the final derivee and $p+q+r=3n-2$. For by the doctrine of simple equations we know how to express F in terms of the linear functions, out of which it is obtained by permutation, i. e. we are able to assign values of A, B, C, and their antitypes, as also of L and its antitype, which shall satisfy the equation

$$\Sigma(\mathrm{A}.x^{r}.y^{r'}.z^{r''}.\mathrm{U})+\Sigma.(\mathrm{B}.x^{s}.y^{s'}z^{s''}.\mathrm{V})+\Sigma(\mathrm{C}.x^{t}.y^{t'}.z^{t''}.\mathrm{W})$$
$$+\Sigma(\mathrm{L}.\Pi(\alpha,\beta,\gamma))=\mathrm{F}.x^{f}.y^{g}.z^{h} \quad . \quad . \quad . \quad (1.)$$

where A, B, C, as well as L and all the quantities formed after them, are made up of integer combinations of the original prefixes.

Now the functions $\Pi(\alpha,\beta,\gamma)$ may be expressed in three ways in terms of U, V, W, as has been already shown.

We may therefore suppose these functions to be divided into three groups, and make

$$\left.\begin{aligned}\Sigma\,\mathrm{L}\Pi(\alpha\beta\gamma)=\Sigma\frac{\mathrm{Q}.\mathrm{U}+\mathrm{Q}'.\mathrm{V}+\mathrm{Q}''.\mathrm{W}}{x^{\alpha}}\\+\Sigma\frac{\mathrm{R}.\mathrm{U}+\mathrm{R}'.\mathrm{V}+\mathrm{R}''.\mathrm{W}}{x^{\beta}}\\+\Sigma.\frac{\mathrm{S}.\mathrm{U}+\mathrm{S}'.\mathrm{V}+\mathrm{S}''.\mathrm{W}}{x^{\gamma}}\end{aligned}\right\} \quad . \quad . \quad (2.)$$

And it is evident that the equations (1.) and (2.) lead immediately to the equation

$$\mathrm{X}.\mathrm{U}+\mathrm{Y}.\mathrm{V}+\mathrm{Z}.\mathrm{W}=\mathrm{F}x^{a+f}.y^{b+g}.z^{c+h},$$

if we call a, b, c the *greatest values* attributed respectively to α, β, γ.

Now if we suppose the first method to be followed,

$$f+g+h=2n-1.$$

And it will always be possible to make a, b, c of what values we please *subject* to the condition of $a+b+c=n-1$; for *one* at least of the indices of derivation in $\Pi(\alpha,\beta,\gamma)$ must be not greater than its correspondent among a, b, c; otherwise $\alpha+\beta+\gamma$ would be not less than $(a+b+c)+3$,

$$\text{but}\ \left.\begin{aligned}\alpha+\beta+\gamma=n+1\\a+b+c=n-1\end{aligned}\right\}\text{, which is absurd.}$$

Hence we can satisfy $\mathrm{X}.\mathrm{U}+\mathrm{B}.\mathrm{Y}+\mathrm{Z}.\mathrm{W}=\mathrm{F}.x^{p}.y^{q}.z^{r}$, p, q, r being subject to the condition of $p+q+r=3n-2$, but otherwise arbitrary.

Moreover, we can *not* do so if $p+q+r$ be *less* than $3n-2$, for that would require $a+b+c$ to be less than $n-1$. Now if two of the indices of derivation, as α and β, be made equal to $a+1$, $b+1$ respectively, the third $\gamma=(n+1)-(a+b+2)=(n-1)-(a+b)$, and is therefore greater than c:

so that $\alpha + \beta + \gamma$ for this case becomes greater than $a + b + c$, and the method falls to the ground.

In fact, I have discovered a theorem which lets me know this, *à priori*, a law which serves as a staff to guide my feet from falling into error in devising linear methods of solution, and the importance of which all candid judges who have studied the general theory of elimination cannot fail to recognize. To wit, if $X_1\ X_2\ X_3 \ldots\ldots X_n$ be (n) integer complete polynomial functions of n letters $x_1\ x_2 \ldots x_n$, and severally of the degree $b_1\ b_2\ b_3 \ldots\ldots b_n$; then it is always possible to satisfy the identity,

$$P_1 . X_1 + P_2 . X_2 + P_3 . X_3 + \ldots\ldots + P_n . X_n$$
$$= F . x_1^{\alpha_1} . x_2^{\alpha_2} . x_3^{\alpha_3} \ldots x_n^{\alpha_n}$$

if $\alpha_1 + \alpha_2 + \alpha_3 + \ldots\ldots + \alpha_n$ be equal to or greater than $b_1 + b_2 + b_3 + \ldots\ldots + b_n - n + 1$, but otherwise *not**. This again is founded immediately upon a simple proposition, of which I have obtained a very interesting and instructive demonstration, shortly to appear, and which may be enumerated thus: "*The number of augmentees of the same degree that can be formed, linearly independent of one another, out of any number of polynomial functions of as many variables, may be either equal to or less than the number of distinct arguments contained in such augmentees, but* never greater. *The latter will be the case when the index of the augmentees diminished by unity is less than the sum of the indices of the original unaugmented polynomials each so diminished; the former, when the aforesaid index is equal to or greater than the aforesaid sum.*"

To return to the particular case of finding X, Y, Z to satisfy $X . U + Y . V + Z . W = F . x^p . y^q . z^r$.

This has been already done according to the first method; if we employ the *second* method of elimination we shall have

$$f + g + h = 2n - 2;$$

---

* Hence it is apparent, that in applying the method of multipliers, a curious and important distinction exists between the cases of there being two equations, and there being a greater number to eliminate from: for in the first case the element of arbitrariness needs never to appear; in the *latter* it cannot possibly be excluded from appearing in the multipliers.

This will explain how it comes to pass that the method of the text may be employed to give *various* solutions of the $X . U + Y . V + Z . W = F . x^p . y^q . z^r$; thus not only can (p), (q) and (r) be variously made up of $(f + a)$, $(g + b)$, $(h + c)$, but also $\Pi\ (\alpha, \beta, \gamma)$ when two of the indices (α, β suppose) are each not greater than the assigned greatest values a, b may be made to figure indifferently either under the form

$$\frac{\lambda . U + \mu . V + V . W}{x^\alpha} \text{ or that of } \frac{\lambda' . U + \mu . V + V' . W}{x^\beta}.$$

but, now since $\alpha + \beta + \gamma = n + 2$, we shall easily see by the same method as above, that the least value of $a + b + c$ (where a, b, c denote respectively the greatest values of α, β, γ, appearing in the denominator of the fractional *forms* used to express $\Pi(\alpha, \beta, \gamma)$, will be one greater than before, or n; so that $f + g + h + a + b + c$ will still be equal to $3n - 2$, as we might, *à priori*, by virtue of our rule, have been assured.

Ternary Systems.

Case (B).—*Two of the indices equal; the third* less *by a unit.*

Let $U = 0$, $V = 0$, $W = 0$, be the three given equations severally of the degree n, n, $(n-1)$.

Make $r + r' + r'' = n - 2$, $s + s' + s'' = n - 2$, $t + t' + t'' = n - 1$, by multiplying U into $x^r . y^r . z^{r''}$, V into $x^s . y^{s'} . z^{s''}$, W into $x^t . y^{t'} . z^{t''}$, we obtain augmentees each of the same, namely, the $(2n - 2)$th degree.

The number of these is, $\frac{(n-1)n}{2} + \frac{(n-1)n}{2} + \frac{n . \overline{n+1}}{2}$.

Again, make $\alpha + \beta + \gamma = n + 1$.

It will still be possible, as before, to form equations of decomposition in which $x^\alpha y^\beta z^\gamma$ are the arguments, and affected with *integer* factors. For if we look to W *even*, all its arguments are of the form $x^a . y^b . z^c$, where $a + b + c = (n-1)$, and each of these cannot be less than its correspondent, for that would be to say that $(n-1)$ is not greater $(n+1) - 3$, *à fortiori*, U and V can be decomposed in the manner described. Thus, then, we shall obtain as many secondary derivees as in the last case (Method 1.), i. e. $\frac{n . (n-1)}{2}$ (since $\alpha + \beta + \gamma$ is still equal to $\overline{n+1}$), as before. Moreover, each of these will be of $(n - \alpha) + (n - \beta) + (n - 1 - \gamma)$, i. e. of $2n - 2$ dimensions.

Altogether, therefore, we have

$$\left\{\frac{(n-1)n}{2} + \frac{(n-1)n}{2} + \frac{n(n+1)}{2}\right\} + \frac{(n-1).n}{2}$$

linear independent equations of the degree $2n - 2$, and the number of arguments to eliminate is $\frac{(2n-1)2n}{2}$. Now these two numbers are equal. Thus we obtain a final derivee containing of U's coefficients $\frac{(n-1)n}{2} + \frac{(n-1)n}{2}$, an equal number of V's, but of W's $\frac{n.(n+1)}{2} + \frac{(n-1)n}{2}$; now $n(n-1)$, $n(n-1)$ and n^2 exactly express the number that ought to appear of each of these respectively: hence the final derivee is clear *of irrelevant factors.*

Ternary Systems.

Case (C). *Two of the indices equal; the third one,* greater *by a unit.*

Here, calling (n) the highest index, the augmentees must each be made of the degree $(2n-3,)$ their number will evidently be $\frac{(n-2)(n-1)}{2} + \frac{(n-1)n}{2} + \frac{(n-1)n}{2}$, making the sum of the indices of derivation now, as before, equal to $(n+1)$; it will be still possible to form integer equations of decomposition, which will give rise to augmentatives of the degree $(n-\alpha)+(n-1)-\beta+(n-1)-\gamma$, i. e. of $(2n-3)$ dimensions. The total number of equations, what with augmentatives and secondary derivatives, will be $\left\{\frac{(n-2)(n-1)}{2} + \frac{(n-1)n}{2} + \frac{(n-1)n}{2}\right\} + \frac{n(n-1)}{2} = \frac{4n^2-4n+2}{2} = \frac{(2n-2)(2n-1)}{2}$, i. e. is equal to the exact number of distinct arguments contained between them.

Also the final derivative will contain in each member $\frac{(n-2)(n-1)}{2} + \frac{n.(n-1)}{2}$, i. e. $(n-1)(n-1)$, letters belonging to the first equation, and $\frac{(n-1)n}{2} + \frac{n.(n-1)}{2}$, i. e. $n.(n-1)$ belonging to those of the second and of the third, and will therefore be in its *lowest terms.*

Corollary to Case (B) and (C).

It is not necessary, after all that has been already said, to do more than just point out that the processes applicable to these cases enable us to determine X, Y, Z, which satisfy the equation $X.U + Y.V + Z.W = F.x^f.y^g.z^h$,

where $f+g+h = 3n-3$ for case (B).
and $f+g+h = 3n-4$ for case (C).

22 Doughty Street, Mecklenburgh Square, April 29, 1841.

[To be continued.]

LXX. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. FRANCIS *and* H. CROFT.

[Continued from p. 372.]

The Cacodyl Series (continuation).

PROTOCHLORIDE of cacodyl, $\mathrm{Kd\,Cl^2}$, cannot be obtained pure by distilling the oxide of cacodyl with hydrochloric acid; a basic salt is then formed. The best method is to distil hydrargochloride of cacodyloxide with hydrochloric acid; the product must be dried over lime and chloride of calcium, and then distilled alone in a tube filled with carbonic acid:

$$\left.\begin{array}{l}\mathrm{Kd\,O + Hg^2\,Cl^4}\\ \quad\mathrm{H^2\,Cl^2}\end{array}\right\}\begin{array}{l}\mathrm{Kd\,Cl^2}\\ \mathrm{H^2\,O}\\ \mathrm{Hg^2\,Cl^4}.\end{array}$$

The liquid thus obtained is æthereal, colourless, does not solidify at − 45° C., and is converted a little above 100° C. into a colourless gas, which inflames in the air. The fluid itself, when burnt, deposits metallic arsenic, or arsenious acid, accordingly as the air has more or less access. In oxygen it explodes violently when heated. When the air has but very slow access to it, it deposits beautiful colourless crystals. In chlorine it inflames and burns with deposition of carbon. Its smell is extremely penetrating and stupifying. In greater quantities it excites to such an extent the mucous membrane of the nose, that it swells, and blood drops from the eyes. It does not dissolve in water, but imparts its odour to it. In æther it is insoluble, but soluble in alcohol. Dilute nitric acid dissolves it without decomposition, the concentrated acid causes explosion. The chlorine is precipitated entirely by silver salts. Caustic lime and baryta do not decompose it, except at a high temperature. An alcoholic solution of potash forms chloride of potassium, and an æthereal volatile substance containing no chlorine ($\mathrm{C^4\,H^{10}\,As^2}$?), easily soluble in water and alcohol. Sulphuric and phosphoric acids separate the hydrochloric acid. Formula $\mathrm{C^4\,H^{12}\,As^2\,Cl^2}$; sp. gr. of vapour 4·56; calculated 4·86.

By passing dried hydrochloric acid gas into alkarsin, a hydrate of protochloride of cacodyl is formed; it is a thick oily fluid, which is decomposed by distillation.

Protoiodide of Cacodyl, $\mathrm{Kd\,I^2}$.—Formed by the distillation of cacodyloxide with concentrated hydriodic acid; in the receiver under the water there appears a yellowish oily liquid, which by slow cooling forms beautiful rhomboidal laminæ of a yellow substance; it is then cooled, and the iodide remaining fluid is separated, and again distilled with hydriodic acid. In order to free it from water, it is allowed to stand some days in a closed tube in contact with lime and chloride of calcium, and then distilled in a tube filled with carbonic acid until the half or, at the most, two thirds has passed over. The iodide thus produced is a thin yellowish liquid, smells somewhat similar to the chloride, has a high specific gravity; remains fluid at — 10° C.

Boiling point considerably above 100° C. The vapour of it is yellowish. If allowed to stand in the air, forms beautiful prismatic crystals. The iodide is soluble in æther and alcohol; insoluble in water; decomposed by sulphuric and nitric acid. When heated in the air it burns yielding vapour of iodine. Formula $C^4\ H^{12}\ As^2\ I^2$. The specific gravity of the vapour could not be determined, because it is easily decomposed by mercury. The calculated is 5·816.

Protobromide.—Formed by distilling concentrated hydrobromic acid with hydrargochloride of cacodyloxide; it forms a fuming yellow liquid, very similar in its properties to Kd Cl.

Protofluoride is obtained in a manner analogous to the preceding; it is a colourless fluid of insupportable smell, insoluble in water, but suffers decomposition by it. It attacks glass; $C^4\ H^{21}\ As^2\ Fl^2$.

*Hydrargo-chloride of Cacodyloxide**.—When a dilute alcoholic solution of oxide of cacodyl is added to a dilute solution of corrosive sublimate, a voluminous white body is precipitated, which consists of the above compound mixed with calomel; from this it may be freed by repeated crystallizations. It may be formed with any other compound of cacodyl which is similarly constituted to the oxide. An excess of sublimate decomposes the salt. Formula $C^4\ H^{12}\ As^2\ O + Hg^2\ Cl^4$. Bunsen leaves it for the present undetermined whether the oxygen is only 1, 1½ or 2 atoms. He assumes that sublimate is contained in it, because potassa precipitates oxide of mercury, and hydriodic acid produces the scarlet iodide and iodide of cacodyl, &c. &c. By distillation with phosphorous acid it is decomposed into chloride of cacodyl and calomel. Chloride of gold is reduced by it.

100 parts of boiling water dissolve 3·47 parts of the salt; at 18° C. the solution contains only 0·21 parts. It is soluble in alcohol, inodorous if not brought into the nose; taste disagreeably metallic; in larger quantities is poisonous, &c.

---

* We may here remark, that this name is a literal translation from the German Quecksilberchlorid Kakodyloxyd. We do so because an anonymous writer has criticised our notices, on account of the nomenclature, in a short paper in the Phil. Mag.

The *hydrargobromide* is very similar to the hydrargochloride.

Basic chloride of cacodyl is formed by treating the chloride with water, or by distilling with hydrochloric acid. Formula $(C^4H^{12}As^2 + O) + 3 (C^4H^{12}As^2 + Cl^2)$, or $KdO + 3 KdCl^2$. Density of the vapour 5·46 ; according to calculation it should be 5·35. This substance is very similar to the chloride; it boils at 109° C.

Basic bromide of cacodyl is prepared in the same manner as the above, to which it is very similar. It is yellow, becomes colourless on heating, and acquires its yellow colour again on cooling. Formula $(C^4 H^{12} As^2 + O) + 3 (C^4 H^{12} As^2 + Br^2)$, or $Kd O + 3 Kd Br^2$.

Basic iodide of cacodyl is produced, together with the iodide, on distilling the oxide with hydriodic acid. It is deposited from the neutral compound in yellow crystalline crusts. It may be purified by recrystallization in alcohol and pressure between bibulous paper. The water may be got rid of by allowing it to stand some days in a fluid state in contact with chloride of calcium, and then distilling. It absorbs oxygen with such rapidity, that Bunsen was not able to make an analysis of it.

The iodide of cacodyl and the oxide may be mixed together without combining, if they are both anhydrous; but if a drop of water be added, the solution becomes instantly one solid mass, which consists of the basic iodide. The substance is very soluble in alcohol, but little in water: melts below 100° C., and may be distilled over unchanged. In the air it evolves white vapours, and heats so much that it becomes fluid, and sometimes inflames. By distillation with hydriodic acid it cannot be reconverted into the neutral iodide, and in so far it differs from similarly constituted inorganic salts.

M. Bunsen intends continuing his researches on these interesting bodies.

Oil of Esdragon—Oil of Sabine.

M. Laurent has analysed the oil of Esdragon. Its formula is $C^{24} H^{32} O^2$; it boils at 206° C.; has a specific gravity of 0·945; that of its vapour was found by experiment to be 6·157, calculated 6·158. It combines with sulphuric acid to form sulphodraconic acid; the formula of the baryta salt is $C^{24} H^{32} O^2, \dddot{S} + \dot{B}a$; treated with nitric acid, it gives five new crystallizable acids.

Oil of Sabine has the same constitution and same properties as oil of turpentine. By the action of ammonia on oil of cinnamon M. Laurent obtained a substance which he calls cinnhydramid, $C^{18} H^{18} N_{\frac{4}{3}} O$; he remarks, that the law of substitution has not been followed in this case; the oil has lost 1 atom of oxygen and taken $H^{3}N$.

Bromide of camphor, $C^{20}H^{32}O^{2} + Br^{4}$, is crystallized, deliquesces in the air; bromine flies off and leaves camphor; this change is effected immediately by ammonia. When distilled it is also decomposed, but generally some hydrobromic acid, and an oily bromide are formed, $C^{20}H^{32}O^{2} + Br^{4} = C^{20}H^{30}Br^{2}O^{2} + H^{2}Br^{2}$. (*Compt. Rendus*, x. p. 531.)

Oil of Potatoe-Spirit—Amilen.

M. Cahours has published some more of his experiments on the oil of potatoe-spirit. *Hydrochlorate of amilen* is obtained by distilling equal parts of the oil and chloride of phosphorus; the product is washed with water in which a little potassa has been dissolved, dried over chloride of calcium, and distilled. It is a colourless, aromatic, agreeably smelling fluid, insoluble in water; boils at 102°; does not change litmus paper, and is not decomposed by nitrate of silver. It burns with a flame with green margin. Formula $C^{10}H^{22}Cl^{2}$. M. Cahours examined the action of chlorine on this substance when in the sunshine; hydrochloric acid gas was developed, but soon ceased. The product was a clear colourless liquid, smelling like camphor. Formula $C^{10}H^{6}Cl^{18} = C^{10}H^{22}Cl^{2} - H^{16} + Cl^{16}$.

Acetate of amilen is easily obtained by distilling a mixture of two parts of acetate of potash, one part potatoe-oil, and one part sulphuric acid; the product is dried by means of chloride of calcium, and distilled over oxide of lead. It is a colourless volatile liquid, which boils at 125°. The smell is somewhat similar to that of acetic æther; lighter than water; insoluble in the same, but soluble in alcohol, æther and potatoe-oil; is decomposed by an alcoholic solution of potassa into acetate of potash and potatoe-spirit oil. Density of the vapour 4·458; calculated 4·475. If chlorine be passed into this æther heated to 100°, until no more hydrochloric acid gas is given off, a product is obtained, whose formula is $C^{4}H^{6}O^{3}$, $C^{10}H^{18}OCl^{4}$, similar to Malaguti's "Ether acétique Chloruré." In the sunshine this body is acted on by chlorine; small crystals are formed, but the products have not been examined. By the action of spongy platinum the oil of potatoe-spirit is converted into valerianic acid. (*Ann. de Chim. et de Phys.* lxxv.)

On the Resins of Benzoin. By M. Van der Vliet.

Benzoin is pulverized and boiled with carbonate of soda; one of the resins is dissolved, the brown solution is filtered, treated with hydrochloric acid, and thrown boiling hot on a filter. The resin remains on the filter, and a solution of benzoic acid passes through. The benzoin is boiled two or three times, as stated above, and then treated with æther, which dissolves half of it; on evaporation the æther deposits a resin. The third resin which remains behind is soluble in alcohol. This latter is Berzelius's betaresin, that soluble in æther the alpharesin, and the third gammaresin; these resins thus prepared are not perfectly pure. The gammaresin must be boiled with water to free it from benzoic acid, it then melts at 180° C, and is pure. The alpharesin forms an insoluble compound with carbonate of soda; by long boiling of the benzoin with carbonate of potash, all alpharesin is extracted, for it is converted into gammaresin and becomes soluble. Carbonate of soda effects this change very slowly. After the extraction of the alpharesin by æther, there remain behind betaresin, the alpharesin compound and the impurities; these latter remain when the mass is boiled with alcohol; on cooling the alpharesin compound is precipitated; the betaresin remains dissolved. The properties of these resins have been described by Unverdorben, Poggendorff's *Annalen*, xvii., Berzelius's *Chemie*, vii.

The formula for the alpharesin is $C^{70} H^{84} O^{14}$; the atomic weight, as found from the analysis of the lead compound, is 7214·20; calculated 7274·58. Betaresin is $C^{40} H^{44} O^{9}$; the analysis of the lead salt gives, as atomic weight, 4099·9; calculated 4231·94.

Gammaresin is $C^{30} H^{40} O^{5}$; atomic weight 3042·64; according to analysis 3062·81. It is probable, from what has been said above concerning the metamorphosis of alpharesin into gammaresin, that benzoin contains only two resins for

$$\begin{array}{rl} \text{Alpharesin} \;\ldots\ldots\ldots & C^{70} + H^{84} + O^{14} \\ \textit{minus}\ \text{Gammaresin} & \underline{C^{30} + H^{40} + O^{5}} \\ \text{Betaresin}\ldots\ldots & C^{40} + H^{44} + O^{9} \end{array}$$

They may be considered as oxides of $C^4 H^4$ combined with a hydrocarbon; thus

$$\begin{array}{lrl} \text{Alpharesin} \;\ldots\ldots & 14\,(C^4 H^4)\,O^{14} & +\ 14\,C H^2 \\ \text{Betaresin} \;\ldots\ldots & 9\,(C^4 H^4)\,O^{9} & +\ \ 4\,C H^2 \\ \text{Gammaresin}\ldots & 5\,(C^4 H^4)\,O^{5} & +\ 10\,C H^2 \end{array}$$

(*Annalen der Pharmacie*, xxxiv.)

[The account of Mr. Johnston's experiments, *Phil. Mag.* vol. xvii. p. 384, is too short to allow of any conclusions being drawn from it. I am not acquainted with the methods Mr. J. used for purifying his resins. It appears, however, that he has not observed the alpharesin of Van der Vliet. In paragraph 4, he says that potassa decomposes benzoin into $C^{40} H^{22} O^{9}$ ($C^{40} H^{44} O^{9}$) and $C^{40} H^{30} O^{9}$. The former is evidently the betaresin; probably all alpharesin had been decomposed into the other two. The soluble resin, $C^{40} H^{30} O^{7}$, agrees tolerably well with the gammaresin. I regret extremely that I have no opportunity of obtaining further particulars of Mr. Johnston's experiments on the subject.—H. C.]

Veratric Æther.

Dr. Will has prepared veratric æther by dissolving the acid in strong alcohol, and saturating it with hydrochloric acid gas. On mixing with water the æther separates as a thick oily fluid, which, by washing with a dilute solution of carbonate of soda, gradually solidifies. It is then dried by means of sulphuric acid under the air-pump. It melts at 42° C.; is almost inodorous; has a bitter burning taste; scarcely soluble in water; soluble in alcohol, out of which it crystallizes in groups of acicular crystals; not volatile without partial decomposition. Specific gravity at 18° C. = 1·141. Formula $C^{4} H^{10} O, C^{18} H^{18} O^{7}$. (*Annalen der Pharmacie*, xxxvii.)

Action of Anhydrous Sulphuric Acid on Anhydrous Camphoric Acid.

M. Walter has examined the action of anhydrous sulphuric acid on anhydrous camphoric acid. The camphoric acid is dissolved by fuming sulphuric acid; on heating carbonic oxide is developed, but no other gas. By saturating the fluid with carbonate of baryta a soluble salt is obtained, which must be evaporated *in vacuo*; it does not crystallize, but forms an amorphous colourless mass, soluble in water and alcohol. Formula $C^{9} H^{14} O^{3}, SO^{2}, BaO$. Camphoric acid is $C^{10} H^{14} O^{3}$; the decomposition is thus $C^{10} H^{14} O^{3} + SO^{3} = C^{9} H^{14} O^{3} + SO^{2} + CO$. The lead salt is $C^{9} H^{14} O^{3}, \ddot{S} + \dot{Pb}$. The potassa salt is crystalline; the lime salt is amorphous; they have both similar formulæ. The acid itself may be obtained crystallized, but not dry. (*Annales de Chemie.*)

M. Walter has published some experiments on the action of anhydrous phosphoric acid on camphoric acid. The action is quite different from that of sulphuric acid; the one atom of carbon has its place taken by the sulphurous acid in the former case, but in the latter the decompositions are more complicated. When the two acids are distilled together much gas is evolved, and an oily fluid passes over. The gas consists of one volume of carbonic acid, and four volumes of carbonic oxide. The fluid is a hydrocarbon, C 88·4; hydrogen 11·6. It might be oil of turpentine, but M. Walter considers it more probable that it is a kind of naphtha, containing 89 per cent. carbon, and explains his finding too little by the circumstance that the fluid contains phosphuretted hydrogen, from which it is with difficulty freed. (*Annales de Chim. et de Phys.* lxxv.)

LXXI. *On the Manufacture of Platinum.* *By* A CORRESPONDENT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WILL you allow me, through your pages, to suggest to chemists that the price of that necessary article, platinum, might be reduced by means of the electrotype process? I believe that the high price of this metal is in some measure due to the labour required to reduce it into a malleable state by the method of Wollaston. This portion of the expense might, I think, be reduced to a very small part of the whole, if the metal were at once reduced from its solution by the slow action of electricity, as this mode involves no labour, while the necessary apparatus is cheap. Might not the same method be applied to reduce nickel to a malleable state? This being a cheap metal, not liable to rust, it might perhaps be advantageously used for some purposes for which platinum is too expensive.

I am, Gentlemen, your obedient servant,

C.

LXXII. *On testing for Arsenic and Antimony by Hume's Process.* *By* J. MARSH, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IF you should think the following communication of sufficient importance, I beg you will give it a place in your valuable Journal.

In testing for arsenic I have found, by repeated experiments, that Hume's test (the ammonia-nitrate of silver) is extremely useful as a discriminative test for arsenic or antimony. The way that I use it is simply as follows:—After the matter to be tested has been acted on in my apparatus, a piece of common window-glass, (which I prefer, as it can be obtained with ease,) porcelain or mica, is to have one of its surfaces moistened with Hume's test; it is then to be held horizontally, with its moistened side downwards, directly over the ignited jet of gas, about half an inch from the tip of the flame. If arsenic be present, the well-known characteristic lemon-yellow colour is instantly produced; if antimony be in the mixture, a curdy-white precipitate is obtained: if, on the contrary, neither arsenic nor antimony is in the matter under examination, the hydrogen instantly reduces the silver of the test-liquor to the metallic state.

It is really beautiful to see the admirable manner in which this test performs its duty, and I submit the same with confidence to the attention of your numerous readers and correspondents.

I am, Gentlemen, yours truly,
J. MARSH.

Royal Arsenal, Woolwich, May 10th, 1841.

LXXIII. *On the Composition of Chalk Rocks and Chalk Marl by invisible Organic Bodies: from the Observations of Dr. Ehrenberg.* By THOMAS WEAVER, *Esq., F.R.S., F.G.S., M.R.I.A, &c. &c.*

[Continued from p. 397.]

On the Geographical Distribution of Living Polythalamia on the African and Asiatic Coasts of the Mediterranean, and in the Red Sea.

THE materials collected by Dr. Hemprich and myself in the Mediterranean refer to four points on the Libyan coast, and one point on the Syrian coast. In regard to a second point on the latter coast (St. Jean d'Acre), I have acquired a knowledge of some forms from the collection of Dr. Parthey.

From the Red Sea nine forms were made known to us by d'Orbigny, collected from sand presented to him by Deshayes. But from the collections made by Dr. Hemprich and myself from thirteen points along the whole length of the Red Sea, it appears that very numerous forms exist. Of seven of those points, one occurs on the western (African) coast at Suez, and six on the eastern (Arabian) coast, namely, at Tor, Erraie and el Ard, Moileh, el Wusch and Gumfüde; and of the remaining six, five are islands on the Arabian side, namely, Sanafer, Maksure, Barkan, Sanac and Ketumbul, and one an island on the African side of the Red Sea, namely, Massaua.

It is possible that by repeated and closer examination of the marine productions collected by us, many other Polythalamia may be found besides those already discovered. In the mean time, as a preliminary, I have drawn up a list of the species hitherto met with*. From this it results that the total number of species of Polythalamia observed in the Red Sea are *fifty*, and in the Mediterranean, on the Libyan and Syrian coasts, *twenty-seven*. The new species derived from the two seas amount to *fifty-four*, of which *twenty-seven* species are peculiar to the Red Sea, and *seventeen* are common to both seas. Particularly worthy of notice is the wide distribution and massy development of the *Peneroplis planatus* and *Sorites Orbiculus*, which are rare on the European coast. These forms are not only present almost everywhere in the East, but constitute the predominant masses. On the other hand, the *Rotalia Beccarii*, which composes the Italian hills, occurs only singly and very rarely in the Red Sea; and I nowhere found it on the Libyan and Syrian coasts. The *Sorites Orbiculus* I have also from St. Domingo.

---

* Of d'Orbigny's nine species from the Red Sea, there are three which I cannot identify, namely, *Triloculina bicarinata*, *Quinqueloculina limbata*,

In reviewing these subjects, even a superficial comparison of them with the contents of the chalk and chalk marl, is attended with the striking result, that none of these living forms are found among the animalcules of the chalk, not even among those which compose the compact limestone of the Egyptian and Arabian rocks, and which are still partly washed by the sea near Hamam Faraūn.

Remarks on Polythalamia.

After a preliminary view of the researches of earlier labourers in this branch of zoological inquiry, Dr. Ehrenberg observes:—A lively interest respecting the minute Polythalamian bodies which enter into the composition of sea-sand was excited anew by the work of Alcide d'Orbigny in 1826, in which are contained a great number of new species, while many of those which were previously known are examined with greater care, and an improved and easier view is taken of the whole subject. By his active exertions he had collected between 600 and 700 species from the sea-sand of France, Italy, England, the Isle of France, Sandwich Islands, the Malouine and Marian Isles, &c., of which, however, only 425 received names. The whole mass of these microscopic animalcules, which he again decidedly associates with the Mollusks and Cephalopods,

---

and *Q. punctata*; but the other six are probably those with which I have become acquainted, and to which I have therefore given the same names, namely, *Textularia communis*, *Calcarina Defrancii*, *C. Gaudichaudii*, *Quinqueloculina sulcata*, and *Vertebralina striata*. His *Assilina* (*Nummulina*) *nitida* I hold to be the *Sorites Orbiculus*.

Although I possess and have compared many of the Polythalamia which have been described by d'Orbigny derived from the same localities, yet I am in want of a great number of the originals named by him, and as this author has generally given new names unaccompanied by descriptions, I have not in most cases been able to determine to what form the name given by him belongs.

but in a distinct order under the name of *Foraminifères*, are distributed by him into five families, according to the spiral or other form in the grouping of the cells; these families comprising fifty-two genera. On this work Deshayes made various critical remarks in the *Dictionnaire Classique*. D'Orbigny expressly states that the animal of the Polythalamia (his Foraminifera) resembles the Sepia in the structure of its body, although much smaller, and then proceeds to give the essential characters of the living body of the Polythalamia, yet without naming specifically or generically any one animal from which they were taken*.

Both Blainville and Dujardin have made the correct observation that the minute shells of the Polythalamia are external cases, and not, as incorrectly viewed by Denys de Montfort and Alcide d'Orbigny, internal bones. Yet in referring the microscopic so-called Cephalopods to the Infusoria, Dujardin commits a mistake†. It was this contradiction between observers that induced Férussac, in his great work, *Histoire Naturelle des Mollusques*, to exclude the Foraminifers from the class of the Mollusks; and others entertained similar objections, yet without assigning to them a correct position.

In the year 1831 I laid before the Academy contributions to the knowledge of Coral animals, with an attempt to class them physiologically; which attempt was entirely founded on my own observations of the living animalcules, when, accompanied by Dr. Hemprich, I travelled on the Red Sea in the years 1823 and 1825. In that work I designated the Coral animals as composed of two strongly marked organically distinct groups, under the names of *Anthozoa* and *Bryozoa*. In the year 1831 also, I communicated in the *Symbolæ Physicæ* the first development made of the complicated structure of the *Halcyonella stagnorum*, one of the Bryozoa, and showed that it was quite similar to that of Flustra.

The researches of Dujardin in 1835 gave an entirely new direction to the ideas which had been formed of the Polythalamia, showing that not a trace of resemblance was to be found between them and Sepia; on the contrary, the greatest simplicity of structure became apparent, bespeaking a simple animal body covered by a shell, with the power of extending or contracting itself at will. But when Dujardin expressly compares the Polythalamia to the *Proteus* (*Amoeba*) of the Infusoria, such an association cannot be admitted, unless it be first proved that a polygastric structure exists in those bodies. He has given to them the new name of *Rhizopodes*.

---

* *Annales des Sciences Naturelles*, 1826, t. vii. p. 245.

† *Annales des Sciences Naturelles. Seconde Série*, t. iv. p. 343, 1835.

I showed, in 1837, that the Polythalamia could not well possess an organization similar to that of the Infusoria, as not a single known true species of Infusoria has a calcareous shell; and I had, in 1823, discovered, as I conceived, a true living Polythalamia of earlier authors, resembling in organization the very complex Flustra. The correctness of this view was fully established in 1839, after having examined anew, according to my improved method, the small *Nautilus Orbiculus* of Forskål, which d'Orbigny designated in 1826 as *Nummulina* (*Assilina*) *nitida*, specimens of which I had collected from the sand of the Red Sea in 1823, and which I have named *Sorites Orbiculus*. The result proved that the disc-like shell was a Polypary, often composed of more than one hundred single animalcules, the cells of which quite resemble those of a Flustra, the animal putting forth and retracting from six to eight tentacula. And I even discovered in the interior of the single cells well-preserved siliceous Infusoria, the last food taken by the animal; and in some of them also small globular bodies, which, without much constraint, may be considered as eggs. Though I had at an early period observed that the disc was composed of many cells, yet I could not perceive an opening to them; but the discovery of Infusoria in their interior led me to consider by what means they could have been introduced. Reflection reminded me that I had often seen Coral animals which in the expanded state exhibited many large bodies with tentacula and a large mouth, yet when contracted left scarcely a trace of the openings through which they were protruded from the common Polypary. As such I remembered *Pennatula*, *Lobularia*, *Halcyonium* and similar forms, in which I had frequently observed, that in the skin of the animal existed calcareous particles, which on the contraction of the skin so completely closed the opening as to render it no longer perceptible. Renewed examination of the closed surface of the cells of the *Nautilus Orbiculus*, Forskål, now showed to me that in them also dendritic calcareous particles exist, the close approximation of which closes the cell, so that the cover of the cell is in fact the dried skin of the animalcule. I now made an experiment in proof, by dissolving the small shell in dilute muriatic acid, in order to obtain the animal body in a free state; and it succeeded perfectly. I obtained as many animalcular bodies as there were cells, connected together by band-like processes, and in the interior of many of them there were well-preserved siliceous Infusoria. I then treated in the same manner the *Flustra pilosa* and *F. membranacea* of the Baltic, and found in their interior also siliceous Infusoria. The same results followed a similar examination of the shells of *Rotalia* from the sand of Rimini, of the shells of *Peneroplis planatus*, *Pavonina Antillarum*, and of *Orbiculina numismalis* from the sea-sand of St. Domingo, as well as of other shells with their animals from the sand of the Red Sea and the Mediterranean; so that now a view is obtained of the more general organization of the principal groups of the Polythalamia.

It results clearly from what has been said in respect of these species, which are so common and widely distributed, and which have hitherto been designated in systems as small Nautili, that the straight-jointed shells of Nodosaria (formerly viewed as Orthocera), as well as the spiral shells of Rotalia, Cristellaria, &c. (considered as Nautili or Ammonites), and the shells of Biloculina resembling vermiform tubes (*Serpula*), are none of them internal calcareous parts which were encased by an animal body, similar to the internal bone of Sepia, or the cylindrical spiral bone of Spirula; but, on the contrary, that they are external calcareous shells, bearing analogy to those of Mollusks, or more correctly to those of Flustra and Cellepora, which, after separation by an acid, disclose and render visibly free the internal simple body or the Polypary, exhibiting precisely the same form. If the shell of Polythalamia be frequently perforated with pores, this is no proof that no other openings exist, or that the animals receive nourishment through many tubes, for the same structure is not unfrequently found in Flustra accompanied with the peculiar opening from which the fore-part of the animal body may be protruded; and in these exist also fringe-like filaments, which are extensile and retractile, and by no means to be compared to the pseudopodia or variable feet of Amoeba, but probably bear analogy to the mantle fringes of many Mollusks, applicable to the purposes of creeping and attachment, and for which perhaps they were specially designed. Moreover, Flustra possess a distinct large animal organization; and the siliceous Infusoria, and probable eggs found in Polythalamia, clearly bespeak in them also similar relations, the discovery of which, however, had hitherto been prevented by the calcareous encasement and the minuteness of the objects.

It has resulted from the examination of the soft small animal bodies of living Polythalamia, that while many resemble Flustra or Eschara assembled in families or polyparies, each such family being often composed of hundreds of much minuter single animalcules, many others are single animals after the manner of Mollusks. Hence arise external characters and forms which have often a reference to very different relations, which it is first necessary to distinguish before we can succeed in obtaining a clear view of the whole. The assiduous and careful labours of d'Orbigny retain their full value, serving as a basis to all future researches; and if in the present communications I shall have succeeded in turning the inquiry into a more physiological channel, my object will be attained.

To the term *Polythalamia*, (originally introduced by Dr. Breyn, of Danzig, in 1732,) a different extension or signification under other names has been given by different authors. To remove this unsteadiness and wanton change of names, which only lead to obscurity, it appears advisable to apply the term Polythalamia, in preference, as Soldani had done, to that group in which the animalcules actually live in many cells, and do not, like the Nautili, possess many empty cells. This distinction, that the animal of the Polythalamia has no empty cells, but that all its cells are simultaneously occupied, is of particular importance in their systematic arrangement among other animal bodies. Where there are many cells, they consist either of so many single animals, the whole constituting a polypary, or of organically filled integrant portions of one and the same individual forming groups. Both structures are foreign to the true Cephalopods. The shell-bearing Cephalopods may with Linnæus be divided into the *unilocular* and *multilocular*.

On the other hand, the want of a sipho which has been assigned as a character of Polythalamia, and from which they were named *Asiphonoidea* by De Haan, is incorrect, inasmuch as many really possess a part which may be fully compared to a sipho, if not in function, yet in form, namely, the tube which connects the separate cells of Nodosarina and of all individual many-celled forms. It is only in the *Miliolina* family among the simple Polythalamia, and it is only in the families of *Asterodiscina* and *Soritina* among those forming polyparies, that the want of a sipho is really necessary, because they live individually in single cells. But all the Nodosarina, Textularina, Uvellina, Rotalina, and Plicatilia among the simple Polythalamia, and the Frumentarina, Helicosorina, and Alveolinea among those which form polyparies, possess tubes of connexion between the cells, which very frequently resemble also in form the sipho of the Nautilus. D'Orbigny, it is true, states also that the cells of Foraminifers are connected by several openings; that, however, proceeds from an erroneous view, for such Polythalamia alone present several openings at the border of the cells, whose calcareous surface is interrupted in the form of a net-work, exhibiting often a relation analogous to that which is frequent in Madrepora and Astræa, in which the soft body is not divided or sharply cut off by compact calcareous plates, but the soft parts appear interwoven with minute calcareous rods, in a lattice-like manner. These numerous small connecting openings, which are sometimes visible in some of the Rotalia and Rosalina, and also in the Textularia, I do not consider essential, but hold that the true channel of connexion has always a large diameter, and is simple for each single animal. The erroneous view of d'Orbigny and of all his followers becomes so complicated, that polyparies are held to be single animals, and consequently the various connecting openings to be those of a simple individual.

With respect to d'Orbigny's genus *Nummulina,* although it has derived advantage from his diligent investigations, I consider it as composed of very heterogeneous elements, which belong to quite different divisions of animals. Some species of the sub-genus *Assilina,* and perhaps all of them, may belong to the families *Soritina* and *Asterodiscina,* while the *Assilina nitida* of the Red Sea is assuredly Forskål's *Nautilus Orbiculus,* that is, *Sorites Orbiculus.*

I am of opinion that all those species which are provided with visible traces of mouths or openings, as in Lamarck's genus *Lenticulina* with d'Orbigny's character of Nummulina, are to be received among the Polythalamia; but that all such species as have the form of a lens or disc, and are provided with internal spiral cells, but without a trace of such mouths, the cells being moreover separated from the external surface by thick calcareous layers, are to be considered as internal bones. These mouthless Nummulina are rather to be ranged with the Velellida of the Acalepha along with *Porpita,* where similar internally cellular coin-shaped bones exist. The considerable size of many Nummulina is also striking and foreign to Polythalamia, but agrees very well with the family of the Velellida, as also in the want of traces of the attachment of muscles, and in the want of a sipho or channel of connexion between the cells. Until better informed, therefore, I decidedly exclude the mouthless Nummulina from the Polythalamia, and retain only Lamarck's Lenticulina in the sense attached to d'Orbigny's Nummulina in a young state.

The distinctive character of the Polythalamia, when compared with their nearest relatives the *Flustra, Eschara, Cristatella,* &c., consists in the shell, and in their freedom of motion. But with this may be combined the power of attaching itself to other bodies, just as in the *Cristatella* (or *Hydra* also) which often remains long attached, and then creeps again. Those bodies which are apparently Polythalamian, but are really adherent and immoveable, belong to the *Cellepora, Flustra, Tubulipora,* and similar forms. The simplest Polythalamian form is the *Miliola* in Dujardin's sense, if there be really such self-existent animals, and they be not the young of others, or of many-celled forms most nearly related to Biloculina. And perhaps *Gromia oviformis* might be so viewed, should it not prove to be a *Difflugia* (an Infusoria). In this series I myself place provisionally, as doubtful, those numerous small globules of the sand of Rimini which have no distinct opening, or sometimes present a very minute one. The next simplest form is that of a simple straight row of cells, as in the *Nodosaria*, a jointed continued development of a simple body. *Textularina*, *Uvellina* and *Rotalina* (Lenticulina), may, as to external form, be viewed as Nodosarina developed in another manner, namely, in botryoidal or spiral forms.

I have here to make a remark that appears important. In the entire vast mass of known Polythalamia, a case or vestment prevails which is either cuticular or composed of a calcareous substance, while in Infusoria either a cuticular or siliceous substance prevails, so that hitherto no calcareous-shelled Infusoria nor siliceous-shelled Polythalamia had presented themselves. Yet among the fossil microscopic organisms of the chalk marl of Sicily, we find intermingled with the Infusoria shells bodies whose forms may be ranked with Polythalamia, namely, with Nodosarina, but the shells of which are siliceous, insoluble in acids, and which to the eye have a more transparent vitreous aspect than the calcareous shells when penetrated by balsam. I have hence been induced to place these siliceous-shelled forms, until a further knowledge may be acquired of their organization, among the polygastric Infusoria near the shelled Amoeba, in a separate family, under the name of *Arcellina composita*, or *Polycystina**. Such siliceous-shelled *Polycystina*, resembling calcareous-shelled Polythalamia, are the genera *Lithocampe*, *Cornutella* and *Haliomma*, with several species.

I wish here to draw attention to a small character hitherto unregarded, which is distinctive of true Polythalamia, and often even of their fragments. It consists in this, that in the tube or channel of connexion between the cells, the mouth of the tube which belongs to the earlier smaller cell is overgrown and surrounded by the succeeding larger cell. If the mouth of the last cell be prolonged in a beak-like form, we find in all the earlier smaller cells a distinct tube, quite similar to the hard remains of the sipho in the Nautilus; but so placed that the tube always projects forward from the smaller into the larger cell, and never backward from the larger into the smaller cell. In the Nautilus, this projection of the tube of connexion is reversed, always proceeding from the larger to the smaller chamber, so that in the last, the greatest chamber, the body of the animal thus acquires a smooth foundation, upon which it can move more freely. In true Nautili also the base of the cells is concave or undulated in the forward direction, while in the Polythalamia it appears without exception to be either quite straight or convex in that direction. This character was also observed by Fichtel and Moll.

---

* This view has been already indicated in the work "On the Infusoria as perfect organisms," 1838, p. 136.

The tabular view which I have given of the Bryozoa, founded as it is on the new observations which I have made, is drawn up with special regard to a definite expression of fossil phænomena, the ancient names of d'Orbigny being mostly retained. This very diligent precursor in these studies first laid down a foundation rich in forms and systematically ordered, which may serve for all future investigations, and has given names to families which are well adapted to his purpose; but these I have been obliged to alter, yet not arbitrarily, inasmuch as from the difference of our views it became necessary to separate from each other the forms which constitute his families, according as they are either simple Polythalamia, or Polythalamia composing polyparies.

Since the foregoing pages were drawn out, a newer work by Dr. Ehrenberg has made its appearance, embracing communications made to the Berlin Academy, on the continued researches of the author between September 1839 and August 1840, and bearing the title, "On the numerous Living Species of Animals found in the Chalk Formation*." Of this very interesting publication I had designed presenting an abstract, but having learned that a complete English edition of the work is about to appear† accompanied by the engravings, I now confine myself to a few notices immediately connected with the preceding part of this paper.

In this memoir Dr. Ehrenberg repeats his objections to the views entertained by MM. Alcide d'Orbigny and Dujardin. It has been seen, that to the Polythalamia, whose minute and often microscopic calcareous shells compose in inconceivable numbers, and in now nearly 1000 known different forms, the principal mass of chalk rocks and of many sands of the sea, M. d'Orbigny had several years since ascribed an external animal bearing the form of a Sepia, the small shell itself, which often resembles an Ammonite or Nautilus, being considered as the internal bone. On the other hand, at a later period, M. Dujardin denied that these animals possessed any organic structure, stating that they consisted simply of an animated slime capable of extension, encased by an indurated external shell, and associating them with the pseudopodian Amoeba of the Infusoria. Dr. Ehrenberg now further demonstrates, by figures and descriptions, their true organic structure, thus fully establishing his former positions, both as to simple Polythalamia and Polythalamia forming Polyparies. He proves that they are not internal bones, but external shells encasing a soft body, the shell being perforated, as it were, in all parts by numerous pores, from which the animal projects long filaments, capable at will of extension, retraction and bifid division, and productive of locomotion. The author further observes: M. Dujardin has, in August 1840, presented to the Paris Academy a *Mémoire sur une Classification des Infusoires en rapport avec leur organisation*, in which a new arrangement of the Infusoria is exhibited, and in this the Polythalamia are again introduced as *Rhizopodes* in association with *Amoeba* and *Actinophrys* of the Infusoria, forming a separate family. If, however, anatomical and physiological details are to be taken into account when we proceed to the systematic arrangement of different organic bodies, and we are not governed merely by the relations of external forms, M. Dujardin's arrangement cannot be deemed a happy one. He has in no case shown a polygastric structure in the Rhizopodes, and that it is not polygastric is proved anew by my investigations now communicated.

---

* *Ueber noch zahlreich jetzt-lebende Thierarten der Kreidebildung*, pp. 94, with four plates, Berlin, 1840.

† In the Scientific Memoirs of Mr. R. Taylor. Its publication cannot fail to prove very acceptable to British Naturalists in general.

It has been shown in a former part of this paper that Dr. Ehrenberg had recognized six species of Infusoria in the chalk formation, so closely resembling living species as not to be distinguished from them, and hence he was led to give to them the same names; namely, *Eunotia Zebra*, *Fragilaria rhabdosoma*, *Fragilaria striolata ?*, *Gallionella aurichalca*, *Navicula ventricosa*, and *Synedra ulna*. He had also referred, with a mark of interrogation, the following four species of calcareous-shelled Polythalamia to the white chalk, in which they are very extensively distributed, namely, *Globigerina bulloides*, *Globigerina helicina*, *Rosalina globularis*, and *Textilaria aciculata*, all of which were stated by M. d'Orbigny to have occurred in the living state only in the Adriatic Sea and the Ocean. If any doubt had existed as to the identity of all these fossil and living species, it has been completely removed by the later researches of Dr. Ehrenberg, by which the actual number of known species found in the chalk formation and in living state has been extended to fifty-seven, namely, of careous-shelled Polythalamia nine species, and of siliceous-lled Infusoria forty-eight species. The following is a list hese species and of the localities in which they occur, both the living and fossil state. In the fossil localities, W. C. nifies white chalk, C. M. chalk marl, and C. C. compact lk.

Calcareous-shelled Polythalamia.

	Living.	Fossil.
Globigerina bulloides	In the Adriatic Sea and the Ocean	W. C. Denmark.
—— helicina	——————	W. C. Cattolica.
Rosalina globularis .	——————	W. C. Gravesend.
Planulina (Synon. Rotalia) ocellata. .	North Sea, near Cuxhaven	W. C. Cattolica.
Rotalia globulosa . .	——————	W.C. in Russia, Poland, Prussia, Denmark, England, France and Sicily; and C. M. in Greece, Zante, Sicily and Oran.
—— stigma	——————	W. C. Cattolica. C. M. Caltasinetta.
—— (Synon. Planulina?) turgida . . .	——————	W. C. England, France, Prussia, Denmark. C. M. Oran. C. C. Egypt and Arabia.
Textilaria aciculata .	——————, and Adriatic and the Ocean	W. C. Prussia, Denmark, England and Sicily. C. M. Greece. C. C. Egypt and Arabia.
—— globulosa . . .	North Sea.	W. C. of all European countries, from Wolsk to Ireland. C. M. Sicily, Oran, and Greece. C. C. Egypt and Arabia.

Siliceous-shelled Infusoria.

Actinocyclus quinarius	North Sea, Tjörn Isle in the Cattegat	C. M. Caltasinetta.
—— biternarius . . .	North Sea, Tjörn	C. M. Oran and Caltasinetta.
—— senarius	North Sea, Cuxhaven, Christiania, Tjörn . . .	C. M. Oran, Caltasinetta, and Greece.
—— septenarius . .	North Sea in the Cattegat	C. M. Oran, Caltasinetta, and Zante.
—— octonarius . . .	——————	C. M. Oran and Caltasinetta.
—— nonarius	N. Sea, Cattegat near Tjörn.	C. M. Oran.
—— denarius	——————	C. M. Oran.
—— undenarius . . .	——————, and Bay of Christiania.	C. M. Oran and Zante.
—— bisenarius	Cattegat near Tjörn . . .	C. M. Oran.
—— quindenarius . .	—————— . . .	C. M. Oran.
Amphitetras antediluviana	—————— . . .	C. M. Oran and Greece

	Living.	Fossil.
21. Biddulphia pulchella.	Baltic, N. Sea, Mediterranean, and Ocean near Cuba	C. M. Greece.
22. Cocconema lanceolatum	Brackish and fresh waters.	C. M. Greece.
23. Coscinodiscus Argus.	North Sea, Cuxhaven . .	C. M. Caltasin Oran.
24. —— eccentricus . . .	————————, Tjörn in Cattegat, and Mexican Gulf, Vera Cruz.	C. M. Oran.
25. —— lineatus	North Sea, Cuxhaven . .	C. M. Caltasine
26. —— minor	———————— . .	C.M. Caltasine and Zante.
27. —— Oculus Iridis . .	———————— . .	C. M. Greece.
28. —— Patina	———————— . .	C. M. Zante.
29. —— radiatus	————————,and Baltic, Wismar.	C. M. Oran, Ca and Zante.
30. Dictyocha sulcata . .	North Sea near Tjörn . .	C. M. Caltasine Zante, and C
31. —— Fibula	N. Sea, Christiania and Tjörn, & Baltic, Wismar	C. M. Oran an netta.
32. —— Pentasterias . .	N. Sea, Christiania haven.	C. M. Zante.
33. —— Speculum . . .	N. Sea, Cuxhaven, Christiania and Tjörn, Baltic, near Kiel	C. M. Caltasine Zante, and C
34. Eunotia granulata . .	Brackish and fresh waters.	C. M. Greece.
35. —— Zebra	Berlin fresh waters	C. M. Greece.
36. Fragilaria rhabdosoma	Berlin, Halle, Copenhagen, Sweden	W. C. Gravese
37. —— striolata	. . . .	W. C. Gravese
38. Gallionella aurichalca	Berlin fresh waters, Leipzig, Thuringia, Franconia, Würzburg, Stuttgart, and on rocks near the Faroe Isles	W. C. Rügen.
39. —— sulcata	North Sea, Cuxhaven . .	C. M. Caltasine Zante, and
40. Grammatophora africana	N. Sea, Heligoland, Tjörn	C. M. Oran.
41. —— angulosa	North Sea, Tjörn	C. M. Oran.
42. —— oceanica	Callao in Peru, Vera Cruz in Mexico, Tjörn in Cattegat, Wismar in Baltic, and the Mediterranean.	C. M. Oran.
43. —— undulata	Among marine Confervæ near Vera Cruz.	C. M. Greece.
44. Haliomma radians . .	North Sea, Cuxhaven . .	C. M. Greece.
45. Navicula Didymus. .	N. Sea, Cuxhaven, Baltic, Wismar	C. M. Caltasin
46. —— Entomon	N. Sea, Christiania haven.	C. M. Greece.
47. —— norwegica . . .	————————	C. M. Greece.
48. —— quadrifasciata .	————————, and Tjörn Isle.	C. M. Greece.
49. —— ventricosa	Paris, Berlin, Saxony, Bohemia, Buchtarma in Altai, and Irtysch.	C. M. Oran.
50. —— viridula	Berlin fresh waters, Weissenfels in Saxony, and Wismar in Mecklenburg.	C. M. Greece.

	Living.	Fossil.
51. Peridinium pyrophorum	Baltic, near Kiel	Flints of the W. C. near Gravesend, and Flints of the plain of North Germany near Delitzsch.
52. Striatella arcuata	Gulf of Flensburg, Breakers near Gothenburg.	C. M. Oran.
53. Synedra ulna	Baltic near Wismar, Berlin fresh waters, North of Germany, Denmark, Scotland, Holland, the Ural, and perhaps Isle of France, and Mascarene Isles	C. M. Oran.
54. Tessella Catena	Breakers near Gothenburg and Berlin waters.	C. M. Caltasinetta.
55. Triceratium Favus	North Sea, Cuxhaven	C. M. Greece.
56. Xanthidium furcatum	Berlin	Flints of W. C. Gravesend, and Flints of Delitzsch.
57. —— hirsutum	Peat waters near Berlin.	Flints of W. C. Gravesend, and Flints of Delitzsch.

Of these fifty-seven species, thirty belong to the geologically acknowledged chalk and its Sicilian marls. The remainder from Oran, Greece (probably Egina), and Zante, though perhaps from beds not equally well defined by relative position as chalk marls, yet occurring, as they do, with numerous decided calcareous and siliceous animals of the chalk,—the geological relations of these species may also be considered as firmly established.

These new discoveries naturally lead to the conclusion that we have now no very definite boundary between secondary and tertiary tracts, and that the first dawn or eocene period of the present living organic creation, must be sought for deeper than the chalk formation; a view that appears to be confirmed by the occurrence of a living *Trochus* below the chalk, of the *Paludina vivipara* and *Cyclas cornea* in the Weald Clay, and of the *Terebratula caput serpentis* in the Upper Oolite. But as this and other interesting conclusions and views entertained by the author will be shortly laid open to the reader, with a full detail of the progressive researches made, I shall not now enter further upon the important matter contained in the volume.

APPENDIX.

Closely connected with the preceding subjects is the
able Memoir of M. Alcide d'Orbigny, which has recent
peared, entitled, "On the Foraminifers of the White Cl
the Paris Basin*." The subjoined extracts may serve t
vey a view of the general scope of the work, which, pla
parallel with that of Dr. Ehrenberg, cannot but excite
ble interest in the mind of the reader.

Previously to entering upon the direct object of the M
M. d'Orbigny indulges in a few general reflections.

Let us, says the author, cast a rapid glance upon wh
existed and upon what still exists in nature, in refere
the Foraminifers. We have found them distributed th
the oolite series, extending from the lias to the upp
beds; but in the cretaceous system they appear still
numerously and more varied in their forms. The Neoc
beds, those of the gault and the green sand, contain
but in proportion as we ascend from the lower to the
strata, they increase infinitely. In these latter the roc
be said to be often composed of them, and, as an ex
we may mention the largest of the Pyramids of Egyp
the white chalk the number is nearly as great as in tho
in which they now most abound. In a word, we have
Foraminifers in the cretaceous basins of the Seine, the
the Gironde, and of the whole South of France,
Belgium.

If we pass to the tertiary tracts, a whole world is
to us. The multiplied Foraminifers which appear
basins of Paris, Bourdeaux, Touraine, Italy, Austria
many, England, and Belgium, often form there the
part of the mass. A bed of considerable thickness
environs of Gentilly, near Paris, is entirely composed o
the Foraminifers being in contact with each other, s
united by a slight cement. In a cubical inch of the r
have found *fifty-eight thousand*, which is equal to *thre
sand millions* in a metre, and shows what myriads ma
in the Paris basin. These small bodies, which we th
forming entire beds in the lowest portions of the tertiary
are not less common in the higher stages; for in Austr

---

* *Mémoire sur les Foraminifères de la Craie Blanche du Basin*
in the 4th vol. part 1 of the Transactions of the Geological So
France, 1840.

the environs of Sienna in Italy, they often constitute one-ixth of the fossil mass; they are also extensively distributed the Crag of England[*] and of Belgium. So much in refer- nce to what has existed; let us now throw a glance upon hat which exists.

We are in the present day acquainted with Foraminifers rom every region of the sea, and we know that they exist in xtent from the equator to the frozen portions of continents. f we judge of the important part they play by their numbers n certain quarters, it will be impossible to doubt that their emains form the greater part of the banks of sand which im- ede navigation, obstruct gulfs and straits, fill up ports, and orm with corals those isles which are daily rising in warm egions from the bosom of the ocean.

Thus these minute shells, which, anterior to our epoch, ave assisted in leveling basins of immense extent, and in orming mountains, are now still constantly changing the epth of coasts and modifying the bottom. This view of their gency in nature is doubtless sufficient to prove the import- nce which attaches to their study.

We will add, that the comparative study of the fossil Foraminifers of all beds has proved to us a fact important to geology, namely, that each bed has its characteristic species, which serve to distinguish it, let the circumstances be what hey may; and as these minute shells are infinitely more common than those of Mollusks, the knowledge to be derived rom them is so much the more certain, and becomes extremely nteresting.

Another fact no less curious has been demonstrated to us by the study of living species from every region of the globe[†]. Many genera are peculiar to the hottest zones of continents, while others, on the contrary, are found only in temperate or cold regions. Hence the geographical distribution of iving genera and species offers to us a means of comparison of the highest importance with a view to the determination of he temperature of the waters in which fossil species lived, and may lead to very satisfactory results in geology, if we may judge by the fruits of our observations in this respect.

---

[*] Mr. Lyell has communicated to us the species which he discovered in the Crag.

[†] We are acquainted at present with nearly *fifteen hundred* living and fossil species of Foraminifers; and how many important facts may be derived from the study of these small bodies may be seen in three works which we are now publishing: 1. the Fauna of the Antilles, printed in *l'Histoire politique, physique, et naturelle de l'Ile de Cuba*, by M. de la Sagra; 2. that of the Canaries, published in *l'Histoire Naturelle* of those islands, by MM. Webb and Berthelot; 3. the Fauna of the southern extremity of America, forming a part of our *Voyage dans l'Amérique Méridionale*.

We could have desired to establish some general facts of much greater extent, founded on new observations recently made by us on the class of the Foraminifers; but the present occasion not admitting such an extension, let us pass to the Foraminifers of the white chalk of the Paris basin.

The geological position of the white chalk of Paris is so well known that we have not thought it necessary to speak of it; yet, if we seek to determine its position relatively to the other cretaceous beds by means of the Foraminifers it contains, compared with living species, the *facies* of the genera and species proves to us, that the chalk of Maestricht, of Fauquemont (Belgium), of Tours, of Chavagne, and of Vendome, is above it; while, on the contrary, all the other beds are below it; thus in the chalk of Maestricht and the upper beds of the basins of the Loire, we recognize only genera still existing, or at least occurring in tertiary tracts, while the white chalk of the Paris basin already exhibits to us different genera, such as *Flabellina*, *Verneuilina*, and *Gaudryina*, and a great number of species quite distinct.

It would therefore be easy to establish, by means of the Foraminifers alone, the relative antiquity of the cretaceous beds; but we must previously make two geographical sections quite independent of each other, founded on the zoological forms; the first comprising the entire basin of the Seine, of the Loire, of Belgium, and of England, in which we find a striking analogy between the species found in all the beds, from the lowest to the highest, with a regular passage from one to the other; the second, comprising the West and South of France, in which the species of Foraminifers have not only no analogy with those of the other section, but in which, moreover, almost all the genera are different. If we seek an example of this fact, we shall find it on comparing the green sand of the environs of Mans with that of the mouth of the Charente. The first, which in fact contains species approximating to those of the white chalk of Paris, contains already several species analogous to those which have lived up to that bed; while the second, with perfectly distinct species, presents to us genera different from all that we know in the cretaceous beds of the North of France and of Belgium.

The Foraminifers are sufficient to establish the following descending order of superposition in the cretaceous beds :—

Group of the North of France and of Belgium.	*Group of the West and South of France.*
pper chalk of Maestricht and Fauquemont (Belgium).	
oral chalk of Valognes and Nehou.	
oral chalk of the basin of the Loire, at Vendome (Loir and Cher), at Chavagne (Maine and Loire), at Tours (Indre and Loire).	
hite chalk of Ciply (Belgium).	
hite chalk of Paris, of the departments of Yonne and Aube, and of England.	
	Nummulite chalk of Royan (Charente Inférieure), of Saint Martory (Haute Garonne), of Saint Gaudens, &c.
	Coral chalk of Saintes (Charente Inférieure).
halk marl of the Loire, with *Gryphæa columba.*	Ammonite chalk of Martrons, near Rochefort (with *Gryphæa columba*).
	Caprine chalk of the Isle of Aix, of the Corbières (Aude).
reen sand of Mans (Sarthe).	Green sand of Fouras, of the Isle of Aix, and Corbières.
ault of the environs of Troyes (Aube).	
eocomian tract of Aube.	

To establish zoologically what we have advanced, let us ass in review the succession of the genera, and endeavour to onvey an idea of the modifications which have taken place in he Foraminifers of the cretaceous system, in the ascending rder of the beds.

At the epoch of the Neocomian formation we have hitherto ound only the genus *Textularia*.

The green sand presents, as we have said, two series of enera nearly distinct. That of the mouth of the Charente ontains the genera *Dentalina*, *Cristellaria*, *Lituola*, *Alveolina*, *hrysalidina*, and *Cuneolina*; that of Mans, the genera *Den-alina*, *Citharina*, *Frondicularia*, *Flabellina*, *Cristellaria*, *Bu-imina*, and *Guttulina*. Hence we see, that, with the excep-ion of two genera common to both localities, all the rest are ifferent in each of them.

If we follow our examination of the succession of genera in he cretaceous groups of the South and the North, we shall ind—

1. That in the South the same genera of the green sand re reproduced in the Caprine chalk. By degrees they pre-ail at length in the upper beds, and are reduced to the *Cris-ellaria* alone in the environs of Saintes; but near the mouth f the Gironde (at Royan) they are accompanied by the enera *Nummulina* and *Guttulina*, as well as on the whole ine of the foot of the Pyrenees, at Saint Martory, at Saint audens, extending into the department of Aude; thus pre-

senting a zone well characterized by the abundance of
mulina, of which we have not found the analogue in the
ceous beds of the North of France.

2. That in the North the succession is far from taking
in the same manner; and that the Foraminifers, in
greater numbers, present a larger suite in superposition,
facts not less curious. The genus *Citharina*, which c
tutes the greatest portion of the species in the oolite f
tion, ceases with the green sand of Mans, being fou
further in the cretaceous beds. In the chalk marl of the
of the Loire we meet for the first time with the genus *L*
with the *Dentalina*; but all at once, in the white cha
observe a great number of species, among which, with a
genera and even some analogous species of the green sa
Mans, there appear for the first time on the globe the g
Nodosaria, *Marginulina*, *Valvulina*, *Rotalina*, *Rosalina*,
catulina, *Uvigerina*, *Verneuilina*, *Gaudryina*, *Globigerin*
rulina, *Sagrina*, *Flabellina*, and *Frondicularia*. These g
contain a considerable number of species; but with the
chalk the genus *Flabellina* ceases, which had continued hi
from the green sand, and the genera *Verneuilina* and
dryina, which first appear in the white chalk, also term
with it; while in its interior the Frondicularia abound, a
as species whose cells form a pile on a single line.

The white chalk of Ciply, although contemporaneou
that of the Paris basin, since it also contains Flabellina
not present the same species, and may perhaps be a
higher in the series, but we have not as yet sufficient d
enable us to affirm this fact.

In the beds which we consider higher in the series
the white chalk of Paris, namely, in the coral chalk of T
of Chavagne, and of Vendome, we meet for the first
with the genera *Polystomella*, *Polymorphina* and *Glob*
yet accompanied with the same genera as those of the
chalk, with the exception of those whose discontinuan
have noticed; again, in the upper chalk of Maestrich
Fauquemont we have, with the three genera just menti
also the genera *Nonionina*, *Faujasina*, and *Heterost*
All are found living at present, or at least occurring i
tiary tracts; but we arrive at the last beds of the creta
group without having seen a single species of the *Mili*
Lamarck (our order of Agathistègues), which, as we a
tained in 1825, only commences with the tertiary beds
may be considered as the most certain sign of a chan
formation.

This rapid survey shows that in ascending from the to the higher beds of the cretaceous group, the genera and species of Foraminifers progressively increase, and that the forms, at first very simple, analogous to those of oolitic tracts, afterwards more complicated and specially appropriate to the lower beds of the cretaceous system, have at last been replaced in the upper parts by forms still more varied, the whole recurring in tertiary tracts, and even in the living state; facts which it has appeared to us important to establish in the history of Palæontology.

M. A. d'Orbigny then proceeds to describe the species of Foraminifers found by him in the white chalk of the Paris basin. The following is a list of them, together with their localities:—

		Localities.
1.	Nodosaria limbata	Meudon: very rare.
2.	Dentalina aculeata	Common at Sens: more rare at Meudon and in England.
3.	——— communis	Meudon: rare. Its analogue is found fossil in the Subapennine tracts of Italy and Austria, and living in the Adriatic.
4.	——— gracilis	At Sens and in England.
5.	——— nodosa	Common at Sens, more rare at Meudon and St. Germain.
6.	——— Lorneiana	Only in the environs of Sens.
7.	——— sulcata	Very common at Sens, Meudon, and St. Germain, and in the chalk of England. Found also in the green sand of the environs of Mans (Sarthe).
8.	——— multicostata	Sens, St. Germain: rare. Also at Maestricht rarely.
9.	Marginulina trilobata	Common at Sens, very rare at Meudon, St. Germain, and in England: found only in the young state.
10.	——— compressa	Meudon: very rare. Occurs also in the green sand in the environs of Mans.
11.	——— elongata	Common near Sens, very rare at Meudon and St. Germain. Occurs also in the chalk of Ciply.
12.	——— gradata	Only near Sens.
13.	——— raricosta	Meudon: very rare.
14.	Frondicularia radiata	Meudon and St. Germain: very rare.
15.	——— elegans	Meudon and Sens: very rare.
16.	——— Verneuiliana	Common at Sens, on the banks of the Yonne; rare at St. Germain and Meudon.
17.	——— Archiaciana	Meudon and Sens: rare.
18.	——— ornata	Found only once at Meudon.
19.	——— tricarinata	Environs of Sens: seems to be rare.
20.	——— angulosa	Meudon: very rare.
21.	Flabellina rugosa	Sens and Meudon: common.
22.	——— Baudouiniana	Only at Sens.
23.	——— pulchra	Meudon: very rare.
24.	Cristellaria rotulata	Very common in the white chalk of Meudon, St. Germain, Sens, and in England. Occurs also in the green sand near Mans.
25.	——— navicula	Sens and Meudon: rare.
26.	——— triangularis	Sens: very rare.
27.	——— recta	Meudon and St. Germain: rather rare.

	Localities.
28. Cristellaria Gaudryana	Only at St Germain : rare.
29. Lituola nautiloidea	Very common at Sens in the complete St. Germain only young, and adu rarely at Meudon. Occurs also in th of England.
30. Rotalina Voltziana	Very common at Meudon, St. Germain England.
31. ———— Micheliniana	Common at St. Germain, Meudon, England ; rare at Sens.
32. ———— umbilicata	Common at Meudon and St. Germain ; Sens and in England ; common als tertiary tracts of Austria. Its anal found living at Rimini in the Adriat being no difference between the fo living species.
33. ———— crassa	St. Germain, Meudon, and England rare.
34. ———— Cordieriana	St. Germain, England, and upper c Maestricht.
35. Globigerina cretacea	St. Germain and England.
36. ———— elevata	Common near Sens ; rare in England.
37. Truncatulina Beaumontiana	Meudon and England : rare.
38. Rosalina Lorneiana	Common at St. Germain and Meudon ; Sens and in England.
39. ———— Clementiana	Rare at St. Germain, more common in E
40. Valvulina gibbosa	St. Germain : rare.
41. Verneuilina tricarinata	St. Germain and Sens : rather rare.
42. Bulimina obtusa	Very common at Meudon ; rare at St. and in England.
43. ———— obliqua	Very common at Meudon, St. Germai and in England.
44. ———— variabilis	Very common at Sens ; rare at Meud Germain, and in England.
45. ———— brevis	Very common at Meudon, St. Germa Sens.
46. ———— Murchisoniana	St. Germain and England : rare.
47. Uvigerina tricarinata	Sens : very rare.
48. Pyrulina acuminata	Very rare at Sens and St. Germain ; ve mon at Meudon.
49. Gaudryina rugosa	Meudon, St. Germain, and Sens : rath mon.
50. ———— pupoides	Rather common at Meudon, Sens, St. G and in England.
51. Textularia trochus	Only at Meudon.
52. ———— turris	Sens, Meudon, St. Germain, and E without being common.
53. ———— Baudouiniana	St. Germain and Meudon : rare.
54. Sagrina rugosa	St. Germain and Meudon.

From the preceding list it appears, that of the fifty species found in the white chalk of the Paris basin, *t* *eight* occur at Meudon, *thirty-three* at Saint Germain *twenty-eight* at Sens: of these numbers, *nine* are pecul Meudon, *two* to Saint Germain, and *six* to Sens, whi the others are simultaneously common to two or three l ties, thus proving the perfect identity of the beds. It v seen also, that of these fifty-four species, *twenty-two* are mon to the white chalk of England also.

Of the fifty-four species, *seven* occur also in lower or higher beds: thus in the green sand of Mans are found three species, *Dentalina sulcata*, *Marginulina compressa*, and *Cristellaria rotulata*; in the coral chalk of Tours, which is higher in position than the white chalk, two species, *Bulimina obtusa* and *Textularia turris*; and in the chalk of Maestricht, being the highest in position, two species, *Dentalina multicostata* and *Rotalina Cordieriana*. We also find two species, the analogues of which occur both fossil in the tertiary tracts of Austria and Italy, and in the living state in the Adriatic, namely, *Dentalina communis* and *Rotalina umbilicata*. With these exceptions there still remain *forty-seven* species peculiar to the white chalk, showing clearly that it forms a bed distinct from all the rest of the cretaceous system, belonging to a small local fauna well-defined.

On comparing the above genera given by M. d'Orbigny with those named by Dr. Ehrenberg in his tabular view of the Bryozoa, inserted in the early part of this paper, it will be seen that Nodosaria, Dentalina, Marginulina, Frondicularia are included in the family of the Nodosarina of the latter author; Cristellaria, Rotalina, Truncatulina, included in his family of the Rotalina; Globigerina, Rosalina, Valvulina, Bulimina, Uvigerina, Pyrulina, in his family of the Uvellina; and Textularia in his family of the Textularina. The *Lituola nautiloidea* of Lamarck and d'Orbigny is the *Coscinospira nautiloides* of Ehrenberg, included in the Fabularina family of the latter.

If we now, observes M. d'Orbigny, compare the fauna of the Foraminifers of the white chalk with those of different seas, with a view of determining the analogy of composition, and of obtaining data respecting the temperature of that basin at the time when these species lived, we shall find this analogy more striking in the Adriatic Sea than anywhere else. There only, the same as in the chalk, are found in abundance Nodosaria, Dentalina, Marginulina, Frondicularia; there only occur a considerable number of species of Bulimina. This sea alone in the present day contains living Frondicularia; of Frondicularia so varied in the white chalk; and, to complete the approximation, it exhibits to us the only two living species, the analogues of which are found in the fossil state in the white chalk, namely, *Dentalina communis* and *Rotalina umbilicata*. This analogy of zoological forms would lead us to believe, 1st, that the basin in which is deposited the white chalk of Paris was subject to a warm temperature; 2nd, that it was circumscribed, protected from waves and from every violent current proceeding from a distance, since the bodies are deposited there without having experienced the slightest wearing previous to their becoming fossil; 3rd and lastly, that it extended to the whole of the white chalk of England.

Concluding Remarks.

The preceding extracts from the labours of Dr. Ehrenberg and M. A. d'Orbigny show that microscopic Polythalamia are found in all calcareous formations from the lias upward; but in England they have been lately discovered in still deeper strata. Mr. Tennant was, I understand, the first to announce their discovery in 1839 in the mountain limestone of England. In 1840 they were also met with in the Kendal limestone, from which Mr. Lonsdale has prepared thin slices mounted on glass, which appear transparent under a strong light, exhibiting the crowded state of the microscopic Polythalamia in great perfection. Mr. Bowerbank also has been led to turn his attention to this subject by examining the siliceous bodies of the chalk, green sand, and oolites*.

I had written thus far, when an interesting article by the Rev. Dr. Buckland, in reference to the researches of Dr. Ehrenberg up to 1839, met my eye, entitled, "On the agency of Animalcules in the formation of Limestone†," which notices in particular the researches of MM. Tennant and Darker on this subject in the Derbyshire limestone and the Stonesfield slate, as well as the labours of Mr. Bowerbank, referred to above, and conveying judicious reflections. Dr. Buckland justly remarks, that in the application of the microscope from the living to the fossil Infusoria and Foraminifers we are commencing a new and important era in Palæontology. A very interesting branch of the inquiry will be to ascertain whether these microscopic bodies retain throughout a distinctive character in the several formations into whose composition they enter. In the unbounded field of nature presented to the consideration of the Microscopical Society of London lately established, no subject appears more worthy of their attention than an examination of the microscopic organic constituents of all the older limestone formations of the British Isles, as well as of other countries; and it is much to be desired that this attention may not be wanting, although the concurrence of many labourers may be required to reap a harvest of great promise, yet of indefinite extent.

---

* Proceedings of the Geological Society, March 11, 1840.

† Edinburgh New Philosophical Journal, January to April, 1841.

LXXIV. *Second Letter to* Prof. Faraday, *from* ROBERT HARE, *M.D., Professor of Chemistry in the University of Pennsylvania*[*].

MY DEAR SIR,

39[†]. IN the month of July last I had the pleasure to read, in the American Journal of Science, your letter in reply to one which I had addressed to you through the same channel. I should sooner have noticed this letter, but that meanwhile I have had to republish two of my text books, and, besides, could not command, until lately, a complete copy of all those numbers of your researches to which you have referred.

40. The tenor of the language with which your letter commences, realizes the hope which I cherished, that my strictures would call forth an amicable reply. Under these circumstances, it would grieve me that you should consider any part of my language as charging you with inconsistency or self-contradiction, as if it could be my object to put you in the wrong, further than might be necessary to establish my conception of the truth. Certainly it has been my wish never to go beyond the sentiment "amicus Plato, sed magis amica veritas." I attach high importance to the facts established by your "Researches," which can only be appreciated sufficiently by those who have experienced the labour, corporeal and mental, which experimental investigations require. I am, moreover, grateful for the disposition to do me justice, manifested in those researches; yet it may not always be possible for me to display the deference, which I nevertheless entertain. I am aware that when, in a discussion, which due attention to brevity must render unceremonious, diversities of opinion are exhibited, much magnanimity is requisite in the party whose opinions are assailed; but I trust that both of us have truth in view above all other objects, and that so much of your new doctrine as tends to promote that end, will not be invalidated by a criticism, which, though free, is intended to be perfectly fair.

41. In paragraph 11 your language is as follows:—"*My theory of induction makes no assertion as to the nature of elec-*

---

[*] Communicated by the Author.

[†] As originally printed for the American Journal of Science, the paragraphs of my first letter to Prof. Faraday were not numbered; but as numbers were attached to the paragraphs in the republication of it in the London and Edinburgh Philosophical Magazine and Journal [vol. xvii. p. 44], I have directed them to be attached to this, my second letter, in due succession.

tricity, nor at all questions any of the theories respecting
subject."

42. Owing to this avowed omission to state your opi
as to the nature of electricity, as preliminary to the stat
of your "*theory,*" and because I was unable to reconcil
theory with those previously accredited, I received the im
sion that you claimed no aid from any imponderable prin
It appeared to me that there was no room for the agen
any such principle, if induction were an *action* of *conti*
ponderable particles consisting of a species of polarity
seemed to follow that what we call electricity, could b
thing more than a polarity in the ponderable particles, di
caused by those mechanical, or chemical frictions, move
or reactions, by which ponderable bodies are electrified.
have correctly inferred that I had not seen the fourt
series of your researches, containing certain paragraphs
From them it appears that the polarity, on which so
stress has been laid, is analogous to that which has long
known to arise in a ponderable body, about which the el
equilibrium has been subverted by the inductive influe
the electricity accumulated upon another such body. T
clearly explained in paragraph 4 of your letter, by the
tration, agreeably to which three bodies, A, B, C, are
ated in a line, in the order in which they are named, in
imity, but not in contact. "A is electrified positively
then C is uninsulated." It is evident that you are corr
representing that, under these circumstances, the extremi
B will be oppositely excited, so as to have a reaction wit
similarly excited body, analogous to that which takes
between magnets; since the similarly excited extremit
two such bodies would repel each other, while those di
larly excited would be reciprocally attractive. Henc
doubt, the word polarity is conceived by you to conv
idea of the state of the body B. If I may be allowed to
pose an epithet to convey the idea which I have of the
of a body thus electrified, I would designate it as an el
polar state, or as a state of electro-polarity.

43. It does not appear to me, that in the suggestion o
electro-polarity, which we both conceive to be induced
the body B (4), so long as it concerns a mass of ponde
matter, there is any novelty. The only part of your do
which is new, is that which suggests an analogous state
caused in the particles of the bodies through which the in
ive power is propagated. Admitting each of the partic
a dielectric, through which the process of ordinary indu
takes place, to be put into the state of the body B, it do

ppear to me to justify your definition of electrical induction. think that, consistently with your own exemplification of hat process, you should have alleged ordinary induction to e *productive* of an *affection* of particles, *causing* in them a pecies of polarity. In the case of the bodies A, B, C (4), B is evidently passive. How then can we consider as active, articles represented to be in an analogous state? If in B here is no action, how can there be any action in particles erforming a perfectly similar part? Moreover, how can the nductive power of an electrical accumulation upon A *consist* f the polarity which it induces in B?

44. Having supposed (8) an electrified ball, A, an inch in iameter, to be situated within a thin metallic sphere, C, of a oot in diameter, you suggest, that where one thousand conentric metallic spheres interpose between A and the inner urface of C, the electro-polar state of each particle in those pheres would be analogous to that of B, already mentioned. f course, if there be any action of those particles, there must e an action of B; but this appears to me not only irreconilable with any previously existing theory, but also with your wn exposition of the process by which B is polarized.

45. Supposing concentric metallic hemispheres to be interosed only upon one side of A, you aver that, agreeably to our experience, more of the inductive influence would be xtended towards that side of the containing shell than before 14). Admitting this, I cannot concede that the greater inuence of the induction, resulting from the presence of the etallic particles, is the consequence of any *action* of theirs, hether in *contiguity* or in *proximity*. Agreeably to my view, e action is confined to the electrical accumulation in the phere A. Between the electricity accumulated in this sphere, nd that existing in or about the intervening ponderable partiles, there may be a *reaction*; but evidently these particles are s inactive as are the steps of a ladder in the scaling of a wall.

46. Suppose a powerful magnet to be so curved as to have e terminating polar surfaces parallel, and to leave between em an interval of some inches. Place between these surces a number of short pieces of soft iron wire. These would, f course, be magnetized, and would arrange themselves in ows, the north and south poles becoming contiguous. Would is be a sufficient reason for saying that the inductive influnce of the magnetic poles was an *action* of the contiguous ires? Would not the phænomena be the consequence of n *affection* of the contiguous pieces of wire, not of their *action*?

47. As respects the word charge, I am not aware that I ave been in the habit of attaching any erroneous meaning to

it, as your efforts to define it in paragraph 3 would imply.
have been accustomed to restrict the use of it to the case w
you distinguish as an inductive charge, illustrated by tha
the Leyden jar. To designate the states of the conducto
a machine, I have almost always employed the words *exc*
or *excitement*. In my text book these words are use
designate the state of glass or resin electrified by friction, w
that of coated surfaces, whether panes or jars, inducti
electrified, has been designated by the words *charge* or *char*

48. I understood the word contiguous to imply contact
contiguity, whereas it seems that it was intended by yo
convey the idea of proximity. In the last-mentioned sen
is not inconsistent with the idea of an action, at the dist
of half an inch: but by admitting the word contiguous t
ill chosen, you have with great candour furnished me with
apology for having mistaken your meaning.

49. Any inductive action which does not exist at sens
distances (20) you attribute to *ordinary* induction, conside
the case of induction through a vacuum as an *extraordin*
case of induction. To me it appears that the induction r
be the same in both cases, and that the *circumstances* u
which it acts are those which may be considered in the
case as *ordinary*, in the other *extraordinary*. Thus take
case cited in your reply (8, 9, 10). Does the interpositio
the spheres alter the character of the inductive power in
sphere A?

50. Either the force exercised by the charge in A is
that of gravitation, altogether independent of the influen
intervening bodies, or, like that of light, is dependent on
agency of an intervening matter. Agreeably to one doct
the matter, by means of which luminous bodies act, ope
by its transmission from the luminous surface to that illumi
agreeably to another doctrine, the illuminating matter ope
by its undulations. If the inductive *power* of electrified bo
be not analogous to gravitation, it must be analogous to
power by which light is produced, so far as to be depen
on intervening matter. But were it to resemble gravita
like that force it would be uninfluenced by intervening ma
If your experiments prove that electrical induction is liab
be modified by intervening matter, it is demonstrated th
its mode of operating it is analogous to light, not to gra
tion. It is then proved that, agreeably to your doct
electrical induction requires the intervention of matter;
you admit that it acts across a vacuum, and, of course,
without the presence of *ponderable* matter. Yet it requ
intervening matter of some kind, and since that matter is

ponderable, it must of necessity be imponderable. When light is communicated from a luminous body in the centre of an exhausted sphere, agreeably to the undulatory hypothesis, its efficacy is dependent on the waves excited in an intervening imponderable medium. Agreeably to your electro-polar hypothesis, the inductive efficacy of an electrified body in an exhausted sphere would be due to a derangement of electric equilibrium, by which an opposite electric state would be produced at the surface of the containing sphere from that at the centre (26, 27). This case you consider as one of extraordinary induction; but when air is admitted into the hollow sphere, or when concentric spheres are interposed, you hold it to be a case of ordinary induction. Let us then, in the case of the luminous body, imagine that concentric spheres of glass are interposed, of which the surfaces are roughened by grinding. In consequence of the roughness thus produced, the rays, instead of proceeding in radii from the central ball, would be so refracted as to cross each other. Of the two instances of illumination, thus imagined, would the one be described as *ordinary*, the other as *extraordinary radiation*? But if these epithets are not to be applied to radiation, wherefore, under analogous circumstances, are they applicable to induction? Wherefore is induction, when acting through a plenum, to be called ordinary, and yet, when acting through a vacuum, to be called extraordinary? In the well-known case of the refracting power of Iceland spar, light undergoes an *ordinary* and *extraordinary refraction*; not an *ordinary* and *extraordinary radiation*. The candle, of which, when viewed through the spar, two images are seen, does not *radiate ordinarily* and *extraordinarily*.

51. If there be occasionally, as you allege (21), large intervals between the particles of radiant heat, how can the distances between them resemble those existing between particles acting at distances, which are not sensible? The repulsive reaction between the particles of radiant caloric, as described by you (21), resembles that which I have supposed to exist between those of electricity; but I cannot conceive of any description less suitable for either, than that of particles which do not act at sensible distances.

52. Aware that the materiality of heat, and the Newtonian theory, which ascribes radiation to the projection of heat or light-producing particles, have been questioned, I should not have appealed to a doctrine which assumes both the materiality of heat and the truth of the Newtonian theory, had not you led the way; but, agreeably to the doctrine and theory alluded to, I cannot accord with you in perceiving any

similitude between the processes of conduction and r tion (21).

53. Consistently with the hypothesis that electricity i terial, you have shown that an enormous quantity of it exist in metals. To me it seems equally evident that, a ably to the idea that heat is material, there must exist in n a proportionably great quantity of caloric. The intense produced when wires are deflagrated by an electrical disch cannot otherwise be consistently accounted for. Agre to the same idea, every metallic particle in any metallic must be surrounded by an atmosphere of caloric; sin *changes* of dimensions, consequent to variations of temper can only be explained by corresponding variations i quantity of caloric imbibed, and in the consequent den the calorific atmospheres existing in the mass which u goes these changes*.

54. Such being the constitution of expansible bodies a ably to the hypothesis in question, it seems to me that th cess, by which caloric is propagated through them by *co tion*, must be extremely different from that by which transmitted from one part of space to another by *radi* In the one case, the calorific particle flies like a bulle jected from a gun, but with an inconceivably greater vel which is not sensibly retarded by the reflecting or refr influence of intervening transparent media: in the other

---

* I subjoin the language which I have held respecting the const of expansible solids, during the last twenty years.

"The expansion of matter, whether solid, liquid, or aëriform, by crease of temperature, may be thus explained:—

"In proportion as the temperature within any space is raised, th be more caloric in the vicinity of the particles of any mass containe space. The more caloric in the vicinity of the particles, the mor will combine with them; and in proportion to the quantity of calo combined, will they be actuated by that reciprocally repellent power, in proportion to its intensity, regulates their distance from each ot

"There may be some analogy between the mode in which each able atom is surrounded by the caloric which it attracts, and that i the earth is surrounded by the atmosphere; and as in the latter probably in the former, the density is inversely as the square of the d

"At a height at which the atmospheric pressure does not exceed to the square inch, suppose it to be doubled, and supported at creased pressure by a supply of air from some remote region; i evident that a condensation would ensue in all the inferior strata atmosphere, until the pressure would be doubled throughout, so a come at the terrestrial surface 30 pounds instead of the present p of 15 pounds? Yet the pressure at the point from which the chang be propagated would not exceed two grains per square inch.

"In like manner, it may be presumed that the atmospheres of are increased in quantity and density about their respective atom slight increase in the calorific tension of the external medium."

must be slowly imparted from one calorific atmosphere to
iother, until the repulsion, sustained on all sides, is in equi-
brio. It is in this way that I have always explained the fact
iat metals are bad radiators, while good reflectors*.

55. In paragraph 25 you allege that conduction of heat
iffers from electrical induction, because it passes by a very
ow process; while induction is, in its distant influence, si-
ultaneous with its force at the place of action. How then
n the passage of heat by conduction be "*a process precisely
ke that of radiation*" (21), which resembles induction in the
elocity with which its influence reaches objects, however
emote?

56. Although (21) you appeal to the "modern views re-
pecting radiation and conduction of heat," in order to illus-
ate your conception of the contiguity of the particles of
odies subjected to induction, yet (in 25) you object to the
ference which I had made to the same views, in order to
iow that the intensity of electro-polarization could not be
versely as the number of the polarized particles, interposed
etween the "inductric" surfaces. Let us then resort to the
se above suggested, of the influence of the surfaces of the

---

* I will here quote the rationale which has been given in my lectures
r the last twenty years:—

" Metals appear to consist of particles so united with each other, or with
loric, as to leave no pores through which radiant caloric can be projected.
ence the only portion of any metallic mass which can yield up its rays
radiation, is the external stratum.

" On the other hand, from its porosity, and probably from its not retain-
g caloric within its pores tenaciously as an ingredient in its composition,
arcoal opposes but little obstruction to the passage of that subtile prin-
le, when in the radiant form; and hence its particles may all be simul-
ieously engaged in radiating any excess of this principle with which a
ble affinity may have caused them to be transiently united, or in receiv-
g the rays emitted by any heated body, to the emanations from which
ey may have been exposed.

" We may account in like manner for the great radiating power of
rthenware and wood.

" For the same reason that calorific rays cannot be projected from the
erior of a metal, they cannot enter it when projected against it from
hout. On the contrary, they are repelled with such force as to be re-
cted without any perceptible diminution of velocity. Hence the pre-
inence of metallic reflectors.

" It would seem as if the calorific particles which are condensed between
se of the metal, repel any other particles of their own nature which may
liate towards the metallic superficies, before actual contact ensues;
erwise, on account of mechanical imperfection, easily discernible with
aid of a microscope, mirrors would not be as efficacious as they are
ind to be in concentrating radiant heat. Their influence, in this respect,
ems to result from the excellence of their general contour, and is not
portionably impaired by blemishes."

poles of a magnet upon intervening pieces of iron wire. 1679, 14th series, you suggest this as an analogous ca that of the process of *ordinary* electrical induction, whic have under consideration. Should there be in the one a thousand pieces of wire interposed, in the second, a hun will it be pretended that the intensity of their reciproca ductive reaction would be inversely as the number; so the effect of the last-mentioned number of wires wou *equivalent* to that of the first? Were intervals to be cr between the wires, by removing from among the numbe mentioned alternate wires, it would seem to me tha energy of their reciprocal influence would be diminishe only as the number of them might be lessened, but al they should consequently be rendered more remote.

57. If, as you suggest, the interposition of ponderabl ticles have any tendency to promote inductive influence there must be some number of such particles by whic effect will be best attained. That number being inter I cannot imagine how the intensity of any electro-po thus created in the intervening particles, can, by a dimi of their number, acquire a proportionable increase; an dently in no case can the excitement in the particles e that of the "inductric" surfaces, whence the derangem electrical equilibrium arises.

58. The repulsive power of electricity being admitte inversely as the squares of the distances, you correctly that the aggregate influence of an electrified ball B, si at the centre of a hollow sphere C, will be a constant tity, whatever may be the diameter of C. This is pe analogous to the illuminating influence of a luminous situated at the centre of a hollow sphere, which wo course receive the whole of the light emitted, whatever be its diameter; provided that nothing should be inte to intercept any portion of the rays. But in order to a the objection which I have advanced, that the diminut the density of a "*dielectric*" cannot be compensated consequent increase of inductive intensity, it must be in the case of several similar hollow spheres, in which numbers of electrified equidistant balls should exist, th influence of such balls upon each other, and upon the su of the spheres, would not be directly as the number balls, and inversely as the size of the containing Were gas-lights substituted for the balls, it must be e that the intensity of the light in any one of the spheres be as the number of lights which it might contain: no of your illustrations (8), above noticed, makes light an

trical induction obey the same law as respects the influence of distance upon the respective intensities.

59. From these considerations, and others above stated, I infer, that if electrical induction were an action of particles in proximity, operating reciprocally with forces varying in intensity with the squares of the distances, their aggregate influence upon any surfaces, between which they might be situated, would be proportionable to their number; and since experience demonstrates that the inductive power is not diminished by the reduction of the number of the intervening particles, I conclude that it is independent of any energy of theirs, and proceeds altogether from that electrical accumulation with which the inductive change is admitted to originate.

60. In paragraph 31, you say, "that at one time there was a distinction between heat and cold. At present that theory is done away with, and the phænomena of heat and cold are referred to the same class, and to different degrees of the same power."

61. In reply to this I beg leave to point out, that although, in ordinary acceptation, cold refers to relatively low temperature, yet we all understand that there might be that perfect negation of heat, or abstraction of caloric, which may be defined absolute cold. I presume that, having thus defined absolute cold, you would not represent it as identical with caloric. For my own part, this would seem as unreasonable as to confound matter with nihility.

62. Assuming that there is only one electric fluid, there appears to me to be so far an analogy between caloric and electricity, that negative electricity conveys, in the one case, an idea analogous to that which cold conveys in the other. But if the doctrine of Du Fay be admitted, there are two kinds of electric matter, which are no more to be confounded than an acid and an alkali. Let us, upon these premises, subject to further examination your argument (1330), that insulation and conduction should be identified, "*since the moment we leave in the smallest degree perfection at either extremity, we involve the element of perfection at the opposite end.*" Let us suppose two remote portions of space, one replete with pure vitreous electricity, the other with pure resinous. Let there be a series of like spaces, containing the resinous and vitreous electricities in as many different varieties of admixture, so that in passing from one of the first-mentioned spaces, through the series to the other, as soon as we should cease to be exposed to the vitreous fluid, in perfect purity, we should begin to be exposed minutely to the resinous; or that, in passing from the purely resinous atmosphere, we should

begin to be exposed to a minute portion of the vitreous
would this be a sufficient reason for confounding th
fluids, and treating the phænomena to which they give
the effect of one only ?

63. But the discussion into which your illustration
led me refers to things, whereas conduction and insulati
I understand them, are opposite and incompatible prop
so that, inasmuch as either prevails, the other must be
teracted. Conduction conveys to my mind the idea o
meability to the electric fluid, insulation that of *impermea*
and I am unable to understand how these irreconcilabl
perties can be produced by a difference of degree in an
property of electrics and conductors.

64. If, as you infer, glass have, comparatively with m
an almost infinitely minute degree of the conducting p
is it this power which enables it to prevent conduction,
other words, to insulate? Let it be granted that you
correctly supposed conduction to comprise both inductio
discharge, the one following the other in perfect cond
within an inexpressibly brief interval: insulation does no
vent induction, but, so far as it goes, prevents discharge
practice, this part of the process of conduction does no
place through glass during any time ordinarily allotted
experiments, however correct you may have been in supp
it to have ensued before the expiration of a year or mo
the case of the tubes which you had sealed after cha
them. But conceding it to have been thus proved, that
has, comparatively with metals, an infinitely small deg
the conducting power, is it this minute degree of cond
power which enables it to prevent conduction, or, in
words, to insulate?

65. Induction arises from one or more properties of
tricity, insulation from a property of ponderable matter
although there be no matter capable of preventing indu
as well as discharge, were there such a matter, would
annihilate insulation? On the contrary, would it not ex
the property in the highest perfection?

66. As respects the residual charge of a battery, is
evident that any electrical change which affects the surf
the glass, must produce a corresponding effect upon the
tum of air in contact with the coating of the glass?
place one coating between two panes, will it not enable
a certain extent, to charge or discharge both? Substit
the air for one of them, will it not in some measure be
to an affection similar to that of the vitreous surface, for
it is substituted? In the well-known process of the co

ing electrometer, the plate of air interposed between the discs s, I believe, universally admitted to perform the part of an lectric, and to be equivalent in its properties to the glass in coated pane.

67. When I adverted to a gradual relinquishment of elec- ricity by the air to the glass, I did not mean to suggest that t was attended by any more delay than the case actually de- nonstrates. It might be slow or gradual, compared with the elocity of an electric discharge, and yet be extremely quick omparatively with any velocity ever produced in ponderable natter. That the return should be slow when no coating vas employed, and yet quick when it was employed, as stated y you (38), is precisely what I should have expected, because he coating only operates to remove all obstruction to the lectric equilibrium. The quantity or intensity of the excite- nent is dependent altogether upon the electrified surfaces of he air and the glass. You have cited (1632) the property of charged Leyden jar, as usually accoutred, of electrifying a arrier ball. This I think sanctions the existence of a power o electrify, by convection, the surrounding air to a greater or ess depth; since it must be evident that every aërial particle nust be competent to perform the part of the carrier ball.

68. Agreeably to the Franklinian doctrine, the electricity lirectly accumulated upon one side of a pane repels that upon he other side. You admit that this would take place were a acuum to intervene; but when ponderable matter is inter- osed you conceive each particle to act as does the body B, vhen situated as described between A and C (4). But agree- bly to the view which I have taken, and what I understand o be your own exposition of the case, B is altogether passive, o that it cannot help, if it does not impede, the repulsive nfluence. Moreover, it must be quite evident, that were B emoved, and A approximated to C, without attaining the triking distance, the effect upon C, and the consequent energy f any discharge upon it from A, would be greater instead of ess. If, in the charge of a coated pane, the intermediate onderable vitreous particles have any tendency to enhance he charge, how happens it that, the power of the machine mployed being the same, the intensity of the charge which an be given to an electric is greater in proportion to its enuity?

69. In reference to the direction of any discharge, it appears o me that as, in *charging*, the fluid must always pass from the athode to the anode, so in reversing the process it must pur- ue the opposite course of going from the anode back to the athode. Evidently the circumvolutions of the circuit are as

unimportant as respects a correct idea of the direction, as their length has been shown by Wheatstone to be incompetent to produce any perceptible delay.

70. The dissipation of conductors being one of the most prominent among electrical phænomena, it appears to me to be an objection to your theory, if, while it fails to suggest any process by which this phænomenon is produced, it assumes premises which seem to be incompatible with the generation of any explosive power. If discharge only involves the restoration of polarized ponderable particles to their natural state, the potency of the discharge must be proportionable to the intensity of the antecedent polarity; yet it is through conductors liable, as you allege, to polarization of comparatively low intensity (31) that discharge takes place with the highest degree of explosive violence.

71. Having inquired how your allegation could be true, that discharge brings bodies to their natural state, and yet causes conductors to be dissipated, you reply (34), that different effects may result from the same cause, acting with different degrees of intensity, as when by one degree of heat ice is converted into water, by another into steam. But it may be urged, that although, in the case thus cited, different effects are produced, yet that the one is not inconsistent with the other, as were those ascribed to electrical discharges. It is quite consistent that the protoxide of hydrogen, which, *per se*, constitutes the solid called ice, should, by one degree of calorific repulsion, have the cohesion of its particles so far counteracted as to be productive of fusion; and yet that a higher degree of the same power should cause them to recede from each other, so as to exist in the aëriform state.

72. In order to found, upon the influence of various temperatures, a good objection to my argument, it should be shown that while a certain reduction of temperature enables aqueous particles to indulge their innate propensity to consolidation, a still further reduction will cause them, in direct opposition to that propensity, to repel each other so as to form steam.

73. In your concluding paragraph you allege, "*that when ponderable particles intervene, during the process of dynamic induction, the currents resulting from this source do require these particles.*" I presume this allegation is to be explained by the conjecture made by you (1729), that since certain bodies, when interposed, did not interfere with dynamic induction, therefore they might be inferred to cooperate in the transmission of that species of inductive influence. But if the induction takes place without the ponderable matter, is it right to assume that this matter *aids*, because it does not prevent

ie effect? Might it not be as reasonably inferred, in the case f light, that, although its transmission does not require the iterposition of a pane of glass, yet, that when such a pane is iterposed, since the light is not intercepted, there is reason suppose an active cooperation of the vitreous particles in id of the radiation? It may be expedient here to advert to ie fact, that Prof. Henry has found a metallic plate to interere with the dynamic induction of one flat helix upon another. have myself been witness of this result.

74. Does not magnetic or electro-dynamic induction take lace as well *in vacuo* as *in pleno*? Has the presence of any as been found to promote or retard that species of reaction? t appears that, agreeably to your experiments, ponderable odies, when made to intervene, did not enhance the influence question, while in some of those performed by Henry it 'as intercepted by them. Does it not follow that ponderable articles may impede, but cannot assist in this process?

75. I am happy to find that, among the opinions which I xpressed in my letter to you, although there are several in 'hich you do not concur, there are some which you esteem f importance, though you do not consider yourself justified extending to them your sanction, being constrained, in the resent state of human knowledge, to hold your judgment in uspense. For the present I shall here take leave of this subect, having already so extended my letter as to occupy too iuch of your valuable time. I am aware that as yet I have ot sufficiently studied many of the intricate results of your agacity, ingenuity and consummate skill in experimental inestigations. When I shall have time to make them the subect of the careful consideration which they merit, I may venure to subject your patience to some further trials.

Philadelphia, Jan. 1, 1841.

22, 263

XXV. *On the Principles of the Application of Analysis to the Motion of Fluids. By the Rev.* J. Challis, *M.A., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge*[*].

FOR the sake of simplicity, the following remarks will be restricted to the motion of an incompressible fluid: hey may without difficulty be extended to fluid motion in eneral.

It is well known that one of the fundamental equations of uid motion is obtained on the principle that the mass of each mall element remains the same while its position and form are varying. If u, v, w be the parts of the velocity point whose coordinates are x, y, z, resolved in the tions of the axes of rectangular coordinates, the equatio obtained for an incompressible fluid is,

---

[*] Communicated by the Author.

$$\frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0 \quad \ldots\ldots$$

Under this form it is not proper for application to pro instances of motion, for which purpose it must admit of or approximate integration, and therefore consist of differential coefficients of a principal variable. The formation into the required form is made by assuming $+ v\,dy + w\,dz$ to be an exact differential of a functi x, y and z. For if ϕ be the function, evidently

$$u = \frac{d\phi}{dx}, \quad v = \frac{d\phi}{dy}, \quad w = \frac{d\phi}{dz},$$

and

$$\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2} = 0 \quad \ldots\ldots$$

No general rule has hitherto been given for detern when it is allowable to assume $u\,dx + v\,dy + w\,dz$ an exact differential, nor has it been ascertained to wha ticular circumstance of the motion this analytical con refers. This must be considered a defect in the mathem theory of hydrodynamics.

The equation (A.) may also be arrived at by the foll considerations. In whatever manner a mass of fluid is i tion, we may conceive an unlimited number of surfac scribed in it, so that each is perpendicular to the direct the motion at a given time of the particles through wl passes. These surfaces are not necessarily continuou must be supposed to consist of continuous parts poss the general properties of curve surfaces. Hence if, in the surfaces separated by an indefinitely small interva indefinitely small rectangular areas be taken opposite t other, so that the lines joining the corresponding a points are normals to the surfaces, then the directions tion in the fluid element lying between them will, by a property of curve surfaces, all pass through two *focal* line pendicular to the directions of the normals, and situa planes at right angles to each other. The normal v will in general vary in passing from one point to anot the intersecting surface, in a manner depending on the trary disturbance of the fluid; but taking it to be u through the extent of each of the indefinitely small are continuity of the fluid will be maintained if the same qu

fluid passes the two areas at the same time. On this prin-
ole I have obtained the equation (A.) in the Cambridge
hilosophical Transactions (vol. v. part ii. p. 182). The
athematical reasoning is too long to be inserted. I have
cently obtained the corresponding equation for compressible
iids by a similar process.

The above method of obtaining the equation (A.) has the
lvantage of making known the condition that is introduced
ien $u\,dx + v\,dy + w\,dz$ is assumed to be an exact diffe-
ntial of a function of x, y, and z. For if V be the velocity
the point $x\,y\,z$ distant by r from a focal line, and θ, η be
o angles fixing the direction of the normal through that
int, we may put $V = f(\theta, \eta) \times \phi(r)$, the latter factor ex-
essing the law of the variation of V at a given instant
rough a small space resulting from the convergence or di-
rgence of the normals. Hence if α, β, γ be the coordi-
tes of the intersection of the normal with the focal line, we
all have

$$= f(\theta, \eta) \,.\, \phi(r) \,.\, \frac{x-\alpha}{r}; \quad v = f(\theta, \eta) \,.\, \phi(r) \,.\, \frac{y-\beta}{r};$$

$$= f(\theta, \eta) \,.\, \phi(r) \,.\, \frac{z-\gamma}{r}; \text{ and } u\,dx + v\,dy + w\,dz$$

$$f(\theta, \eta) \,.\, \phi(r) \left\{ \frac{x-\alpha}{r} dx + \frac{y-\beta}{r} dy + \frac{z-\gamma}{y} dz \right\}.$$

Now since $r^2 = (x-\alpha)^2 + (y-\beta)^2 + (z-\gamma)^2$, the above
iantity is an exact differential of a function of x, y, and z,
α, β, γ, θ, and η be constant; that is, if *the variation with*
spect to space be in the direction of the motion. This condi-
on, which has hitherto been unnoticed, is independent of the
rticular mode in which the fluid is disturbed, and being
troduced when $d\phi$ is substituted for $u\,dx + v\,dy + w\,dz$,
ust be attended to in all the subsequent calculations in
iich ϕ is involved.

The preceding reasoning fails if the lines of motion be not
nvergent, or the motion be such as the fluid might take if
were either wholly solid, or consisted of solid disconnected
rts: for instance, when the fluid mass revolves about a fixed
is and the velocity is a function of the distance from the
is. In fact, for this instance, $u\,dx + v\,dy + w\,dz$ is not
exact differential.

As it appears from the above reasoning that, in general, at
e same time that the equation (A.) is transformed into one
nsisting of partial differential coefficients of a principal va-
ible, the condition is introduced, that the variation with re-

spect to space is in the direction of the motion, it follows that the transformed equation and its integral containing arbitrary functions must be immediately applied to the parts of the fluid disturbed in a given and arbitrary manner, and where, consequently, the direction of the motion is known. The velocity, and direction of the velocity, at other parts are to be inferred from the laws of propagation of the motion and variation of the velocity which the integration reveals.

Admitting the extension of the foregoing considerations to a compressible fluid, the following is the manner in which the problem of the resistance to a vibrating sphere requires, on these principles, to be treated. The general equation in rectangular coordinates, viz.

$$\frac{d^2 \phi}{d t^2} = a^2 . \left(\frac{d^2 \phi}{d x^2} + \frac{d^2 \phi}{d y^2} + \frac{d^2 \phi}{d z^2}\right),$$

is first to be transformed into polar coordinates, having the centre of the vibrating sphere for the pole. The polar coordinates being r the distance of the point $x\, y\, z$ from the centre of the sphere, θ the angle which r makes with the straight line in which the centre is moving, and η the angle which the plane of these two lines makes with a vertical plane, regard also being had to the motion of the sphere, the transformation, effected in the manner adopted by Poisson in Art. 2 of his Memoir in the *Connaissance des Tems* for 1834, leads to the equation

$$\frac{d^2 . r \phi}{d t^2} = a^2 . \left\{ \frac{d^2 . r \phi}{d r^2} + \frac{1}{r^2 \sin \theta} . \frac{d . \left(\sin \theta \frac{d . r \phi}{d \theta}\right)}{d \theta} \right.$$
$$\left. + \frac{1}{r^2 \sin^2 \theta} . \frac{d^2 . r \phi}{d \eta^2} \right\},$$

which is independent of the motion of the sphere. If now the reasoning be just by which I have argued, that into this equation must be introduced the condition, that the variation with respect to space be in the direction in which the velocity is impressed, it will plainly reduce itself to

$$\frac{d^2 . r \phi}{d t^2} = a^2 . \frac{d^2 . r \phi}{d r^2};$$

in which, from what has been already said, ϕ may contain as a factor an arbitrary function of θ and η. This is the equation I have employed in the solution I have given of the problem in the Number of this Journal for December 1840, and by it I have been conducted to the value 2 for the coefficient of resistance. I find also that the part of the velocity directed to or from the centre of the sphere varies inversely as the *square* of the distance, and that in a plane drawn at any instant through the centre of the sphere perpendicular to the direction of its motion, the fluid is stationary. Poisson, by integrating the equation with the term containing the differential coefficient with respect to θ included, finds the coefficient of resistance to be $1\frac{1}{2}$, the law of variation of the velocity to be inversely as the *cube* of the distance, and the velocity of the fluid in the above-mentioned plane and in contact with the sphere, to be *half* the velocity of the sphere. With these statements I leave the problem to the consideration of mathematicians, not venturing, in a question of so much difficulty, to assert unhesitatingly the correctness of my own views.

As an objection may be raised against the reasoning in my communication in the December Number, because it takes account only of terms involving the first power of the velocity, which, by reason of the small factor $\frac{r}{\lambda}$, may not be of larger magnitude than rejected terms involving the square of the velocity, I take this opportunity of mentioning that, in a paper submitted to the Cambridge Philosophical Society, I have carried the approximation to the next degree, and, as might have been anticipated, I obtain the former result, the pressure depending on the square of the velocity being the same on the preceding as on the following half of the surface of the vibrating sphere. In the same paper I have attempted to give an answer to the query proposed at the end of the above-mentioned communication, respecting the motion of a small sphere submitted to the mechanical action of the vibrations of an elastic medium. I find that there will be a motion of vibration of the sphere depending on the first power of the velocity of the vibrating medium, and a permanent motion of translation depending on the second power. This result is important if it serves to explain how the vibrations of the æther (assuming them to be like those of air) may at the same time produce two different effects, such as light and heat, or light and chemical action.

Cambridge Observatory, April 13, 1841.

LXXVI. *Contributions to Electricity and Magnetism. No. IV. On Electro-dynamic Induction.* (*Continued.*) *By* JOSEPH HENRY, *LL.D., Professor of Natural Philosophy in the College of New Jersey, Princeton**.

Introduction.

1. IN the course of my last paper, it was stated that the investigations which it detailed were not as complete in some parts as I could wish, and that I hoped to develope them more fully in another communication. After considerable delay, occasioned by alterations in the rooms of the physical department of the college, I was enabled to resume my researches, and since then I have been so fortunate as to discover a series of new facts belonging to different parts of the general subject of my contributions. These I have announced to the Society at different times, as they were discovered; and I now purpose to select from the whole such portions as relate particularly to the principal subject of my last paper, namely, the induction at the beginning and ending of a galvanic current, and to present them as a continuation, and, in a measure, as the completion, of this part of my researches. The other results of my labours in this line will be arranged for publication as soon as my duties will permit me to give them a more careful examination.

2. In the course of the experiments I am about to describe, I have had occasion to repeat and vary those given in my last paper; and I am happy to be able to state, in reference to the results, that, except in some minor particulars, which will be mentioned in the course of this paper, I have found no cause to desire a change in the accounts before published. My views, however, of the connexion of the phænomena have been considerably modified, and I think rendered much more definite by the additional light which the new facts have afforded.

3. The principal articles of apparatus used in these experiments are nearly the same as those described in my last paper, namely, several flat coils and a number of long wire helices (III. 6, 7, 8.†). I have, however, added to these a constant battery, on Professor Daniell's plan, the performance of which has fully answered my expectations, and confirmed the accounts given of this form of the instrument by its author. It consists of thirty elements formed of as many copper cylinders, open at the bottom, each five inches and a half in height, three inches and a half in diameter, and placed in earthen

---

* From the Transactions of the American Philosophical Society, vol. viii., having been read June 19, 1840.

† When the numerals II. or III. are included in the parenthesis, reference is made to the corresponding Nos. of my contributions.

cups. A zinc rod is suspended in each of these, of the same length as the cylinders, and about one inch in diameter. The several elements are connected by a thick copper wire, soldered to the copper cylinder of one element, and dipping into a cup of mercury on the zinc of the next. The copper and zinc, as usual, are separated by a membrane, on both sides of which is placed a solution of one part of sulphuric acid in ten parts of water; and to this is added, on the side next the copper, as much sulphate of copper as will saturate the solution. The battery was sometimes used as a single series, with all its elements placed consecutively, and at others in two or three series, arranged collaterally, so as to vary the quantity and intensity of the electricity as the occasion might require.

4. The galvanometers mentioned in this paper, and referred to in the last, are of two kinds; one, which is used with a helix, to indicate the action of an induced current of intensity, consists of about five hundred turns of fine copper wire, covered with cotton thread, and more effectually insulated by steeping the instrument in melted cement, which was drawn into the spaces between the spires by capillary attraction. The other galvanometer is formed of about forty turns of a shorter and thicker wire, and is always used to indicate an induced current, of considerable quantity, but of feeble intensity. The needle of both these instruments is suspended by a single fibre of raw silk.

5. I should also state, that in all cases where a magnetizing spiral is mentioned in connexion with a helix, the article is formed of a long, fine wire, making about one hundred turns around the axis of a hollow piece of straw, of about two inches and a half long: also the spiral mentioned in connexion with a coil is formed of a short wire, which makes about twenty turns around a similar piece of straw. The reason of the use of the two instruments in these two cases is the same as that for the galvanometers, under similar circumstances; namely, the helix gives a current of intensity, but of small quantity, while the coil produces one of considerable quantity, but of feeble intensity.

SECTION I.—*On the Induction produced at the moment of the beginning of a Galvanic Current, &c.*

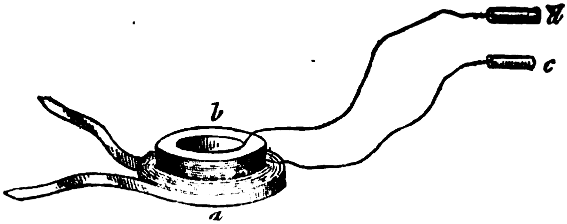
6. It will be recollected that the arrangement of apparatus employed in my last series of experiments gave a powerful induction at the moment of breaking the galvanic circuit, but the effect at making the same was so feeble as scarcely to be perceptible. I was unable in any case to get indications of currents of the third or fourth orders from the beginning in-

duction, and its action was therefore supposed to be so feeble as not materially to affect the results obtained.

7. Subsequent reflection, however, led me to conclude, that in order to complete this part of my investigations, a more careful study of the induction at the beginning of the current would be desirable; and accordingly, on resuming the experiments, my attention was first directed to the discovery of some means by which the intensity of this induction might be increased. After some preliminary experiments, it appeared probable that the desired result could be obtained by using a compound galvanic battery, instead of the single one before employed. In reference to this conjecture the constant battery before mentioned (3.) was constructed, and a series of experiments instituted with it, the results of which agreed with my anticipation.

8. In the first experiment, coil No. 2, which it will be remembered (III. 7.) consists of a copper riband of about sixty feet long, and coiled on itself like the main spring of a watch, was connected with the compound battery, and helix No. 1 (III. 8.), formed of one thousand six hundred and sixty yards of fine copper wire, was placed on the coil to receive the induction, as is shown in Figure 3, which is again inserted here for the convenience of the reader.

Fig. 3.



a represents coil No. 1, *b* helix No. 1, and *c*, *d*, handles for receiving the shock.

This arrangement being made, currents of increasing intensity were passed through the coil by constantly retaining one of its ends in the cup of mercury forming one extremity of the battery, and successively plunging the other end into the cups which served to form the connexions of the several elements of the battery. With the current from one element, the shock at breaking the circuit was quite severe; but at making the same it was very feeble, and could be perceived in the fingers only or through the tongue. With two elements in the circuit, the shock at beginning was slightly increased;

with three elements the increase was more decided, while the shock at breaking the circuit remained nearly of the same intensity as at first, or was comparatively but little increased. When the number of elements was increased to *ten*, the shock at making contact was found fully equal to that at breaking, and by employing a still greater number, the former was decidedly greater than the latter; the difference continually increasing until all the thirty elements were introduced into the circuit.

9. In my last paper, a few experiments are mentioned as being made with a compound battery of Cruickshank's construction; but from the smallness of the plates of this, and the rapidity with which its power declined, I was led into the error of supposing that the induction at the ending of the current, in the case of a short coil, was diminished by increasing the intensity of the battery (see paragraph 19. of No. III.); but by employing the more perfect instrument of Professor Daniell in the arrangement of the last experiment, I am enabled to correct this error, and to state that the induction at the ending remains nearly the same when the intensity of the battery is increased. If the induction depends in any degree on the quantity of current electricity in the conductor, then a slight increase in the induction should take place; since, according to theory, the current is somewhat increased in quantity, in the case of a long coil, by the increase of the intensity of the battery. Although very little, if any, difference could be observed in the intensity of the shock from the secondary current, yet the snap and deflagration of the mercury appeared to be greater from the primary current when *ten* elements of the battery were included in the circuit, than with a single one. The other results which are mentioned in my last paper, in reference to the compound battery, are, I believe, correctly given.

10. The intensities of the different shocks in the foregoing experiments were compared by gradually raising the helix from the coil (see Fig. 3.), until, on account of the distance of the conductors, the shock in one case would be so much reduced as to be scarcely perceptible through the fingers or the tongue, while the shock from another arrangement, but with the same distance of the conductors, would be evident, perhaps, in the hands. The same method was generally employed in the experiments in which shocks are mentioned as being compared, in the other parts of this paper.

11. Experiments were next made to determine the influence of a variation in the length of the coil, the intensity of the

battery remaining the same. For this purpose, the consisting of a single element, and the arrangement apparatus as represented in Fig. 3, the coil was dimini length from sixty feet to forty-five, then to thirty, and With the first-mentioned length the shock, at making with the battery, was, of course, very feeble, and coulc only in the tongue; with the next shorter length it wa perceptible, and increased in intensity with each dim of the coil, until a length of about fifteen feet appeared a maximum result.

12. The diminution of the intensity of the shock in experiment, after the length of the coil was diminishe fifteen feet, was due to the diminution of the number c of the coil, each of which, by acting on the helix to increase the intensity of the secondary current, un combined length of the whole is too great for the inte the battery. That this is the fact is shown by the fo experiment: the helix was placed on a single spire of the coil, and the length of the other part of the co band, which did not act on the helix, was continuall ened, until the whole of it was excluded from the circ this case the intensity of the shock at the beginning v stantly increased. We may therefore state generall at the beginning of the battery current, the inducti unit of its length is increased by every diminution length of the conductor.

13. In the experiment given in paragraph 11, the i of the shock at the *ending* of the battery current dir with each diminution of the length of the coil; an also due to the decrease of the number of the spire coil, as is evident from an experiment similar to the which the helix was placed on a coil consisting of o turns or spires of copper riband; the shock at the with this arrangement, was comparatively feeble, bu be felt in the hands. Different lengths of coil No. now introduced into the same circuit, but not so as t the helix; but although these were varied from fou feet to the whole length of the coil (sixty feet), not t difference in the intensity of the shock could be pe We have, therefore, the remarkable result, that the i of the ending induction of each unit of length of the current is not materially altered, at least within certai by changing the length of the whole conductor. Fr we would infer that the shock depends more on the i of the action than on the quantity of the current,

know that the latter is diminished in a given unit of the conductor by increasing the length of the whole.

14. We have seen (8.) that, with a circuit composed of ten elements of the compound battery and the coil No. 2, the shock, at the beginning of the current, was fully equal to that at the ending. It was, however, found, that if, in this case, the length of the coil was increased, this shock was diminished; and we may state, as an inference from several experiments, that however great may be the intensity of the electricity from the battery, the shock at the beginning may be so reduced, by a sufficient increase of the length of the primary circuit, as to be scarcely perceptible.

15. It was also found, that when the thickness of the coil was increased, the length and intensity of the circuit remaining the same, the shock at the beginning of the battery current was somewhat increased. This result was produced by using a double coil; the electricity was made to pass through one strand, and immediately afterwards through both: the shock from the helix in the latter case was apparently the greater.

16. By the foregoing results we are evidently furnished with two methods of increasing, at pleasure, the intensity of the induction at the beginning of a battery current; the one consisting in increasing the intensity of the source of the electricity, and the other in diminishing the resistance to conduction of the circuit while the intensity remains the same.

17. The explanation of the effects which we have given, relative to the induction at the beginning, is apparently not difficult. The resistance to conduction in the case of a long conductor and a battery of a single element is so great, that the full development of the primary current may be supposed not to take place with sufficient rapidity to produce the instantaneous action on which the shock from the secondary current would seem to depend. But when a battery of a number of elements is employed, the poles of this, previous to the moment of completing the circuit, are in a state of electrical tension; and therefore the discharge through the conductor may be supposed to be more sudden, and hence an induction of more intensity is produced.

18. That the shock at both making and breaking the circuit in some way depends on the rapidity of formation and diminution of the current, is shown by the following experiment, in which the tension just mentioned does not take place, and in which, also, the current appears to diminish more slowly. The two ends of the coil were placed in the two cups which

formed the poles of the battery, and permanently retaine
during the experiment; also, at the distance of about six
from, say the right hand end of the coil, a loop was made
riband, which could be plunged into the cup containi
left hand end. With this arrangement, and while o
two extreme ends of the coil were in connexion with t
of mercury, of course the current passed through the
length of the riband of the coil; but by plunging the lo
the left hand cup, the whole length of the coil, except
inches before mentioned, was excluded from the batt
cuit. And again, when the loop was lifted out of the c
whole length was included. In this way the current
coil could be suddenly formed and interrupted, w
poles of the battery were continually joined by a con
but no shock with either a single or a compound
could be obtained by this method of operation.

19. The feebleness of the shock at the beginning
current, with a single battery and a long coil, is not
owing to the cause we have stated (17.), namely, the re
to conduction offered by the long conductor, but a
pends, in a considerable degree, if not principally, on
verse influence of the secondary current, induced in
mary conductor itself, as is shown by the result of the
ing experiment. Helix No. 1. was placed on a coil co
of only three spires or turns of copper riband; with
shock, both at making and breaking the circuit with
battery, could be felt in the hands. A compound
then formed of the copper ribands of coils No. 3. and
together so that the several spires of the two alterna
each other, and when this was introduced into the c
as not to act on the helix by its induction, and the
current passed through, for example, coil No. 3, the
making contact with the pole of the battery was so n
duced as to be imperceptible in the hands, while the
breaking the contact was about the same as before t
tion was made to the length of the circuit. The end
No. 4. were now joined so as to produce a closed cir
induced current in which would neutralize the second
rent in the battery conductor itself; and now the
making the contact was nearly as powerful as in
where the short conductor alone formed the circuit
battery. Hence, the principal cause of the feeblenes
effect at the beginning of the battery current, is the
action on the helix of the secondary current produc
conductor of the battery circuit itself. The shoc

breaking of the circuit, in this experiment, did not appear affected by joining or separating the ends of coil No. 4.

20. Having investigated the conditions on which the inductive action at the beginning of a battery current depends, experiments were next instituted to determine the nature of the effects produced by this induction: and first the coils were arranged in the manner described in my last paper (III. 79.), for producing currents of the different orders. The result with this arrangement was similar to that which I have described in reference to the ending induction, namely, currents of the third, fourth, and fifth orders were readily obtained.

21. Also, when an arrangement of apparatus was made similar to that described in paragraph 87 of my last paper, it was found that a current of intensity could be induced from one of quantity and the converse.

22. Likewise, the same screening or rather neutralizing effect was produced, when a plate of metal was interposed between two consecutive conductors of the series of currents, as was described (III. Section IV.) in reference to the ending induction. In short, the series of induced currents produced at the beginning of the primary current appeared to possess all the properties belonging to those of the induction at the ending of the same current.

23. I may mention, in this place, that I have found, in the course of these experiments, that the neutralizing power of a plate of metal depends, in some measure, on its superficial extent. Thus a broad plate which extends, in every direction, beyond the helix and coil, produces a more perfect screening than one of the same metal and of the same thickness, but of a diameter only a little greater than that of the coil.

24. The next step in the investigation was to determine the direction of the currents of the different orders produced by the beginning induction, and for this purpose the magnetizing spirals (5.) were used, and the results obtained by these verified by the indications of the galvanometer. It should be stated here, as a fact which was afterwards found of some importance, that although the needle of the galvanometer was powerfully deflected when the instrument was placed in the circuit of the secondary current, yet a very feeble effect was produced on it by the action of a current of the third, fourth, or fifth order. The directions, however, of these current, as indicated by the feeble motion of the needle, were the same as those given by the magnetizing spiral.

25. The direction of the different currents produced at the making of the battery current, as determined by these instruments, is as follows: namely, the direction of the secondary

current is, as stated by Dr. Faraday, adverse to that o primary current, and, also, the direction of each succee current is opposite to that of the one which produced it. have, therefore, from these results, and those formerl tained (III. 92.), the following series of directions of curr one produced at the moment of beginning, and the oth that of ending of the battery current.

	At the beginning,		At the end
Primary current	+	......	+
Secondary current . . .	—	......	+
Current of the third order .	+	......	—
Current of the fourth order	—	......	+
Current of the fifth order .	+	......	—

26. These two series, at first sight, may appear very ferent, but, with a little attention, they will be seen to the same nature. If we allow that the induction at the ing of a galvanic battery should be opposite to that at beginning of the same, then the sign at the top of the se column may be called minus instead of plus, and we have the second series — + — + alternating precisely the first.

27. In connexion with the results given in the last paragraphs, it is due to Mr. Sturgeon that I should state in a letter addressed to me, and published in the Anna Electricity, he has predicted, from his theory, that I w find, on examination, the series of alternation of current the beginning induction which I have here given. I however, add, that it appears to me that this result m have been predicted without reference to any theory. T was no reason to suppose the induction at the begin would be different in its nature from that at the ending, therefore the series which would be produced from the for might be immediately inferred from that belonging to latter, by recollecting that the direction of the inductio the beginning should be opposite to that at the ending. not wish it to be supposed, however, from this remark, I had, myself, drawn any inference from my experiment to the alternations of currents which might be produced by beginning induction; the truth is, that this action was so ble with the arrangement of apparatus I employed, th supposed it could not produce a series of currents of the ferent orders.

---

28. In the course of the experiments given in this sect I have found that a shock can be produced without usi coil, by arranging about ten elements of the battery in

form of a circle, and placing the helix within this. The shock was felt in the hands at the moment of closing the circuit, but the effect at opening the same was scarcely perceptible through the tongue. An attempt was also made to get indications of induction by placing the helix within a circle of dilute acid, connected with a battery instead of a coil, but the effect, if any, was very feeble.

29. I have shown, in the second number of my contributions, that if the body be introduced into a circuit with a battery of one hundred and twenty elements, without a coil, a thrilling sensation will be felt during the continuance of the current, and a shock will be experienced at the moment of interrupting the current by breaking the circuit at any point. This result is evidently due to the induction of a secondary current in the battery itself, and on this principle the remarkable physiological effects produced by Dr. Ure, on the body of a malefactor, may be explained. The body, in these experiments, was made to form a part of the circuit, with a compound galvanic apparatus in which a series of interruptions was rapidly made by drawing the end of a conductor over the edges of the plates of the battery. By this operation a series of induced currents must have been produced in the battery itself, the intensity of which was greater than that of the primary current.

30. In this connexion I may mention that the idea has occurred to me, that the intense shocks given by the electrical fish may possibly be from a secondary current, and that the great amount of nervous organization found in these animals may serve the purpose of a long conductor*. It appears to me, that in the present state of knowledge, this is the only way in which we can conceive of such intense electricity being produced in organs imperfectly insulated and immersed in a conducting medium. But we have seen that an original current of feeble intensity can induce, in a long wire, a secondary current capable of giving intense shocks, although the several strands of the wire are separated from each other only by a covering of cotton thread. Whatever may be the worth of this suggestion, the secondary current affords the means of imitating the phænomena of the shock from the electrical eel, as described by Dr. Faraday. By immersing the apparatus (Fig. 3.) in a shallow vessel of water, the handles being placed at the two extremities of the diameter of the helix, and the hands plunged into the water parallel to a line joining the two poles, a shock is felt through the arms; but

---

* Since writing the above, I have found that M. Masson has suggested the same idea in an interesting thesis lately published.

when the contact with the water is made in a line at angles to the last, only a slight sensation is felt in each but no shock.

31. Since the publication of my last paper, I have exh to my class the experiment (No. III. Sect. III.) relative induction at a distance on a much larger scale. All my were united so as to form a single length of conduct about four hundred feet, and this was rolled into a ri five and a half feet in diameter, and suspended vert against the inside of the large folding doors which sep the laboratory from the lecture-room. On the other s the doors, in the lecture-room, and directly opposite th was placed a helix, formed of upwards of a mile of c wire, one-sixteenth of an inch in thickness, and woun a hoop of four feet in diameter. With this arrangement a battery of one hundred and forty-seven square feet o surface divided into eight elements, shocks were perce in the tongue, when the two conductors were separat the distance of nearly seven feet; at the distance of be three and four feet, the shocks were quite severe. Th hibition was rendered more interesting by causing the i tion to take place through a number of persons standin row between the two conductors.

Sect. II.—*On apparently two kinds of Electro-dynam Induction.*

32. The investigations arranged under this head had origin in the following circumstances. After the publi of my last paper, I received, through the kindness o Faraday, a copy of the fourteenth series of his resea and in this I was surprised to find a statement which app in direct opposition to one of the principal facts of my munication. In paragraph 59, I state, in substance, when a plate of metal is interposed between the coil t mitting a galvanic current, and the helix placed above receive the induction, the shock from the secondary cu is almost perfectly neutralized. Dr. Faraday, in the e sion of his new and ingenious views of the agency of th termediate particles in transmitting induction, was led to an experiment on the same point; and apparently, unde same circumstances, he found that it "makes not the difference, whether the intervening space between the conductors is occupied by such insulating bodies as air phur, and shell-lac, or such conducting bodies as coppe other non-magnetic metals."

33. As the investigation of the fact mentioned above f

an important part of my paper, and is intimately connected with almost all the phænomena subsequently described in the communication, I was, of course, anxious to discover the cause of so remarkable a discrepancy. There could be no doubt of the truth of my results, since a shock from a secondary current which would paralyze the arms was so much reduced by the interposition of plates of metal, as scarcely to be felt through the tongue.

34. After some reflection, however, the thought occurred to me that induction might be produced in such a way as not to be affected by the interposition of a plate of metal. To understand this, suppose the end of a magnetic bar placed perpendicularly under the middle of a plate of copper, and a helix suddenly brought down on this; an induced current would be produced in the helix by its motion towards the plate, since the copper, in this case, could not screen the magnetic influence. Now, if we substitute for the magnet a coil through which a galvanic current is passing, the effect should be the same. The experiment was tried by attaching the ends of the helix to a galvanometer*, and the result was as I expected: when the coil was suddenly brought down on the plate the needle swung in one direction, and when lifted up, in the other; the amount of deflection being the same, whether the plate was interposed or not.

35. It must be observed in this experiment, that the plate was at rest, and consequently did not partake of the induction produced by the motion of the helix. From my previous investigations, I was led to conclude that a different result would follow, were a current also generated in the plate by simultaneously moving it up and down with the helix. This conclusion, however, was not correct, for on making the experiment, I found that the needle was just as much affected when the plate was put in motion with the helix as when the latter alone was moved.

36. This result was so unexpected and remarkable, that it was considered necessary to repeat and vary the experiment in several ways. First, a coil was interposed instead of the plate, but whether the coil was at rest or in motion with the helix, with its ends separated or joined, the effect on the galvanometer was still the same; not the least screening influence could be observed. In reference to the use of the coil in this experiment, it will be recollected that I have

---

* The arrangement will be readily understood by supposing in Fig. 3. the handles removed, and the ends of the helix joined to the ends of the wire of a galvanometer; also, by a plate of metal interposed between the helix and the coil.

found this article to produce a more perfect neutrali
than a plate.

37. Next, the apparatus remaining the same, and the
at rest during the experiment, currents were induced
by moving the battery attached to the coil up and do
the acid. But in this case, as in the others, the effect
galvanometer was the same, whether the plate or the co
interposed or not.

38. The experiment was also tried with magneto-elect
For this purpose, about forty feet of copper wire, co
with silk, were wound around a short cylinder of stiff
and into this was inserted a hollow cylinder of sheet c
and into this again, a short rod of soft iron; when the
was rendered magnetic, by suddenly bringing in contac
its two ends the different poles of two magnets, a curr
course, was generated in the wire, and this, as befor
found to affect the galvanometer to the same degree
the copper cylinder was interposed, as when nothing b
paper intervened.

39. The last experiment was also varied by wrappin
copper wires of equal length around the middle of the k
of a horse-shoe magnet, leaving the ends of the inn
projecting, and those of the outer attached to a galvano
A current was generated in each by moving the kee
the ends of the magnet, but the effect on the galvano
was not in the least diminished by joining the ends of t
ner wire.

40. At first sight, it might appear that all these resu
at variance with those detailed in my last paper, rela
the effect of interposed coils and plates of metal. But
be observed, that in all the experiments just given, t
duced currents are not the same as those described in m
communication. They are all produced by motion, an
an appreciable duration, which continues as long as th
tion exists. They are also of low intensity, and thu
have not been able to get shocks by any arrangement
paratus from currents of this kind. On the other han
currents produced at the moment of *suddenly* maki
breaking a galvanic current are of considerable intensit
exist but for an instant. From these, and other fact
sently to be mentioned, I was led to suppose that the
two kinds of electro-dynamic induction; one of which
neutralized by the interposition of a metallic plate be
the conductors and the other not.

41. In reference to this surmise, it became import
examine again all the phænomena of induction at su

making and breaking a galvanic current*. And in connexion with this part of the subject, I will first mention a fact which was observed in the course of the experiments given in the last section, on the direction of the induced currents of different orders. It was found, that though the indications of the galvanometer were the same as those of the spiral, in reference to the direction of the induced currents, yet they were very different in regard to the intensity of the action. Thus, when the arrangement of the apparatus was such that the induction at making the battery circuit was so feeble as not to give the least magnetism to the needle, and so powerful at the ending as to magnetize it to saturation, the indication of the galvanometer was the same in both cases.

42. Also, similar results were obtained in comparing the shock and the deflection of the galvanometer. In one experiment, for example, the shock was so feeble at making contact that it could scarcely be perceived in the fingers, but so powerful at the breaking of the circuit as to be felt in the breast; yet the galvanometer was deflected about thirty-five degrees to the right, at the beginning of the current, and only an equal number of degrees to the left, at the ending of the same.

43. In another experiment, the apparatus being the same as before, the magnetizing spiral and the galvanometer were both at once introduced into the circuit of the helix. A sewing needle being placed in the spiral, and the contact with the battery made, the needle showed no signs of magnetism, although the galvanometer was deflected thirty degrees. The needle being replaced, and the battery circuit broken, it was now found strongly magnetized, while the galvanometer was only moved about as much as before in the opposite direction.

44. Also, effects similar to those described in the last two paragraphs were produced when the apparatus was so arranged as to cause the induction at the beginning of the battery current to predominate. In this case the galvanometer was still nearly equally affected at making and breaking battery contact, or any difference which was observed could be referred to a variation in the power of the battery during the experiment.

45. Another fact of importance belonging to the same class has been mentioned before (24.), namely, that the actions of the currents of the third, fourth, and fifth orders produce a very small effect on the galvanometer, compared with that of the secondary current; and this is not alone on account of the diminishing power of the successive inductions, as will be evident from the following experiment. By raising the helix from

---

* See my last paper in Lond. and Edinb. Phil. Mag., March 1840, p. 200.

the coil, in the arrangement of apparatus for the secondary current, the shock was so diminished as to be inferior to one produced by the arrangement for a tertiary current; yet, while with the secondary current the needle was deflected twenty-five degrees, with the tertiary it scarcely moved more than one degree; and with the currents of the fourth and fifth orders the deflections were still less, resembling the effect of a slight impulse given to the end of the needle.

46. With the light obtained from the foregoing experiments, I was the more fully persuaded that some new and interesting results might be obtained by a re-examination of my former experiments, on the phænomena of the interposed plate of metal, in the case where the induction was produced by making and breaking the circuit with a cup of mercury; and in this I was not disappointed. The coil (Fig. 3.) being connected with a battery of ten elements, the shocks, both at making and breaking the circuit, were very severe; and these, as usual, were almost entirely neutralized by the interposition of a zinc plate. But when the galvanometer was introduced into the circuit instead of the body, its indications were the same whether the plate was interposed or not; or, in other words, the galvanometer indicated no screening, while, under the same circumstances, the shocks were neutralized.

47. A similar effect was observed when the galvanometer and the magnetizing spiral were together introduced into the circuit. The interposition of the plate entirely neutralized the magnetizing power of the spiral, in reference to tempered steel, while the deflections of the galvanometer were unaffected.

48. In order to increase the number of facts belonging to this class, the last experiments were varied in several ways; and first, instead of the hard steel needle, one of soft iron wire was placed in the spiral, with a small quantity of iron filings almost in contact with one of its ends. The plate being interposed, the small particles of iron were attracted by the end of the needle, indicating a feeble, temporary development of magnetism. Hence the current which moves the needle, and is not neutralized by the interposed plate, also feebly magnetizes soft iron, but not hard steel.

49. Again, the arrangement of apparatus being as in paragraph 46, instead of a plate of zinc, one of cast iron, of about the same superficial dimensions, but nearly half an inch thick, was interposed; with this the magnetizing power of the spiral, in reference to tempered steel, was neutralized; and, also, the action of the galvanometer was much diminished.

50. Another result was obtained by placing in the circuit

of the helix (fig. 3.), at the same time, the galvanometer, the spiral, and a drop of distilled water; with these the magnetizing power of the spiral was the same as without the water, but the deflection of the galvanometer was reduced from ten to about four degrees. In addition to these, the body was also introduced into the same circuit; the shocks were found very severe, the spiral magnetized needles strongly, but the galvanometer was still less moved than before. The current of low intensity, which deflects the needle of the galvanometer in these instances, was partially intercepted by the imperfect conduction of the water and the body.

51. To exhibit the results of these experiments with still more precision, an arrangement of apparatus was adopted similar to that used by Dr. Faraday, and described in the fourteenth series of his researches, namely, a double galvanometer was formed of two separate wires of equal length and thickness, wound together on the same frame; and, also, a double magnetizing spiral was prepared by winding two equal wires around the same piece of hollow straw. Coil No. 1, connected with the battery, was supported perpendicularly on a table, and coils Nos. 3 and 4 were placed parallel to this, one on each side, to receive the induction, the ends of these being so joined with those of the galvanometer and the spiral that the induced current from the one coil would pass through the two instruments, in an opposite direction to that of the current from the other coil. The two outside coils were then so adjusted, by moving them to and from the middle coil, that the induced currents perfectly neutralized each other in the two instruments, and the needle of the galvanometer and that in the spiral were both unaffected when the circuit of the battery was made and broken. With this delicate arrangement the slightest difference in the action of the two currents would be rendered perceptible; but when a zinc plate was introduced so as to screen one of the coils, the needle of the galvanometer still remained perfectly stationary, indicating not the least action of the plate, while the needle in the spiral became powerfully magnetic. When, however, a plate of iron was interposed instead of the one of zinc, the needle of the galvanometer was also affected.

52. From the foregoing results it would seem that the secondary current, produced at the moment of suddenly beginning or ending of a galvanic current, by making and breaking contact with a cup of mercury, consists of two parts, which possess different properties. One of these is of low intensity, can be interrupted by a drop of water, does not magnetize hardened steel needles, and is not screened by the interposition

of a plate of any metal, except iron, between the condu
The other part is of considerable intensity, is not inter
by a drop of water, developes the magnetism of hardened
gives shocks, and is screened or neutralized by a close
or a plate of any kind of metal. Also, the induced c
produced by moving a conductor towards or from a b
current, and that produced by the movement up and
of a battery in the acid, are of the nature of the first
tioned part, while the currents of the third, fourth, an
orders partake almost exclusively of the properties of t
cond part.

---

53. The principal facts and conclusions of this sectio
announced to the Society in October 1839, and agai
sented in the form in which they are here detailed in
last. Since then, however, I have had leisure to exami
subject more attentively, and after a careful comparis
these results with those before given, I have obtaine
more definite views of the phænomena which are given
next section.

SECT. III.—*Theoretical Considerations relating to the P mena described in this and the preceding Communica Read November* 20, 1840.

54. The experiments given in the last No. of my con
tions were merely arranged under different heads, an
such inferences drawn from them as could be immediate
duced without reference to a general explanation. Th
dition, however, which I have since made to the num
facts, affords the means of a wider generalization ; an
an attentive consideration of all the results given in th
the preceding papers, I have come to the conclusion tha
can all be referred to the simple laws of the induction
beginning and the ending of a galvanic current.

55. In the course of these investigations the limited
theses which I have adopted have been continually m
by the development of new facts, and therefore my p
views, with the further extension of the subject, may a
quire important corrections. But I am induced to b
from its exact accordance with all the facts, so far a
have been compared, that if the explanation I now v
to give be not absolutely true, it is so, at least, in appro
tion, and will therefore be of some importance in the
suggesting new forms of experiment, or as a first step to
a more perfect generalization.

56. To render the laws of induction at the beginnin
the ending of a galvanic current more readily applica

the explanation of the phænomena, they may be stated as follows:—1. During the time a galvanic current is increasing in quantity in a conductor, it induces, or tends to induce, a current in an adjoining parallel conductor in an opposite direction to itself. 2. During the continuance of the primary current in full quantity, no inductive action is exerted. 3. But when the same current begins to decline in quantity, and during the whole time of its diminishing, an induced current is produced in an opposite direction to the induced current at the beginning of the primary current.

57. In addition to these laws, I must frequently refer to the fact, that *when the same quantity of electricity in a current of short duration is passed through a galvanometer, the deflecting force on the needle is the same, whatever be the intensity of the electricity.* By intensity is here understood the ratio of a given quantity of force to the time in which it is expended*; and according to this view, the proposition stated is an evident inference from dynamic principles. But it does not rest alone on considerations of this kind, since it has been proved experimentally by Dr. Faraday, in the third series of his researches.

58. In order to form a definite conception of the several conditions of the complex phænomena which we are about to investigate, I have adopted the method often employed in physical inquiries, of representing the varying elements of action by the different parts of a curve. This artifice has been of much assistance to me in studying the subject, and without the use of it at present, I could scarcely hope to present my views in an intelligible manner to the Society.

59. After making these preliminary statements, we will now proceed to consider the several phænomena; and, first, let us take the case in which the induction is most obviously produced in accordance with the laws as above stated (56.), namely, by immersing a battery into the acid, and also by withdrawing it from the same. During the time of the descent of the battery into the liquid, the conductor connected with it is constantly receiving additional quantities of current electricity, and each of these additions produces an inductive action on the adjoining secondary conductor. The amount, therefore, of induced current produced during any moment of time will be just in proportion to the corresponding increase in the current of the battery during the same moment. Also, the amount of induction during any moment while the cur-

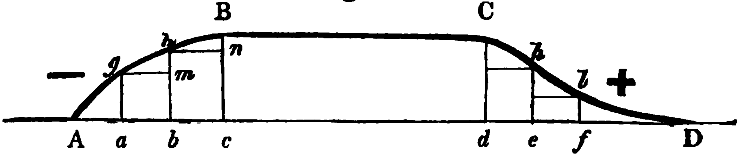
---

* Or more correctly speaking, the ratio of two quantities of the same species representing the force and time.

rent of the battery is diminishing in quantity will be in proportion to the decrease during the same moment.

60. The several conditions of this experiment may be represented by the different parts of the curve, A, B, C, D, fig. 17, in which the distances, A *a*, A *b*, A *c*, represent the times during which the battery is descending to different depths into the acid; and the corresponding ordinates, *a g*, *b h*, *c* B, represent the amount of current electricity in the battery conductor corresponding to these times. The differences of the ordinates, namely, *a g*, *m h*, *n* B, express the increase in the quantity of the battery current during the corresponding moments of time represented by A *a*, *a b*, *b c*; and since the inductive actions (59.) are just in proportion to these increases, the same differences will also represent the amount of induced action exerted on the secondary conductor during the same moment of time.

Fig. 17.



61. When the battery is fully immersed in the acid, or when the current in the conductor has reached its state of maximum quantity, and during the time of its remaining constant, no induction is exerted; and this condition is expressed by the constant ordinates of the part of the curve B C, parallel to the axis. Also, the inductive action produced by each diminution of the battery current, while the apparatus is in the progress of being drawn from the acid, will be represented by the differences of the ordinates at the other end, C D, of the curve.

62. The sum of the several increments of the battery current, up to its full development, will be expressed by the ordinate *c* B, and this will, therefore, also represent the whole amount of inductive action exerted in one direction at the beginning of the primary current; and, for the same reason, the equal ordinate, C *d*, will represent the whole induction in the other direction at the ending of the same current. Also, the whole time of continuance of the inductive action at the beginning and ending will be represented by A *c* and *d* D.

63. If we suppose the battery to be plunged into the acid to the same depth, but more rapidly than before, then the time represented by A *c* will be diminished, while the whole

amount of inductive force expended remains the same; hence, since the same quantity of force is exerted in a less time, a greater intensity of action will be produced (57.), and consequently a current of more intensity, but of less duration, will be generated in the secondary conductor. The intensity of the induced currents will, therefore, evidently be expressed by the ratio of the ordinate *c* B to the abscissa A *c*. Or, in more general and definite terms, the intensity of the inductive action at any moment of time will be represented by the ratio of the rate of increase of the ordinate to that of the abscissa for that moment*.

64. It is evident from the last paragraph, that the greater or less intensity of the inductive action will be immediately presented to the eye, by the greater or less obliquity of the several parts of the curve to the axis. Thus, if the battery be suddenly plunged into the acid for a short distance, and then gradually immersed through the remainder of the depth, the varying action will be exhibited at once by the form of A B, the first part of the curve, fig. 17. The steepness of the part A *g* will indicate an intense action for a short time A *a*, while the part *g* B denotes a more feeble induction during the time represented by *a c*. In the same way, by drawing up the battery suddenly at first, and afterwards slowly, we may produce an inductive action such as would be represented by the parts between C and D of the ending of the curve.

65. Having thus obtained representations of the different elements of action, we are now prepared to apply these to the phænomena. And, first, however varied may be the intensity of the induction expressed by the different parts of the two ends of the curve, we may immediately infer that a galvanometer, placed in the circuit of the secondary conductor, will be equally affected at the beginning and ending of the primary current; for, since the deflection of this instrument is due to the whole amount of a current, whatever may be its intensity (57.), and since the ordinates *c* B and C *d* are equal, which represent the quantity of induction in the two directions, and, consequently, the amount of the secondary current, therefore the deflection at the beginning and ending of the battery current will, in all cases, be equal. This inference is in strict accordance with the results of experiment;

---

* According to the differential notation, the intensity will be expressed by $\frac{dy}{dx}$. In some cases the effect may be proportional to the intensity multiplied by the quantity, and this will be expressed by $\frac{dy^2}{dx}$, x and y representing, as usual, the variable abscissa and ordinate.

for, however rapidly or slowly we may plunge the b into the acid, and however irregular may be the rate at it is drawn out, still if the whole effect be produced wit time of one swing of the needle, the galvanometer is de to an equal degree.

66. Again, the intensity of one part of the inductive for example that represented by A g, may be suppose so great as to produce a secondary current capable of trating the body, and of thus producing a shock* wh other parts of the action, represented by g B and C so feeble as to affect the galvanometer only. We woul have a result the same as one of those given in the l tion (42.), and which was supposed to be produced kinds of induction; for if the shock were referred to test of the existence of an induced current, one wo found at the beginning only of the battery current, w the galvanometer were consulted, we would perceive fects of a current as powerful at the ending as at the ning.

67. The results mentioned in the last paragraph can obtained by plunging a battery into the acid; the for of the current in this way is not sufficiently rapid to p a shock. The example was given to illustrate the ma which the same effect is supposed to be produced, in t of the more sudden formation of a current, by plungi end of the conductor into a cup of mercury permane tached to a battery already in the acid, and in full ope The current, in this case, rapid as may be its develo cannot be supposed to assume *per saltum* its maximu of quantity; on the contrary, from the general law c tinuity, we would infer that it passes through all the i diate states of quantity, from that of no current, if pression may be allowed, to one of full development are, however, considerations of an experimental nature would lead us to the same conclusion (18, 90.), and the further inference that the *decline* of the current is stantaneous. According to this view, therefore, the in actions at the beginning and the ending of a primary c of which the formation and interruption is effected by of the contact with a cup of mercury, may also be repr by the several parts of the curve, fig. 17.

68. We have now to consider how the rate of incr diminution of the current, in the case in question, can tered by a change in the different parts of the app

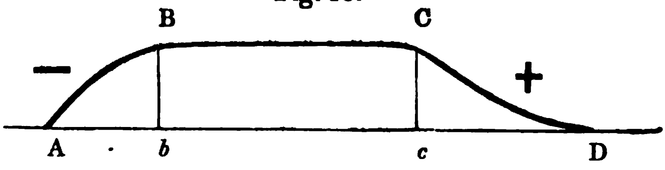
---

* The shock depends more on the intensity than on the quanti paragraph 13.

and, first, let us take the example of a single battery and a short conductor, making only one or two turns around the helix; with this arrangement a feeble shock, as we have seen (11.), will be felt at the making, and also at the breaking of the circuit. In this case it would seem that almost the only impediment to the most rapid development of the current would be the resistance to conduction of the metal; and this we might suppose would be more rapidly overcome by increasing the tension of the electricity; and, accordingly, we find, that if the number of the elements of the battery be increased, the shock at making the circuit will also be increased, while that at breaking the circuit will remain nearly the same. To explain, however, this effect more minutely, we must call to mind the fact before referred to (17.), that when the poles of a compound battery are not connected, the apparatus acquires an accumulation of electricity, which is discharged at the first moment of contact, and which in this case would more rapidly develope the full current, and hence produce the more intense action on the helix at making the circuit.

69. The shock, and also the deflection of the needle, at breaking the circuit with a compound battery and a short coil (9.), appears nearly the same as with a battery of a single element, because the accumulation just mentioned, in the compound battery, is discharged almost instantly, and, according to the theory (71.) of the galvanic current, leaves the constant current in the conductor nearly in the same state of quantity as that which would be produced by a battery of a single element; and hence the conditions of the ending of the current are the same in both cases. Indeed, in reference to the ending induction, it may be assumed as a fact which is in accordance with all the experiments (9, 13, 73, 74, 75, 76, &c.), as well as with theoretical considerations*, *that when the circuit is broken by a cup of mercury, the rate of the diminution of the current, within certain limits, remains the same, however the intensity of the electricity or the length of the conductor may be varied.*

Fig. 18.



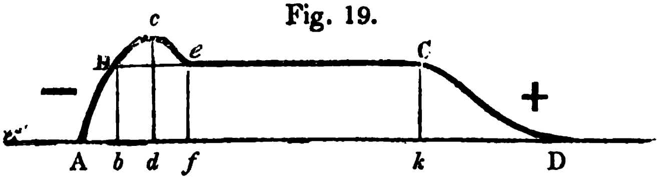
70. The several conditions of the foregoing examples are exhibited by the parts of the curves, figs. 18 and 19. The

---

* See the theory of Ohm.—Taylor's Scientific Memoirs, vol. i. p. 312, 511, and vol. ii. p. 401.

gradual development of the current in the short conductor, with a single battery, and the gradual decline of the same, are represented by the gentle rise of A B and fall of C D, fig. 18; while, in the next fig. (19.), the sudden rise of A B indicates the intensity which produces the increased shock, after the number of elements of the battery has been increased. The accumulation of the electricity, which almost instantly subsides, is represented by the part B*c e*, fig. 19; and from this we see, at once, that although the shock is increased by using the compound battery, yet the needle of the galvanometer will be deflected only to the same number of degrees, since the parts B*c* and *c e* give inductive actions in contrary directions, and both within the time of a single swing of the needle, and, consequently, will neutralize each other. The resulting deflecting force will, therefore, be represented by *e f*, which is equal to C*k*, or to *b* B, in fig. 18.

Fig. 19.



The intensity of the shock at the breaking is represented as being the same in the two figures, by the similarity of the rate of descent of the part C D of the curve in each.

71. We have said (69.) that the quantity of current electricity in a short conductor and a compound battery, after the first discharge, is nearly the same as with a single battery. The exact quantity, according to the theory of Ohm, in a unit of length of the conductor, is given by the formula

$$\frac{n\,A}{r\,n + R}.$$

In this n represents the number of elements; A, the electromotive force of one element; r, the resistance to conduction of one element; and R, the length of the conductor, or rather its resistance to conduction in terms of r. Now, when R is very small, in reference to $r\,n$, as is the case with a very short metallic conductor, it may be neglected, and then the expression becomes

$$\frac{n\,A}{r\,n} \text{ or } \frac{A}{r};$$

and since this expresses the quantity of current electricity in a unit of the length of the circuit, with either a single or a compound battery, therefore, with a short conductor, the quantity of current electricity in the two cases is nearly the same.

72. Let us next return to the experiment with a battery of a single element (68.), and instead of increasing the intensity of the apparatus, as in the last example, let the length of the conductor be increased; then the intensity of the shock at the beginning of the current, as we have seen (14.), will be diminished, while that of the one at the ending will be increased. That the shock should be lessened at the beginning, by increasing the length of the conductor, is not surprising, since, as we might suppose, the increased resistance to conduction would diminish therapidity of the development of the current. But the secondary current, which is produced in the conductor of the primary current itself, as we have seen (19.), is the principal cause which lessens the intensity of the shock, and the effect of this, as will be shown hereafter, may also be inferred from the principles we have adopted.

73. The explanation of the increased shock at the moment of breaking the circuit with the long conductor, rests on the assumption before mentioned (69.), that the velocity of the diminution of a current is nearly the same in the case of a long conductor as in that of a short one. But, to understand the application of this principle more minutely, we must refer to the change which takes place in the quantity of the current in the conductor by varying its length; and this will be given by another application of the formula before stated (71.). This, in the case of a single battery, in which n equals unity, becomes

$$\frac{A}{r+R};$$

and since this, as will be recollected, represents the quantity of current electricity in a unit of length; of the conductor, we readily infer from it that, by increasing the length of the conductor, or the value of R, the quantity of current in a unit of the length is lessened. And if the resistance of a unit of the length of the conductor were very great in comparison with that of r (the resistance of one element of the battery), then the formula would become

$$\frac{A}{R},$$

or the quantity in a single unit of the conductor would be inversely as its entire length, and hence the amount of current electricity in the whole conductor would be a constant quantity, whatever might be its length. This, however, can never be the case in any of our experiments, since in no instance is the resistance of R very great in reference to r, and therefore, according to the formula (73.), the whole quantity of current

electricity in a long conductor is always somewhat greate
in a short one.

74. Let us, however, in order to simplify the conditi
the induction at the ending of a current, suppose th
quantity in a unit of the conductor is inversely as its
length, or, in other words, that the quantity of curren
tricity is the same in a long conductor as in a short one
let us also suppose, for an example, that the length of the
conductor, fig. 3, was increased from one spire to t
spires; then, if the velocity of the diminution of the sec
the current is the same (69.) in the long conductor as
short one, the shock which would be received by subn
the helix to the action of one spire of the long coil wo
nearly of the same intensity as that from one spire of the
conductor; the quantity of induction, however, as sho
the galvanometer, should be nearly twenty times less
these inferences I have found in accordance with tl
sults of experiments (75.). If, however, instead of placi
helix on one spire of the long conductor, it be submit
once to the influence of all the twenty spires, then the
sity of the shock should be twenty times greater, since t
times the quantity of current electricity collapses, if w
be allowed the expression, in the same time, and exerts a
all its influence on the helix. If, in addition to this, w
the consideration, that the whole quantity of current elec
in a long conductor is greater than that in a short one
we shall have a further reason for the increase of the te
shock, when we increase the length of the battery cond

75. The inference given in the last paragraph, rela
the change in the quantity of the induction, but not in t
tensity of the shock from a single spire, by increasin
whole length of the conductor, is shown to be true by r
ing the experiment described in paragraph 13. In this,
have seen, the intensity of the shock remained the sam
though the length of the circuit was increased by the ad
of coil No. 2. When, however, the galvanometer wa
ployed in the same arrangement, the whole quantity
duction, as indicated by the deflection of the needle, w
minished almost in proportion to the increased length
circuit. I was led to make this addition to the exper
(13.) by my present views.

76. The explanation given in paragraph 74 also in
that of the peculiar action of a long conductor, either
or extended, in giving shocks and sparks from a batter
single element, discovered by myself in 1831 (see Co
No. II.). The induction, in this case, takes place in the

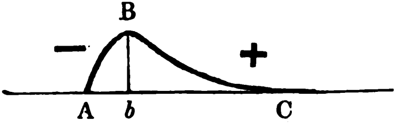
ductor of the primary current itself, and the secondary current which is produced is generated by the joint action of each unit of the length of the primary current. Let us suppose, for illustration, that the conductor was at first one foot long, and afterwards increased to twenty feet. In the first case, because the short conductor would transmit a greater quantity of electricity, the secondary current produced by it would be one of considerable quantity or power to deflect a galvanometer; but it would be of feeble intensity, for although the primary current would collapse with its usual velocity (69.), yet, acting on only a foot of conducting matter, the effect (74.) would be feeble. In the second case, each foot of the twenty feet of the primary current would severally produce an inductive action of the same intensity as that of the short conductor, the velocity of collapsion being the same; and as they are all at once exerted on the same conductor, a secondary current would result, of twenty times the intensity of the current in the former case.

77. To render this explanation more explicit, it may be proper to mention, that a current produced by an induction on one part of a long conductor of uniform diameter, must exist, of the same intensity, in every other part of the conductor; hence the action of the several units of length of the primary current must enforce each other, and produce the same effect on its own conductor that the same current would if it were in a coil, and acting on a helix. I need scarcely add, that in this case, as in that given in paragraph 74, the whole amount of induction is greater with the long conductor than with the short one, because the quantity of current electricity is greater in the former than in the latter.

78. We may next consider the character of the secondary current, in reference to its action in producing a tertiary current in a third conductor. The secondary current consists, as we may suppose, in the disturbance, for an instant, of the natural electricity of the metal, which, subsiding, leaves the conductor again in its natural state; and whether it is produced by the beginning or ending of a primary current, its nature, as we have seen (22.), is the same. Although the time of continuance of the secondary current is very short, still we must suppose it to have some duration, and that it increases, by degrees, to a state of maximum development, and then diminishes to the normal condition of the metal of the conductor; the velocity of its development, like that of the primary current, will depend on the intensity of the action by which it is generated, and also, perhaps, in some degree, on the resistance of the conductor; while, agreeably to the hypothesis we have assumed (69.), the velocity of its diminution is nearly a

constant quantity, and is not affected by changes in these c ditions; hence, if we suppose the induction which prod the secondary current to be sufficiently intense, the velocit its development will exceed that of its diminution, as in example of the primary current from the intense sourc the compound battery of many elements. Now this is case with the inductions which produce currents of the ferent orders, capable of giving shocks or of magnetizing s needles: the secondary currents from these are always of c siderable intensity, and hence their rate of development m be greater than that of their diminution; and, consequen they may be represented by a curve of the form exhibite fig. 20, in which there is no constant part, and in which steepness of A B is gre than that of B C. T are, however, other siderations, which wi noticed hereafter (which may affect the f of the part B C of curve, fig. 20, rendering it still more gradual in its desc or, in other words, which tend to diminish the intensity of ending induction of the secondary current.

Fig. 20.



79. It will be seen at once, by an inspection of the cu that the effect produced, in a third conductor, and whic have called a tertiary current, is not of the same nature as of a secondary current. Instead of being a single deve ment in one direction, it consists of two instantaneous rents, one produced by the induction of A B, and the ot by that of B C, in opposite directions, of equal quantities, of different intensities. The whole quantity of inductio the two directions will each be represented by the ordi B *b*, and hence they will nearly neutralize each other, in ference to their action on the galvanometer, in the circu the third conductor. I say, they will *nearly* neutralize other, because, although they are equal in quantity, the not both act in absolutely the same moment of time. needle will, therefore, be slightly affected: it will be imp in one direction, say to the right, by the induction of A but before it can get fairly under way, it will be arrested, turned in the other direction, by the action of B C. Thi ference is in strict accordance with observation: the ne as we have seen (24.), starts from a state of rest, with a v city which, apparently, would send it through a large arc before it has reached, perhaps, more than half a degr suddenly stops, and turns in the other direction. As

needle is first affected by the action of A B, it indicates a current in the adverse direction to the secondary current.

80. Although the two inductions in the tertiary conductor nearly neutralize each other, in reference to the indications of the galvanometer, yet this is far from being the case with regard to the shocks, and the magnetization of steel needles. These effects may be considered as the results alone of the action of A B; the induction of B C being too feeble in intensity to produce a tertiary current of sufficient power to penetrate the body, or overcome the coercive power of the hardened steel. Hence, in reference to the shock and magnetization of the steel needle, we may entirely neglect the action of B C, and consider the tertiary excitement as a single current, produced by the action of A B; and, because this is the beginning induction (56.), the tertiary current must be in an opposite direction to the secondary. For a similar reason, a current of the third order should produce in effect a single current of the fourth order, in a direction opposite to that of the current which produced it, and so on: we have here, therefore, a simple explanation of the extraordinary phænomenon of the alternation of the directions of the currents, of the different orders, as given in this and the preceding paper. See paragraph 25.

81. The operation of the interposed plate (32, 47, 48, &c.), in neutralizing the shock, and not affecting the galvanometer, can also be readily referred to the same principles. It is certain that an induced current is produced in the plate (III. 64.), and that this must react on the secondary in the helix; but it should not alter the total amount of this current, since, for example, at the ending induction, the same quantity of current is added to the helix, while the current in the plate is decreasing, as is subtracted while the same current is increasing. To make this more clear, let the inductive actions of the interposed current be represented by the parts of the curve, fig. 20. The induction represented by A B will react on the current in the helix, and diminish its quantity, by an amount represented by the ordinate *b* B; but the induction represented by B C will act, in the next moment, on the same current, and increase its quantity by an equal amount, as represented by the same ordinate B *b*; and since both actions take place within a small part of the time of a single swing of the needle, the whole deflection will not be altered, and consequently, as far as the galvanometer is concerned, the interposition of the plate will have no perceptible effect.

82. But the action of the plate on the shock, and on the magnetization of tempered steel, should be very different;

for although the quantity of induction in the helix m
be changed, yet its intensity may be so reduced, by t
verse action of the interposed current, as to fall belo
degree which enables it to penetrate the body, or ove
the coercive force of the steel. To understand how thi
be, let us again refer, for example, to the induction
takes place at the ending of a battery current: this wil
duce, in both the helix and the plate, a momentary cu
in the direction of the primary current, which we have
plus; the current in the plate will react on the helix, an
to produce in it two inductions, which, as before, may
presented by A B and B C of the curve, fig. 20; th
of these, A B, will be an intense action (78.), in the
direction, and will therefore tend to neutralize the i
action of the primary current on the helix; the second
will add to the helix an equal quantity of induced curre
of a much more feeble intensity, and hence the resultin
rent in the helix will not be able to penetrate the bod
shock will be perceived, or at least a very slight one, a
phænomena of screening will be exhibited.

83. When the plate of metal is placed between th
ductors of the second and third orders, or between th
the third and fourth, the action is somewhat different, alt
the general principle is the same. Let us suppose the
interposed between the second and third conductors; th
helix, or third conductor, will be acted on by four indu
two from the secondary current and two from the curr
the plate. The direction and character of these will
follows, on the supposition that the direction of the seco
current is itself *plus*:

The beginning secondary . . . intense and . . . *min*
The ending secondary . . . . . feeble and . . . . *plus*
The beginning interposed . . . intense and . . . *plus*
The ending interposed . . . . . feeble and . . . . *min*

Now if the action, on the third conductor, of the firs
third of the above inductions be equal in intensity and
tity, they will neutralize each other; and the same wil
take place with the action of the second and fourth if
be equal, and hence, in this case, neither shock nor mot
the needle of the galvanometer would be produced. If
inductions are not precisely equal, then only a partial n
lization will take place, and the shock will only be dimi
in power; and, also, perhaps, the needle will be very sl
affected.

84. If, in the foregoing exposition, we throw out of

ideration the actions of the feeble currents which cannot pass he body, and, consequently, are not concerned in producing he shock, then the same explanation will still apply which vas given in the last paper (III. 94.); namely, in the above xample, the helix is acted on by the minus influence of the econdary, and the plus influence of the interposed current.

85. We are now prepared to consider the effect on the ielix (fig. 3.) of the induced currents produced in the conluctor of the primary current itself. These are true secondry currents, and are almost precisely the same in their acion as those in the interposed plate. Let us first examine he induced current at the beginning of the primary, in the ase of a long coil and a battery of a single element; its acion on the helix may be represented by the parts of the curve, ig. 20. The first part, A B, will produce an intense inducion opposite to that of the primary current; and hence the ction of the two will tend to neutralize each other, and no hock, or a very feeble one, will be produced. The ending ction of the same induced current, which is represented by 3 D, restores to the helix the same quantity of current elecricity (but in a feeble state) which was neutralized by A B, nd hence the needle of the galvanometer will be as much afected as if this current did not exist. These inferences perectly agree with the experiment given in paragraph 19. In his, when the ends of the interposed coil were joined so as o neutralize the induced current in the long conductor, the hock at the beginning of the primary current was nearly as owerful as with a short conductor, while the amount of delection of the galvanometer was unaffected by joining the nds of the same coil.

86. At first sight it might appear that any change in the pparatus which might tend to increase the induction of the rimary current (16.) would also tend to increase, in the same legree, the adverse secondary in the same conductor; and hat hence the neutralization mentioned in the last paragraph vould take place in all cases; but we must recollect, that if a nore full current be suddenly formed in a conductor of a given hickness, the adverse current will not have, as it were, as nuch space for its development, and, therefore, will have less ower in neutralizing the induction of the primary than beore. But there is another, and, perhaps, a better reason, in he consideration, that in the case of the increase of the number of elements of the battery, although the rapidity of the levelopment of the primary current is greater, yet the inreased resistance which the secondary meets with, in its moion against the action of the several elements, will tend to

diminish its effect. Also, by diminishing the length of the primary current, we must diminish (76.) the intensity of the secondary, so that it will meet with more resistance in passing the acid of the single battery, and thus its effects be diminished.

87. The action of the secondary current, in the long coil at the *ending* of the primary current, should also, at first sight, produce the same screening influence as the current in the interposed plate; but, on reflection, it will be perceived that its action in this respect must be much more feeble than that of the similar current at the beginning; the latter is produced at the moment of making contact, and hence it is propagated in a continuous circuit of conducting matter, while the other takes place at the *rupture* of the circuit, and must therefore be rendered comparatively feeble by being obliged to pass through a small portion of heated air; very little effect is therefore produced on the helix by this induction (19.). The fact that this current is capable of giving intense shocks, when the ends of a long wire, which is transmitting a primary current, are grasped at the time of breaking the circuit, is readily explained, since, in this case, the body forms, with the conductor, a closed circuit, which permits the comparatively free circulation of the induced current.

88. It will be seen that I have given a peculiar form to the beginning and ending of the curves, figs. 17, 18, &c. These are intended to represent the variations which may be supposed to take place in the rate of increase and decrease of the quantity of the current, even in the case where the contact is made and broken with mercury. We may suppose, from the existence of analogous phænomena in magnetism, heat, &c., that the development of the current would be more rapid at first than when it approximates what may be called the state of current saturation, or when the current has reached more nearly the limit of capacity of conduction of the metal. Also, the decline of the current may be supposed to be more rapid at the first moment than after it has lost somewhat of its intensity, or sunk more nearly to its normal state. These variations are indicated by the rapid rise of the curve, fig. 17, from A to *g*, and the more gradual increase of the ordinates from *h* to B; and by the rapid diminution of the ordinates between C and *l*, and the gradual decrease of those towards the end of the curve.

89. These more minute considerations, relative to the form of the curve, will enable us to conceive how the time of the ending of the secondary current, as we have suggested (78.), may be prolonged beyond that of the natural subsidence of

he disturbance of the electricity of the conductor on which his current depends. If the development of the primary curent is produced by equal increments in equal times, as would e the case in plunging the battery (59.) into the acid with a niform velocity, then the part A B of the curve fig. 17 would e a straight line, and the resulting secondary current, after he first instant, would be one of constant quantity during early the whole time represented by A *c*; but if the rate of he development of the primary current be supposed to vary n accordance with the views we have given in the last pararaph, then the quantity of the secondary current will begin o decline before the termination of the induction, or as soon s the increments of the primary begin to diminish; and hence he whole time of the subsidence of the secondary will be proonged, or the length of *b* C, fig. 20, will be increased, the escent of B C be more gradual, and the intensity of the endng induction of the secondary current be diminished (see ast part of paragraph 78.).

90. Besides the considerations we have mentioned (88.), here are others of a more obvious character, which would also ppear to affect the form of particular parts of the curve. And rst we might perhaps make a slight correction in the drawing f figs. 17, 18, &c., at the point A, in consideration of the ct that the very first contact of the end of the conductor with he surface of the mercury is formed by a point of the metal, nd hence the increment of development should be a little less apid at the first moment than after the contact has become arger; or in other words, the curve should perhaps start a ttle less abruptly from the axis at the point A. Also Dr. 'age has stated* that he finds the shock increased by spreadng a stratum of oil over the surface of the mercury; in this ase it is probable that the termination of the current is more udden, on account of the prevention of the combustion of the etal by means of the oil, and the fact that the end of the conuctor is drawn up into a non-conducting medium.

91. The time of the subsidence of the current, when the ciruit is broken by means of a surface of mercury, is very small, nd probably does not exceed the ten thousandth part of a seond; but even this is an appreciable duration, since I find that he spark at the ending presents the appearance of a band of ght of considerable length, when viewed in a mirror revolng at the rate of six hundred times in a second; and I think he variations in the time of ending of the current under difrent conditions may be detected by means of this instrument.

92. Before concluding this communication, I should state

---

* Silliman's Journal.

that I have made a number of attempts to verify the suggestion given in my last paper (III. 127.), that an inverse induction is produced by a galvanic current by a change in the distance of the conductors, but without success. These attempts were made before I had adopted the views given in this section, and since then I have found (80.) a more simple explanation of the alternation of the currents.

93. In this Number of my Contributions, the phænomena exhibited by the galvanic apparatus have alone been discussed. I have, however, made a series of experiments on the induction from ordinary electricity, and the reaction of soft iron on currents, and I think that the results of these can also be referred to the simple principles adopted in this paper; but they require further examination before submitting them to the public.

[Prof. Henry's 3rd Series appeared in L. & E. Phil. Mag. vol. xvi. p. 200].

LXXVII. *Correction of an Error in Prof.* Dove's *Letter on the Law of Storms. By Sir* David Brewster, *K.H.*

To Richard Taylor, Esq.

Dear Sir,

I OBSERVE in the Philosophical Magazine for November 1840, a letter addressed to you on the Law of Storms, which contains the following passage:—

"In an article upon Lieut.-Colonel Reid's law of storms in the Edinburgh Review, I find my meteorological researches again alluded to, but upon a distinct ground. The anonymous Reviewer, in his patriotic anxiety to satisfy his readers of the purely British growth of this theory, allows that some remarkable passages upon the subject had previously appeared in the memoirs of the Berlin Professor, but that these are mere ingenious speculations, *for they are no more.* The term *passage,* for a memoir (on barometric minima) of seventeen closely printed pages, strikes me as a little extraordinary, but perhaps this arises from my imperfect acquaintance with the nice distinctions of your language. I leave it to my English readers to determine, with what degree of justice results deduced from a greater number of contemporary observations, than (as I believe) had ever previously or have even since been brought together, can be represented as *no more than ingenious speculations.*"

As the article on General Reid's law of storms in the Edinburgh Review was written by me, I feel it necessary to state that *Professor Dove's name is not even once mentioned in that Review, nor his labours in any way referred to.*

Had I been disposed to enter into any discussion respecting

ıe earliest discovery of the rotatory character of storms, I
ıould certainly not have awarded the honour to Prof. Dove,
ıt to the late Colonel James Capper, of the East India
ompany's service. We agree with General Reid in giving
ıe merit of first suggesting the theory to Colonel Capper;
ıt we must at the same time claim for Mr. Redfield the
'eater honour of having fully investigated the subject, and,
)parently, established the theory upon an impregnable basis.

I am, dear Sir, ever yours,

D. Brewster.

St. Leonard's, St. Andrew's, May 15th, 1841.

LXXVIII. *Proceedings of Learned Societies.*

CHEMICAL SOCIETY OF LONDON.

pril 13, 1841. THE following papers were read:—

1. "On the Preparation and Formation of Yellow
russiate of Potash," by Professor Liebig.

In order to explain the reaction between animal matters and carbon-
e of potash, when fused together at a red heat, which gives rise to this
lt, it is necessary to keep in mind the following properties of the salt:
'hen heated to redness in a close vessel, ferrocyanide of potassium
decomposed into cyanide of potassium, carburet of iron and nitro-
en gas; that is, looking upon the ferrocyanide of potassium as a
ouble cyanide, the cyanide of iron is converted into carburet of
on and nitrogen gas, while the cyanide of potassium escapes de-
)mposition. The cyanides of metals in general which can combine
ith carbon, are decomposed in the same way as the cyanide of iron;
ıus the cyanide of silver when heated gives at first a little cyano-
en, but afterwards it fuses, and glowing suddenly, gives nitrogen
ıs, the carbon remaining in combination with the silver. The
ldition of carbonate of potash to the heated ferrocyanide of potas-
um prevents the decomposition of any cyanogen, cyanide of potas-
um being then formed, together with oxide of iron; and when
ıarcoal forms a third ingredient of the fused mixture, the oxide of
on is reduced to the metallic state. Hence ferrocyanide of potas-
um cannot be supposed ready formed in the red-hot mixture of
ıe iron pot in which it is manufactured, that mixture containing
oth charcoal and carbonate of potash.

A general view is then taken of the process of manufacture of
ıis salt. Animal substances, such as dried blood, horn, hoofs, and
ristles, with common pearlashes, are the materials employed. The
ıimal matter is used either in its natural state, or it is previously
ıbmitted to distillation, as in the preparation of ammonia, and the
ısidual charcoal merely employed for the manufacture of the prus-
ate. The projection of animal matter into the melted potash oc-
ısions a lively effervescence, from the evolution of carbonic acid
ıd some combustible gases. The liquid is stirred after each addi-
on of the materials. The usual proportions employed are equal

parts of pearlashes and animal matter, or ten parts of the former and eight parts of carbonized animal matter. Three or four per cent. of iron filings are usually added to the mixture. After each addition of animal matter the heat is urged until the whole is fused, and the melted material, which is of a thick consistence, is not removed from the pot until the charcoal is seen to be equally diffused through the whole mass. The mass, after cooling, is placed in an iron pan filled with water, the clear liquid after a time drawn off, and water boiled several times on the insoluble residue. The liquids are evaporated for crystallizing the salt at a temperature not exceeding 203° Fahr. The formation of prussiate takes place after the solution of the melted mass, by the action of the matters dissolved upon the insoluble residue; for this melted mass yields nothing but cyanide of potassium to alcohol, and contains no prussiate. In explanation of the formation of cyanide of potassium in the melted mass, it is stated that metallic potassium readily produces that salt when fused with calcined blood, disengaging at the same time a considerable quantity of charcoal; the proportion of nitrogen to carbon, in cyanogen, being one equivalent of the first to two of the last, while in blood, hair, and horn, the proportion is 1 to 6. Now when these animal matters are fused at a high temperature with potash, the free charcoal reduces the potash to the state of potassium; the latter then acts upon the azotized carbonaceous matter, forming cyanogen, with which it unites. A second mode in which cyanide of potassium is produced, is when ammoniacal gas is conducted over a mixture of carbonate of potash and charcoal at a red heat. This is accounted for by the action of ammonia upon charcoal alone at a red heat; the gas is entirely converted into hydrocyanic acid and hydrogen (NH_3 and $2C = H, NC_2$ and $2H$). Now hydrocyanic acid decomposes carbonate of potash at a red heat, forming cyanide of potassium. Hence the product of cyanide of potassium is most considerable when the animal matter is used in its natural state, and not previously carbonized, a fact of which the manufacturers of prussiate of potash have long been aware from experience. To account for the subsequent conversion of the cyanide of potassium in the process into prussiate, it is absolutely necessary that iron exist in the fused mass; but it may indifferently be in the condition of metallic iron, the protosulphuret or the protoxide of iron. The first is readily dissolved by a solution of cyanide of potassium with evolution of hydrogen gas ($3KCy$ with HO and $Fe = 2KCy, FeCy$ and KO and H); the second with the formation of sulphuret of potassium, and the third with that of caustic potash. When the iron is added in the state of protosulphate to a solution of cyanide of potassium, one-third of the latter salt becomes cyanide of iron (a brown insoluble matter), which is dissolved by the other two-thirds of the alkaline cyanide, and the ferrocyanide formed. These processes are not altered in the slightest degree by mixing caustic potash or its carbonate, or the sulphuret of potassium, with the solution of cyanide of potassium. Much of the iron necessary, it is well known, is derived from the corrosion of the iron pot in which the fusion is conducted. Professor Liebig assigns an important place to the sulphur of the sulphate of potash, usually present to the extent of 12 or 16 per cent. in pearlashes, in effecting this corrosion. In the decomposition of the sulphate of potash by charcoal, bisulphuret of potassium is formed, and carbonate of potash. Thus,

$$2SO_3 + 2KO \text{ and } 4C = KS_2 \text{ and } KO, CO_2 \text{ with } 2CO_2 \text{ and } CO.$$

The bisulphuret of iron assumes an atom of iron, either from the sides of the iron vessel or from iron filings which are added; the double sulphuret thus formed is very fusible, and will consequently be equally diffused through the melted mass.

The deficiency of product which frequently occurs in the manufacture of prussiate of potash is ascribed principally to two causes: 1st, to the want of iron in the fused mass. The cyanide of potassium is then, instead of being converted into ferrocyanide when thrown into water, decomposed by the free caustic potash when heat is applied to its solution. Uniting with the elements of water, its cyanogen is converted into formic acid and ammonia:

$$NC_2, K \text{ and } 4HO = C_2HO_3 + KO \text{ and } NH_3.$$

This destruction of the cyanide may be avoided by adding iron or its sulphuret to the ley, or better, the protosulphate of iron. Another cause of loss of cyanide in the pot itself is pointed out. The bisulphuret of potassium yields sulphur to the cyanide of potassium, and converts the latter into sulphocyanide of potassium. But if the mixture contain a quantity of iron sufficient to unite with all the sulphur, the formation of sulphocyanide will be prevented. Indeed, sulphocyanide of potassium itself is decomposed by iron at a high temperature, and resolved into sulphuret of iron and cyanide of potassium. It is thus seen that, by increasing the proportion of iron, the formation of sulphocyanide is at once prevented, and sulphuret of iron offered in quantity more than sufficient for its solution afterwards by the cyanide of potassium. The quantity of iron necessary to add in the fusion varies from 12 to 20 per cent., with the proportion of sulphate of potash in the potashes used. If a sulphocyanide appears in the mother liquors, the proportion of iron must be increased. The only remaining condition for the formation of ferrocyanide of potassium, is the complete exclusion of air during the fusion. Cyanide of potassium cannot be kept in fusion exposed to air without absorbing oxygen, and being converted into cyanate of potash; hence the advantage which English manufacturers derive from effecting this fusion in close vessels. Cyanate of potash may also be produced by the action of cyanide of potassium upon the sulphate of potash existing in the potashes, sulphuret of potassium being at the same time formed. Now cyanate of potash is decomposed, by the application of heat to its solution, into carbonate of potash and ammonia. The ammonia which escapes during the evaporation of the ley may therefore come from this source as well as from the decomposition of cyanide of potassium by potash, already adverted to, Mr. G. Lowe observed that the preparation of prussiate of from the waste gas liquors was difficult, owing to the prese sulphur; but he believed that the suggestions of Professor would enable chemists to remove this obstacle to the manufa

2. "On the Formation of Mellon," by Mr. E. A. Parnell, c versity College.

This paper referred to the decomposition which occurs process for mellon, from the substance considered by Lie be the isolated radical of the sulphocyanides (as obtained action of chlorine or nitric acid on sulphocyanide of potas for which substance, having previously shown it to conta drogen and oxygen in addition to the elements belonging true sulphocyanogen, the author proposes the term *metasulph ogen.* It became necessary, therefore, to seek for other prod the decomposition of this substance than those hitherto reco namely, mellon, sulphur, and bisulphuret of carbon; and in posing pure and dry metasulphocyanogen by heat, water, retted hydrogen, and hydrosulphocyanic acid, in addition above, were detected. Admitting the formula for metasulph ogen, $S_{12}\,Cy_6\,H_3\,O$, to which he has been led by analyses, composition is explained as follows:—Three equivalents o sulphocyanogen, containing $S_{36}\,C_{36}\,N_{18}\,H_9\,O_3$, are resolv four of mellon, $C_{24}\,N_{16}$; two of hydrosulphocyanic acid, $S_4\,C_4$ four of sulphuretted hydrogen, $S_4\,H_4$; eight of bisulphuret bon, $S_{16}\,C_8$; twelve of sulphur, and three of water, $H_3\,O_3$. T of the elements of these compounds will be found to co $S_{36}\,C_{36}\,N_{18}\,H_9\,O_3$; or three equivalents of metasulphocyanog

April 27.—The following communications were read:—

1. A letter from Mr. M. Scanlan, of Wolverhampton, des the appearance of flashes of light observed during the crystal of nitrate of strontian in the dark.

2. "Action of Nitric Acid on Castor Oil," by Mr. T. G. (See present Number, page 417.)

3. "On Bleaching Salts," by M. Detmer, Esq. (See Number, page 422.)

4. The following Note by Professor Graham, "On the Prep of Chlorate of Potash." It is well known that the ordina cesses for this important salt are attended with some practic culties. When a stream of chlorine gas is passed through a solution of carbonate of potash, the absorption of the gas and complete, till one-half of the alkaline carbonate is decon but the remaining portion, which is in the state of bicarbo not so easily acted upon. To decompose the latter salt com chlorine must be applied in excess, and the decomposition is a by the formation of free hypochlorous acid, as has been pr Mr. Detmer. The liquid is also at the end highly bleachi contains much hypochlorite of potash. The boiling neces convert the latter into chlorate of potash and chloride of po occasions, according to M. Morin, a considerable loss of and thus lessens the product of chlorate. When a strong s

of caustic potash is substituted in this process for the carbonate, the absorption of chlorine proceeds without interruption; but the liquid when saturated bleaches strongly from hypochlorite formed. A long-continued boiling is required to destroy this property completely, and as oxygen escapes, the chlorate obtained must be deficient in quantity in a corresponding proportion. The process which the author recommends, and which is attended with none of these inconveniences, consists in mixing carbonate of potash intimately with an equivalent quantity of dry hydrate of lime, and exposing the mixture to chlorine gas. This mixture, although quite dry, absorbs the gas with prodigious energy, the temperature rises much above 212°, and water is freely evolved. When saturated it may be moderately heated, which destroys a mere trace of hypochlorite it contains. The whole lime is found in the state of carbonate, and the potash as chlorate and chloride of potassium. The solution of the two latter salts is neutral, without any bleaching property, and free from lime. The chlorate of potash may be crystallized from it in the usual way. Carbonate of potash, when moistened and exposed to chlorine, without the hydrate of lime, absorbs the gas with great avidity, and certainly answers better than a strong solution of the same salt; but the absorption becomes slow after the salt is in the state of bicarbonate, and subsequently a large quantity of the bleaching hypochlorite of potash is produced. In the new process described above, there is no reason to believe that the carbonate of potash is decomposed by the dry hydrate of lime till the chlorine is presented to the mixture; then, while the lime attracts the carbonic acid, the chlorine acts simultaneously upon the potash, and the carbonate of potash is thus readily decomposed. The same principle of calling in a secondary agency to promote combination may be taken advantage of in many other cases. One of these, of some interest, is the promotion of the absorption of sulphuretted hydrogen by hydrate of lime, through the influence of other salts. Thus hydrate of lime, dry or slightly damped, ceases to absorb sulphuretted hydrogen long before it is saturated with that gas; but if mixed with an equivalent of hydrated sulphate of soda, the absorption takes place with greatly increased avidity, and goes on till two equivalents of sulphuretted hydrogen are taken up for one equivalent of lime. But here, with the assistance of sulphuretted hydrogen, the hydrate of lime decomposes the sulphate of soda, sulphate of lime being formed, while caustic soda combines with the sulphuretted hydrogen.

The author has found that the last mixture may be applied with advantage, from its great absorbing power, in purifying coal-gas, where the highest degree of purification is desirable, and where the products, sulphate of lime and hydrosulphuret of sulphuret of sodium, can be œconomically applied. He recommends it to be introduced into the last of the dry lime-purifiers.

5. An extract from a letter from Ollive Sims, Esq., Shelton, Staffordshire Potteries, was read, announcing a considerable and very accessible source of the hitherto very rare mineral, phosphate of yttria.

The crushed cobalt ore, from Johannisberg in Sweden, whe verted into zaffre, or dissolved by acids, leaves a yellowish n in crystalline grains, in the proportion of about one pound av pois from one thousand pounds of ore. This mineral is the phate of yttria. It may be decomposed by fusion with a carbonates, or by boiling with pretty strong sulphuric acid.

LONDON ELECTRICAL SOCIETY.

May 18th, 1841.—The Secretary presented the Society copy of the second edition of his "Electrotype Manipulation

A letter from Thomas Pine, Esq. was read, containing many esting facts on the power exercised by the points of living veg in "drawing off" electricity from the atmosphere. That ma portant functions in vegetation result from this, the author con from the singular circumstance of dew being deposited only apex of points or leaves. Apparently smooth leaves are, wh amined by a microscope, found studded with these natural tors; and, which is still more illustrative of the case in qu plants vegetate with more vigour in an electrified atmospher when the soil is electrified. This communication was merely a ral sketch of conclusions which the author promised to illustrat at large on a future occasion.

Read, "Some general observations on Electrotype Manipu and on the construction of a constant Acid Battery." By C V. Walker, Esq., Hon. Sec.

The conclusions adduced by the author are the results of series of experiments, and tend to generalize the subject. scribes the most advantageous diaphragms, battery cells, and ma for moulds, and speaks very much in favour of a "constan battery," the construction of which is described. It is slow action, but the deposits obtained are in every respect good battery can be better fitted to deposit coatings on objects.

CAMBRIDGE PHILOSOPHICAL SOCIETY.

March 22, 1841.—Professor Miller gave an account of his ob tions on supernumerary rainbows, made for the purpose of com the observed places of the principal bows, and their supernume with their places as computed by the Astronomer Royal on the latory hypothesis. (Transactions of the Cambridge Philosophic ciety, vol. vi. p. 379.) The bows were formed in the manne employed by M. Babinet, by a vertical cylindrical stream of The incident light was homogeneous, or nearly so. In some thirty supernumerary bows were seen within the primary twenty-five exterior to the secondary.

The following table exhibits the observed radii of the bri part of each principal bow and its first and second dark ring the theoretical radii of the brightest part of each principal bo its second dark ring, computed from the interval between the g trical bow and the observed place of the first dark ring,

1. Diameter of cylinder of water = 0·0206 inch; index of refraction = 1·3318.

(Radius of geometric primary bow = 42° 15′.)

	Observation.	Theory.
Radius of brightest part of primary	41° 51′·4	41° 45′·4
Radius of first dark ring	41 7	
Radius of second dark ring	40 16	40 14·4

(Radius of geometric secondary bow = 50° 34′.)

Radius of brightest part of secondary	51° 25′	51° 27′·5
Radius of first dark ring	52 37	
Radius of second dark ring	54 7	54 12.

2. Diameter of cylinder of water = 0·02105 inch; index of refraction = 1·33464.

(Radius of geometric primary bow = 41° 50′·4.)

	Observation.	Theory.
Radius of brightest part of primary..	41° 27′·7	41° 24′·7
Radius of first dark ring	40 51·4	
Radius of second dark ring	40 4·4	40 5′·7

(Radius of geometric secondary bow = 51° 19′).

Radius of brightest part of secondary	51° 57′	52° 5′·3
Radius of first dark ring..........	53 5	
Radius of second dark ring........	54 27·6	54 27

3. Diameter of cylinder of water = 0·0135 inch; index of refraction = 1·33453. In this series of observations the values of the diameter of the cylinder and of the index of refraction are rather doubtful.

(Radius of geometric primary bow = 41° 52′.)

	Observation.	Theory.
Radius of brightest part of primary	41° 20′	41° 18′
Radius of first dark ring..........	40 33	
Radius of second dark ring........	39 29	39 32

(Radius of geometric secondary bow = 51° 17′·5.)

Radius of brightest part of secondary	52° 16′	52° 18′·5
Radius of first dark ring..........	53 37	
Radius of second dark ring........	55 31	55 26.

April 26, 1841.—Prof. Challis read a communication on the motion of a small sphere submitted to the dynamical action of the vibrations of an elastic medium. The mathematical reasoning embraced terms involving the *square* of the velocity of the vibrating medium, and the principal conclusion arrived at was, that the motion of the sphere consists partly of a vibratory motion, and partly of a permanent motion of translation, the latter depending on the terms which contain the square of the velocity. It was thought that this result may have important applications in the physical theories of light and heat.

The solution of the above problem involves that of another of more immediate interest, viz. the determination of the resistance to the motion of a ball-pendulum vibrating in the air. Professor Challis obtains the same coefficient of resistance as in several previous solutions which he has given of this question, the terms involving square of the velocity being found to have no effect on its value. difference between his result and that obtained by other writer the same question, is shown to be entirely owing to his adopt new principle in the application of analysis to fluid motion, w may be thus stated. If u, v, w be the velocities resolved in th rections of three rectangular axes of a fluid particle situated a point whose coordinates are x, y, z, then, in order that the m may admit of determination by the integral of a partial differe equation, it is necessary to suppose that $u\,dx + v\,dy + w\,dz$ exact differential of a function of x, y, and z. This supposition therefore be true, independently of the particular mode of disturb and of the form of the arbitrary function by which it is expre The quantity in question is an exact differential, for reasons d from the nature of curve surfaces, and independently of all th arbitrary, if the variation of the coordinates from one point to an at a given instant be in the direction in which the motion is impr by the arbitrary disturbance, and not otherwise. Hence the v tion of the coordinates in the partial differential equation to w that supposition conducts, must be subject to the same limita It is plain that this principle, if true, must materially affect the ner of treating many hydrodynamical problems, and would therefore to merit the attention of mathematicians[*].

GEOLOGICAL SOCIETY.

June 10, 1840.—A notice of a mass of trap in the mou limestone on the western extremity of Bleadon Hill, Somerset and on the line of the Bristol and Exeter Railway, by the Re Williams, F.G.S.

This is the first discovery of trap *in situ* in the Mendip Hills Somersetshire, with the exception of the Hestercombe granite scribed by Mr. Horner[†], and a slate porphyry, observed by Mr. liams, a little north of Simmon's birth, in Exmoor. The rock v in character from a granular to a porphyritic and amygdal greenstone. It occurs near a line of fault, which has brough lias on a level with the carboniferous limestone; and when firs posed on the eastern side of the railway cutting, it appeared conformably interstratified with the limestone; but the cuttin the western side (the line of railway ranging north and south subsequently proved that the trap is clearly intrusive, intersecti a considerable angle the limestone beds. On the east side trap is in contact with the lias, but no change appears to have produced in that formation, though the mountain limestone is s to be considerably altered. The trap at the lower part prese broad bed-like mass, but it rapidly diminishes in its upward c through the limestone thinning away entirely. Mr. Williams st that the limestone appears to have yielded along the line of o the north-west joints. He acknowledges his obligation to Mr. Peniston, the resident engineer, for a correct section of the cutting.

---

[*] [On the subject of this communication see L. E. and D. Phil. vol. xvii. p. 465: also present vol. pp. 132, 321, and Mr. Challis's comm cation in the present Number.—Edit.]

[†] Geol. Trans., 1st Series, vol. iii. p. 348.

A memoir descriptive of a "Series of Coloured Sections of the Cuttings on the Birmingham and Gloucester Railway," by Hugh Edwin Strickland, Esq., F.G.S.

The author commences by expressing his regret at the irrecoverable loss, which science has experienced, in full advantage not having been taken of the valuable geological information, which has been exposed by the railway cuttings in different parts of England during the last ten years; and he suggests the propriety of each line of railway being systematically surveyed by a competent observer, while the cuttings are in progress.

Anxious to contribute towards so desirable an end, Mr. Strickland gladly yielded to a request made to him by Captain Moorsom, the chief engineer of the Birmingham and Gloucester Railway, to undertake a geological survey of the line; and he expresses his obligations to that gentleman and to Captain J. Vetch for the valuable assistance they afforded him. The line was originally surveyed by Mr. Burr, when only the trial shafts had been sunk, and before the cuttings were commenced; but Mr. Strickland bears testimony to the accuracy of the account which Mr. Burr laid before this Society.—(Geol. Proceedings, vol. ii. p. 593; or L. & E. Phil. Mag. vol. xii. p. 573.)

The direction of the railway ranges nearly parallel to the strike of the strata, and therefore intersects only the new red sandstone and red marl, the lias, and superficial detritus.

New red sandstone and red marl.—The lowest rock exposed belongs to the new red or bunter sandstone, resting on the anticlinal axis of the Lickey, ten miles south-south-west of Birmingham, and one mile south of the termination of the altered rock, or Lickey Quartz [*]. The sandstone is there thick-bedded, soft, and red, and dips on the western flank about 5° west-south-west, and on the eastern 5° east-south-east. In Grovely Hill, on the north-east of the Lickey, it passes occasionally into a hard quartzose conglomerate with a calcareous paste [†]; and at Finstal, on the south-west of the Lickey ridge, the upper portion of the sandstone is light-coloured, and contains obscure vegetable impressions, being a prolongation of the stratum, with similar impressions, at Breakback Hill, on the west of Bromsgrove [‡].

On each side of the Lickey, the sandstone is conformably overlaid by red marl, which extends on the north-east to Birmingham[§], and on the south-west to Stoke Prior and the neighbourhood of Hadnor,

[*] See Mr. Murchison's Silurian System, p. 492.

[†] Similar conglomerates occur in Worcestershire, Staffordshire, and Warwickshire.—Silur. Syst., p. 42. Geol. Trans., 2nd Series, vol. v. p. 347.

[‡] Geol. Trans., 2nd Series, vol. v. p. 341; Proceedings, vol. ii. p. 564; [or L. and E. Phil. Mag., vol. xi. p. 319.—Edit.]

[§] The red marl extends from Birmingham along the London railway as far as Berkswell, forming the basin, in which occurs the lias outlier of Knowle south-west of Berkswell. The true boundary of the sandstone and marl in this district has been only recently ascertained; it ranges from where the railway intersects a ridge of lias. On the north marl is there cut off by a fault, but on the south, at Dunha the following juncture section is exposed :—

(*a*.) Lias clay with contorted beds of lias limestone.
(*b*.) White micaceous sandstone, with numerous specimens of a smooth oval bivalve . . . . .
(*c*.) Lias clay . . . .
(*d*.) Grey marl 3
(*e*.) Red marl . . . .

Dip of the beds 5° north-north-east.

In the hill south of Dunhamstead, the grey marl (*d*) abuts the red marl (*e*) in consequence of a fault. For the next fiv the railway traverses a valley of red marl, between the esca of the lias and a ridge of Keuper sandstone. On the south Spetchley the strike of that sandstone is altered by a fau south by east to south-west, and a projecting angle has be duced which is intersected by the railway. This stratum is representative of the Keuper sandstone of Burg Hill, &c. sisting chiefly of greenish marl with thin laminæ of whit stone, about twenty feet thick, with red marl above and At Norton the railway ascends the lias escarpment, and cuts a section exactly analogous to the one given above. further south the lias clay contains many calcareous conc abounding with fossils, including *Plagiostoma giganteum, minima*, and a coral. At Abbot's Wood the fissile sandston base of the lias is again exposed, having been brought up by At Defford and Eckington the lias clay encloses numerou mens of *Pachyodon Listeri* (Stuchbury), or *Unio Listeri* of S and *Ammonites Turneri*. At Bredon a higher portion of series was reached, and a different suite of fossils found, t marked being *Pleurotomaria Anglica, Hippopodium ponderosu phæa incurva, Nautilus striatus,* and several species of Amn Between Cheltenham and Gloucester the lias has yielde abundance of organic remains, a considerable number of wh considered to be new, and with the exception of *Hippopodium p sum, Gryphæa incurva,* and one or two others, they are distin the fossils of Bredon Hill; and at Hewlitt's, east of Cheltenh lias near the base of the marlstone presents another series of fossils. The lower lias, therefore, Mr. Strickland observes, evidences of at least four well-marked successions of mol faunæ, in a vertical height of 400 or 500 feet, and unaccom by any change in the mineral character of the deposits.

Superficial detritus.—The author then proceeds to desc deposits of superficial detritus, and he states, that they entir firm the views which he had previously entertained, respect distinction between the ancient terrestrial alluvia in which b

---

Hewell Grange, nearly north, by Cofton Hacket to Northfield, an north-east to the south suburbs of Birmingham.

* Proceedings, vol. ii. p. 503; [or L. and E. Phil. Mag., vol. xi. —Edit.] Geol. Trans., 2nd Series, vol. v. p. 332.

mammalia occur, and the submarine drift which covers most parts of the island*.

He divides the detritus into fluviatile and marine; and the latter, according to its origin, into local and erratic; and this, according to its composition, into gravel with flints and without flints.

Marine erratic gravel without flints†.—Commencing his details with the Birmingham end of the line, Mr. Strickland shows, that these accumulations occur extensively on all sides of that town, and at intervals along the line of the railway till it approaches the valley of the Avon. Mammalian remains appear to be totally wanting. Chalk flints are so extremely rare in it around Birmingham as to prove that the materials were transported from the north. At Mosely it is upwards of 80 feet thick, and consists of rolled pebbles, rarely exceeding 4 inches in diameter, of various granitic and quartzose rocks and altered sandstones, imbedded in a clean ferruginous sand; and a bed of sand 30 feet thick, without pebbles, occurs in the middle of the gravel. Between Cotteridge and Wytchall is an erratic boulder, or shapeless mass of porphyritic trap, about 5 feet by 4, with the angles slightly rounded. At the Lickey, gravel analogous to that near Birmingham, but with a large proportion of slate rocks, attains, on the line of the railway, a height of 387 feet, and at the Lickey Beacon of more than 900 feet. Sugar's Brook is the next locality noticed by Mr. Strickland, but from that point no gravel occurs for sixteen miles. Near Abbot's Wood is another extensive deposit of quartzose gravel and ferruginous sand, devoid of flints and resting upon lias.

Marine erratic gravel with flints.—These accumulations commence immediately south of the Avon. The village of Bredon stands on a platform, seventy feet above the ordinary level of the Avon, composed of lias with an uneven surface, and capped with 10 to 15 feet of this gravel. It contains no mammalian remains.

Fluviatile gravel.—The only example of this drift, on the line of the railway, occupies the two opposite flanks of the Avon at Defford and Eckington, north of Bredon. At these localities the surface is a tabular platform which does not exceed forty-five feet above the Avon, including a capping of ten feet of gravel precisely similar to the flinty gravel of Bredon, but containing abundance of mammalian remains. They were chiefly found in the cutting north of Eckington, at the lower part of the deposit, and often on the surface of the lias clay; and are referrible to *Elephas primigenius, Hippopotamus major, Bos Urus*, and *Cervus giganteus*? On the north, or Defford side of the Avon, the remains of *Elephas primigenius* and *Rhinoceros trichorhinus* have been obtained. Associated with these bones are numerous freshwater shells, agreeing with those found at Cropthorne‡; the most abundant species being *Cyclas amnica* and *C. cornea*. In endeavouring to account for the presence of these re-

---

* See Reports of the British Association, vol. vi., Sessional Meetings, p. 61.

† Northern drift of Mr. Murchison, Silur. Syst., p. 523.

‡ Silur. Syst. p. 555; and Proceedings, vol. ii. pp. 6 and 95; [or L. and E. Phil. Mag., vol. iv. p. 148; vol. v. p. 297.—Edit.]

mains at only one point in the line of the railway, Mr. Stric
states that he can offer no other explanation than that prev
proposed by him *, namely, that after the beds of marine grav
been deposited and laid dry by the elevation of the land, a
river or chain of lakes extended down the valley of the Avon
height varying from twenty to fifty feet above its present c
and that the gravel previously accumulated by marine curren
remodified by the river, and mixed up with remains of mam
which tenanted its banks, or of mollusca which inhabited its w

Local gravel.—This species of detritus occurs abundantly at
tenham, and consists exclusively of detritus from the oolites a
of the vicinity. No bones or terrestrial remains have been fo
it; and, therefore, the author assigns to it, in the absence of
evidence, a marine origin.

Modern alluvia.—The only deposits of this nature mention
the paper, are the peaty accumulations on the banks of the
and its tributaries.

The memoir was accompanied by a copy of the Railway Se
and of the Tewkesbury branch, and the junction branch fro
main line to the London and Birmingham Railway, present
Capt. Moorsom, but coloured geologically by Mr. Strickland.

A letter addressed to Mr. Murchison by Capt. Lloyd,
London, May 11th, 1840.

Having read in the instructions prepared by the Royal Socie
the Antarctic Expedition under Capt. James Ross, that the isl
Bourbon presents indications of the sea having formerly occu
higher level than at present, and having observed similar a
ances in the Mauritius, Capt. Lloyd was induced to lay the fo
ing facts before the Society.

The island of Mauritius is belted by an enormous cora
throughout its whole circumference, except for about ten mi
the broadest and extreme southern side, or from Point Souffle
Souillac, commonly called Port Savanne. Along that part
island the coast is bold, and consists of a basaltic rock.

Near the Rivière des Galets, between Savanne and the B
Cap, the sea foams against a barrier of coral from five to fiftee
in height, and wears it into the most fantastic shapes. At
siderable distance inland, and almost concealed by trees and s
are two remarkable points or headlands of coral, from twen
twenty-five feet above the present level of the sea. They p
the same marks of abrasion as the barrier reef now unde
the action of the waves. The Observatory, Port Louis, is buil
on a stratum, ten feet above high-water mark, of very hard
which requires blasting. There are besides in several parts
island, and at considerable distances inland, enormous blo
coral surrounded with the debris of oyster and other shells and b
corals. Appended to Capt. Lloyd's communication are two l
from agents employed by him to collect information respecti
land blocks of coral. One of the letters is from Mr. Hill, su

---

* Reports of British Association, vol. vi. Sections, p. 64.

of roads, and contains the following data respecting two blocks near Souillac:—

	1st Block.	2nd Block.
Distance from the sea	610 feet	1356 feet.
Probable height above high water	50 —	
Length	12 —	30 —
Breadth	10 —	12 —
Height	7½ —	14 —
Girth round the largest projections	40 —	77 —

If the first of these blocks had been transported by the sea, Capt. Lloyd says, it could have attained its present position only by passing over the almost perpendicular coast.

The other letter is from Mr. Sherlock, and gives the following measurements of two blocks on the Black river:—

	Height.	Width.	Circumference.	Distance from sea.
1st block	13 feet	30 to 40 feet	121 feet	350 feet.
2nd block	10 —	25 feet	Length, 41 feet	840 —

Mr. Sherlock adds, there is no coral in the interior, except a small bed on the habitation, Le Gentele.

METEOROLOGICAL OBSERVATIONS FOR APRIL 1841.

Chiswick.—April 1. Cloudy. 2. Fine: clear. 3. Slight haze: cloudy and fine. 4. Cloudy: slight rain. 5. Cloudy and fine. 6, 7. Very fine. 8. Fine in the morning: hail-shower at 1 P.M.: cloudy and fine at night. 9. Overcast and cold. 10—12. Cloudy and cold. 13. Cloudy: rain. 14. Cloudy: slight rain. 15. Showery. 16, 17. Fine. 18. Overcast and cold. 19. Fine. 20. Overcast. 21. Cold and dry. 22. Cloudy and cold. 23. Heavy rain. 24. Fine. 25. Very fine: slight rain: cloudy and windy at night. 26. Hazy: fine. 27. Very fine. 28. Rain. 29. Rain: very fine: clear at night. 30. Dry haze throughout the day: clear at night.

Boston.—April 1. Fine: rain early A.M. 2. Stormy: rain early A.M. 3, 4. Fine. 5. Cloudy: rain P.M. 6. Fine. 7. Cloudy: rain P.M. 8. Cloudy. 9. Fine: rain early A.M. 10. Cloudy. 11. Cloudy: rain early A.M.: hail-storm P.M. 12. Cloudy: rain early A.M. 13. Fine: rain early A.M. 14, 15. Fine: rain P.M. 16. Fine: ice this morning. 17. Fine. 18. Cloudy. 19. Fine. 20. Fine: hail and rain P.M. 21. Cloudy: rain P.M. 22. Cloudy: rain early A.M. 23. Rain. 24. Fine. 25. Fine: rain early A.M. 26. Cloudy. 27, 28. Fine. 29. Cloudy. 30. Fine.

Applegarth Manse, Dumfries-shire.—April 1. Fine A.M: rain P.M. 2. Fine: showers P.M. 3. Fine: one shower. 4. Fine A.M.: rain P.M. 5. Fine: slight shower. 6. Fine and fair all day. 7. Rain P.M. 8. Fair A.M.: shower P.M. 9. Fine and fair all day. 10, 11. Occasional showers. 12. Fine and fair. 13. Wet P.M. 14. Showery P.M. 15. Rain and hail. 16. Rain, sleet and hail. 17—20. Showers. 21. Fair and cold. 22, 23. Frosty morning: fine. 24. Very wet P.M. 25. Rainy forenoon. 26. Wet all day. 27. Rain A.M.: cleared up. 28. Beautiful day: thunder and rain. 29, 30. Fine and fair.

Sun shone out 27 days. Rain fell 20 days. Thunder 2 days. Frosty mornings 2. Hail 2 days.

Wind north ½ day. North-east 3 days. East 2 days. South-east 4 days. South-south-east 2 days. South 2 days. South-south-west ½ day. South-west 7 days. West-south-west 1 day. West 6 days. North-west 1 day. North-north-west 1 day.

Calm 6 days. Moderate 14 days. Brisk 4 days. Strong breeze 4 days. Boisterous 2 days.

Mean temperature of the month	44°·40
Mean temperature of April 1840	48 ·05
Mean temperature of spring-water	51 ·00

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTON; *by* Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. VEALL *at Boston, and by* Mr. DUNBAR *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. April.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dum-fries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8¾ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·642	29·632	29·596	29·15	29·51	29·40	46·3	54·7	43·7	52	41	46	51	38	W.	W.	N.	SW.	·061	·05	·08	...	45
2.	29·662	29·727	29·600	29·12	29·45	29·60	47·7	51·7	44·6	56	26	47	48½	36½	NW.	NW.	NW.	W.	·058	...	·04	...	46
3.	29·728	29·707	29·640	29·25	29·49	29·47	45·0	53·6	38·2	57	26	43·5	48½	35½	SW.	NW.	calm	SW.	...	...	...	...	44
4.	29·676	29·642	29·467	29·14	29·35	29·23	43·7	53·6	38·3	55	39	46	51	36½	S.	SW.	calm	WSW.	...	·11	...	...	41
5.	29·404	29·436	29·371	28·94	29·28	29·55	47·0	56·0	43·0	56	35	47·5	51	40½	SE.	S.	calm	E.	·102	...	...	...	43
○ 6.	29·710	29·745	29·661	29·28	29·64	29·63	46·4	54·2	43·2	55	31	46·5	53½	35	N.	NE.	calm	E.	...	·02	·08	...	43
7.	29·806	29·781	29·777	29·40	29·60	29·56	43·3	53·8	40·3	52	37	38	49½	39½	E.	SW.	N.	S.	·036	·03	...	0·11	40
8.	29·806	29·779	29·775	29·29	29·45	29·68	47·0	54·0	42·4	53	34	45	52	38½	NW.	W.	W.	NW.	·033	...	·07	...	45
9.	29·910	29·988	29·820	29·43	29·83	29·93	48·0	52·5	41·7	55	37	47	51½	32	NW.	N.	calm	NNW.	·125	·01	·03	...	44
10.	30·082	30·020	29·958	29·62	29·98	29·97	44·7	53·7	40·3	49	31	46	49	37	NW.	E.	calm	NE.	...	...	...	...	41
11.	29·954	29·939	29·923	29·65	30·00	30·00	40·8	46·7	38·6	49	32	42	45½	33	N.	NE.	calm	NE.	...	·01	·07	...	38
12.	29·912	29·962	29·907	29·54	29·90	29·80	41·2	47·2	36·7	45	27	43	48½	32½	NNW.	NE.	calm	N.SSW.	·025	...	·05	...	35
☾ 13.	30·100	30·130	30·077	29·63	29·78	29·83	45·3	46·6	37·7	56	42	46	52	38½	S.	SW.	calm	W.	...	·08	·02	...	38
14.	30·110	30·088	29·983	29·58	29·76	29·78	49·8	53·0	45·0	57	32	49	51	42	S.	SW.	calm	W.	·080	·01	...	0·70	41
15.	29·916	29·899	29·761	29·31	29·65	29·55	48·7	56·7	42·4	58	26	47	47	35	S.	NW.	calm	SW.	...	·05	·10	...	43
16.	29·742	29·762	29·714	29·28	29·48	29·56	45·7	55·3	37·7	57	31	46·5	49	37½	SSW.	NW.	calm	W.	·063	...	·10	...	41
17.	29·914	29·946	29·874	29·45	29·75	29·63	46·4	54·6	42·4	60	39	48	53	33	W.	NE.	calm	SW.	...	...	...	...	41
18.	29·972	29·975	29·924	29·37	29·67	29·67	49·4	54·6	42·8	61	41	51	50½	40½	SW.	W.	calm	SW.	...	·14	...	...	43
19.	29·908	29·895	29·881	29·30	29·60	29·70	48·7	59·2	44·8	58	32	48·5	51	39½	NW.	W.	NW.	W.	·166	...	...	...	42
20.	29·800	29·803	29·682	29·27	29·54	29·70	46·8	56·8	42·2	56	36	48	49	39	W.	W.	calm	W.	...	...	...	...	39
● 21.	29·852	29·904	29·810	29·46	29·92	30·03	47·6	52·8	41·7	53	40	46·5	47½	37	NNE.	NE.	calm	NE.	...	...	·36	0·12	45
22.	29·882	29·866	29·734	29·48	29·94	29·72	45·3	53·2	41·0	53	41	45	49	28½	NE.	NE.	E.	SW.	...	·32	·07	...	39
23.	29·424	29·645	29·450	29·10	29·53	29·41	43·7	51·6	42·3	48	28	44	51	31	NW.	NW.	E.	SW.	·500	·52	·09	...	44
24.	29·712	29·677	29·528	29·20	29·37	29·18	48·5	55·0	40·2	56	39	49	51½	33½	S var.	SW.	calm	SE.	·333	·15	·32	...	43
25.	29·688	29·846	29·650	29·13	29·28	29·20	50·5	65·7	43·9	59	52	50	54	38	S.	S.	W.	SSE.	·072	...	·21	...	46
26.	30·040	30·100	29·980	29·39	29·53	29·74	57·7	59·3	50·2	66	55	59	51	45½	S.	SW.	SW.	S.	...	...	...	...	51
27.	30·140	30·103	30·078	29·47	29·80	30·00	63·3	72·2	56·5	75	46	65·5	55	47½	S.	SW.	calm	SSE.	...	...	...	...	56
☽ 28.	30·200	30·132	30·099	29·50	30·10	30·10	63·3	72·4	57·0	76	45	66	61	41½	N.	NE.	calm	SE.	·022	·07	...	1·00	58
29.	30·152	30·141	30·098	29·56	30·06	30·18	52·3	71·6	50·6	71	42	53	59	45½	N.	NE.	calm	SE.	·072	·01	...	...	55
30.	30·226	30·171	30·091	29·68	30·17	30·03	58·4	69·2	46·6	68	41	53·5	64	41	NE.	NE.	calm	SE.	...	...	...	...	47
Mean.	29·869	29·881	29·796	29·36	29·681	29·694	48·4	56·4	43·2	57·40	36·79	48·4	51·5	37·6					Sum. 1·748	1·58	1·69	1·93	Mean. 44

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE,
SUPPLEMENT TO VOL. XVIII. THIRD SERIES.

LXXIX. *Examination of a Fourth Experiment adduced by* Prof. Faraday *in support of* M. de la Rive's *Theory, and regarded by Dr.* Fusinieri *to be demonstrative. By Dr.* STEPHEN MARIANINI, *Acting Professor of Particular and Experimental Physics in the University of Modena, &c.*

[Continued from p. 202.]

Second Article. Examination of the Observations recently made by Prof. de la Rive *in support of his Theory, and which Dr.* Fusinieri *has considered sufficient to answer the preceding Objections**.

X. DR. AMBROGIO FUSINIERI seems to blame me for not having alluded, in my fourth Memoir†, to the interesting work of M. de la Rive, entitled, "Inquiry into the Cause of Voltaic Electricity‡." I did not instantly allege in my excuse the having written and sent the said memoir to the Secretary of the Italian Society of Sciences, some time previous to my becoming acquainted with the said work, which had been kindly presented to me only in the November of 1836, by my illustrious colleague, Prof. de la Rive, this circumstance having little interest for the scientific, for whom I intend to write, and still less for science; but I applied myself instead to study the work itself, as soon as I was able to do so.

Whoever does not know this production of M. de la Rive, no doubt, on hearing how Signor Fusinieri speaks of it, would believe that in the same manner in which, in my second memoir, I had examined all the arguments and principal experiments brought forward by the Professor of Geneva in support of his theory, and had shown the insufficiency of them with other experiments; so also M. de la Rive might undertake to examine my arguments, and might show the erroneousness of them; but what would be his surprise, when setting himself to read this work (without doubt having other claims to interest), he should see that the same facts are reproduced which were already published by him in 1828, when he wished to found his theory; and of the many arguments and experiments brought forward by me, he only speaks of two or three secondary in importance, and not with the profundity which one might expect from so distinguished a physicist!

---

* *Annali de Scienze del Regno Lombardo-Veneto, Bimestre* iv., 1837, p. 192. † Ibid. ‡ *Recherches, &c.*

In the first part of my said memoir I treat of the influence which voltaic currents exercise in changing the relative electromotive property of the metals, and of that which is exercised upon them by the liquid conductors, in which are immersed the metals themselves; and I established some facts which ought to serve as the *rationale* of some experiments produced by M. de la Rive as inexplicable except upon his theory. But of these we will not speak here. Nor let any one interpret this silence into denial or disapprobation, since the same M. de la Rive, in his historical sketch of the principal facts discovered in electricity, makes mention of all these, and not only without starting a doubt of them, but also with expressions very flattering to me*.

In the second part of that memoir I undertake to show the insufficiency of the theory of M. de la Rive to explain the phænomena of the electromotors, as well simple as complex.

Treating of the simple electromotors, and of the case in which the two plates are immersed in the same liquid, M. de la Rive adduced in support of his theory that an electric current is produced when there are immersed in one liquid two portions of the same metal susceptible of being attacked by it. And I observed that if these two portions have some heterogeneity, the fact accords with the theory of Volta; if not, there is no theory, either chemical or physical, which can explain it. (Memoir above mentioned, § XXIII.)

M. de la Rive observed that if a voltaic pair of copper and tin is immersed in an acid or saline solution, the tin is positive; and if it is immersed in ammonia, the copper is positive, because more acted upon by the liquid than the tin. And I showed that this second part of the experiment is not true, except after the ammonia may have altered, in a contrary sense, the electrotism of the two metals, and that, in consequence, it contradicts the theory of M. de la Rive, instead of supporting it. The same observations I made upon the similar experiment of M. de la Rive, with copper and iron. (§ XXIV.)

Another fact which M. de la Rive adduced was, that on immersing iron and lead in concentrated nitric acid, the iron in the first moment is negative, "*because* (it was said) *there*

---

* *Esquisse historique des principales découvertes faites dans l'électricité depuis quelques années.* Par M. Auguste de la Rive. Genève, 1833.

was not yet any chemical action;" but if one waits until the chemical action shall commence, or if the part of the plate of iron immersed in the liquid is exposed for a moment to the air, which quickly determines the action, that iron itself becomes positive. And I in the first place inquired whence might arise the negative electrization in the iron, and consequently positive in the lead, in the first instant that the copper is immersed, if it had yet no chemical action. And how stands now the theory of M. de la Rive? In the second place I demonstrated that the nitric acid increases the electrotism both in the iron and the lead, but more in the latter than in the former. I showed, in the third place, that it was an error to believe that causing the iron to be exposed to the air might be the cause by which it was electrified positively, on again putting it into the acid, because the cause of this positive electrization of the iron was the alteration which the lead suffered in the acid in which it was left immersed whilst the iron was kept in the air. And I proved, lastly, that to explain those experiments by the chemical theory, it is necessary to suppose now that the action of the acid renders the iron positive with respect to the lead, now that it renders it negative; now we must suppose that the action of the acid may be less forcible in the first instants upon the iron than upon the lead; now we must suppose the contrary. (§ XXVII.)

Another experiment quoted by M. de la Rive was, that two plates, the one of gold, the other of the purest platina, being fixed to the extremity of the galvanometric wire, and immersed in the pure nitric acid, a very sensible current took place when a drop of hydrochloric acid was added to the said liquid. And I demonstrated by ten experiments, first, that the nitric acid, mixed with very little hydrochloric acid, increases the relative electromotive faculty both in the gold and in the platina, but more in the latter than in the former; secondly, that such phænomena are not derived from electricity immediately developed by the chemical action; thirdly, that it is impossible to explain all the phænomena which are observed n these experiments by the new theory. (§ XXV.)

And to all these facts stated in the first section of the second part of my before-mentioned memoir, and which contradict he chemical theory of electromotors, what arguments does M. de la Rive oppose? Perhaps Signor Fusinieri believes hat the silence of the learned Genevan is sufficient confutaion, or that he may have replied sufficiently in saying that he electricity developed by the chemical action *is not always in proportion to the action itself.* I should sooner have expected that Signor Fusinieri might have seen in this proposition a proof of the persuasion which M. de la Rive begins to entertain that his theory is not sufficient to explain the phænomena of the electromotors, if my experiments are true; though, if Dr. Fusinieri would do them the honour to repeat them, I doubt not that he would quickly be persuaded that that modification is not sufficient for the theory; but it would be necessary to add, that sometimes the electrization is in an inverse ratio to the force of the chemical action, that is, to the energy of the cause from which it is pretended to be derived. But we are here at a point of my memoir which M. de la Rive discusses in his *Recherches*, p. 125.

XI. In describing the experiment of which I have now spoken, M. de la Rive remarked that these plates of gold and pure platina, immersed in the pure nitric acid, produce no current whatever: and I said I had obtained the same phænomena with the nitric acid as with the acid itself mixed with a little hydrochloric acid, except that the galvanometric deviations were somewhat less. Now, however, M. de la Rive repeats the same thing; that is, the not having obtained a similar current in making use of the pure nitric acid; and Signor Fusinieri rather inclines to believe in the result of De la Rive, having himself made the experiment with another scientific man; and he thinks the observation just, which the same Genevan physicist then adds, namely, that as I experimented in Venice, it was a matter of difficulty that the nitric acid should be entirely free from hydrochloric. And here it vexes me that I must point out to Signor Fusinieri that this difficulty also presented itself to me, as is seen in the same page in which I speak of this experiment; and not only did I see the objection, but also removed it, by proving that by leaving the nitric acid exposed for a long time to the Venetian air, instead of acquiring to a much greater degree the aptitude to cause that current, it went on losing it; that is, on the contrary, it went on losing the power of altering to positive the relative electromotive faculty of the platina and the gold so far as to acquire the opposite property. (§ XXVI.)

But there is yet more. Is it then true that there is that *difference in the facts*, which Dr. Fusinieri remarks in this proposition, namely, that De la Rive may not have seen in immersing the gold and the platina in the nitric acid, the small electric currents seen by me? And do we not read at the eleventh page of his *Recherches*, that in the act of immersing that pair in the nitric acid there are seen slight galvanometric deviations? "Du moins (these are his own words) le léger courant que l' on aperçoit au premier moment de l'immersion, n'est pas plus fort que celui qui a lieu quand on se sert de

deux lames homogènes de platine, et il est dû aux impuretés dont il est impossible de préserver complètement les surfaces métalliques; aussi disparait-il très-promptement, et quoique les deux métaux restent dans le liquide, on n'en voit bientôt plus aucune trace."

Then also the learned Genevan sees the voltaic current on immersing in the pure nitric acid the gold and the platina fixed to the ends of the galvanometric wire: he observes that the current itself is not stronger than that produced by the two homogeneous plates of platina; and he attributes it to the impurities, from which he says it is impossible completely to preserve metallic surfaces. And in proof that this and no other is the cause of that current, M. de la Rive adduces the fact that it disappears immediately, and that however long the two metals may remain in the liquid, in a few moments there is no more trace of it.

The difference, then, is not in the fact, but in the manner of studying and interpreting it. M. de la Rive not being able to persuade himself that there can be an electric current without chemical action, and insisting that neither the gold nor the platina can be injured by the nitric acid, supposes it impossible entirely to purify the surfaces of the two metals. Instead of this, I imagine that the current is produced because the platina touches the gold metallically, by means of the galvanometric wire, and is immersed with it in a liquid. If the current is weak, it is because the metals are sufficiently near in the voltaic scale; if it vanishes, or rather, if it becomes sensibly weaker in a short time, it is because the current itself increases the electromotive faculty of the gold, and diminishes that of the platina, so that in a short time the two metals become almost homogeneous in the voltaic sense of that term.

M. de la Rive attributes the vanishing of the current to the disappearance of the impurities from the surfaces of the two metals. But I shall here ask if it is desired that the nitric acid may destroy those impurities by means of the little current which is developed, or that it may be destroyed independently of it. In the first case, I shall inquire how it happens that if the gold is immersed in the acid a moment before the platina, the galvanometric deviation is extremely weak, and sometimes even nothing. In the second case, the current which is observed when the two plates are allowed to remain immersed in the acid for some time before putting them in communication with each other by means of the wire of the galvanometer, will have to be explained.

There is still a remark to make respecting the disappearance of the current, which M. de la Rive declares to happen a few moments after the circuit is complete; so that that current disappear, two conditions are necessary. first is, that the experiments may be made with a galvan not very sensitive, that is, such as may not indicate the c itself, except by one or two degrees of deviation. The is, that the two metals may be immersed in the acid they are still in communication with each other, exac M. de la Rive conducts the experiment. But if use is of a galvanometer rather sensitive, such, for example, a show the current by a deviation of ten or more degree very true that in a few moments the force of the curr much weakened; but it does not fail to be visible with degree of deviation, even after more than a minute; s the deviation is strong at first, but proceeds always more in the times succeeding to the first immersion of the

But experimenting with the galvanometer of moderat cacy, if the plates are kept some little time in the liquid completing the circuit with the galvanometric wire, it i that the current does not then cease so quickly; an stronger if more time is allowed to pass before closi circuit itself.

The following are some experiments instituted already times here in Modena, and with the galvanometer suffi sensitive, above mentioned.

A small plate of pure gold, and one of pure platina, immersed for an instant in nitric acid, deprived enti hydrochloric acid whilst they remained in contact by of the wire of the galvanometer, produced a deviation o

Two other similar plates being left immersed for minute, and then the circuit being closed for a momen deviation was of 10°.

The experiment being repeated with two other plate the circuit being completed for a minute after they we mersed, 25°.

Being left immersed for five seconds before completi circuit, 75°.

The two small plates were kept at the distance of a meter from each other, and were immersed together liquid for a square centimeter of their surfaces.

The nitric acid increases the relative electromotive of both those metals, but much more that of the platin that of the gold, and thence may be drawn easily the ex tion of these and of various other phænomena which a served with the voltaic pair of platina and gold, when acid is made use of for a moist conductor. (See my mentioned memoir at §§ XXV. XXVI.)

XII. I must not abandon this argument without making some observations respecting a singular proposition which is read towards the end of the tenth page of M. de la Rive's work, expressed in the following terms: "Lorsque le liquide dans lequel plongent les deux élémens du couple, est le même, il n'existe pas un seul cas dans lequel on ait vu le métal le moins attaqué être positif par rapport à l'autre."

Yet in the actual state of the science it is very easy to find many cases in which it is not yet known which element of the pair may be the most acted upon, although it may be undoubted that the pair itself produces a voltaic current. Which, for example, of the following substances—silver, gold, platina, carburet of iron, and peroxide of manganese—is most acted upon by distilled water? Yet, whatever pair may be made with the said metals, there is a current when it is immersed in the said liquid. If it be shown that charcoal, well freed from hydrogen and extinguished for some time, may be more acted upon by distilled water than the silver, gold and platina, by which it may have to electrify itself, how does it happen when positively voltaically united with those metals and immersed in the same liquid? If it be shown that charcoal itself, when it is oxidated, may be less acted upon by distilled water than the noble metals, yet made into a pair with them it acquires negative electricity. If it be shown that tin may be more acted upon than copper, nickel, bismuth, cobalt, and iron, by the said liquid, yet tin becomes positive when immersed in such liquid voltaically united with any one of the said metals. Zinc united with cadmium is positive even in distilled water. Is it known which of the two is most acted upon by that fluid?

Here, then, are at least twenty cases, all observed by me separately, in which it is doubtful upon which of the two elements of the pair the liquid exercises the strongest chemical action; and it is very certain that one is electrified positively and the other negatively. And this number of doubtful cases is doubled when it is observed, that when making use of alcohol instead of distilled water, the effects are different, although the currents may in this case be weaker. The description of some experiments made with the said pairs may not be useless.

A plate of platina, and one of silver, fixed to the ends of the wire of the galvanometer, were immersed at the same time in rectified alcohol, at thirty-three degrees of the scale of Bauinè, and there was a deviation of two degrees on the part of the platina. Both the plates touched the liquid with a surface of four square centimeters.

A plate of gold with a surface of eight square centim coupled with one of platina having a surface of one s decimeter, immersed in the said liquid, produced the dev of a degree and a half, which indicated that the gol positive in contact with the platina.

And thus, with any other of the above-mentioned whatever, a current was obtained, which indicated th of the metals was positive and the other negative.

XIII. But let us suppose that all these doubtful cas not argue against the chemical theory; let us admit th metal which shows itself positive may be also the most upon; and that if this is not yet demonstrated, it will be one day. What must we say of so many other cases, in it is certain which of the two metals is the most acted and equally certain that it is not electrified positively, new theory would assert?

Copper united with iron and immersed in ammonia i trified negatively in the first moment of immersion. same copper is charged also with negative electricity united with tin and with lead, the pair being immersed same liquid; yet the copper is more acted upon by th monia than are the other two metals.

In the nitric and sulphuric acids, diluted or concen are not copper and iron more acted upon than tin and Yet copper as well as iron is electrified negatively whe immersed in the said liquid, united with tin or lead.

Sulphuric acid, diluted by two hundred parts of wate less upon cobalt than the copper, polished antimony antimony slightly oxidated, which being immersed in th acid, promotes effervescence. But the pairs, cobalt an per, cobalt and antimony, immersed in the said acid, the cobalt always positive.

Polished antimony, cobalt, bismuth, nickel, tin, and are all less acted upon by acetic acid than copper i they are all positive when, in voltaic association with c they are immersed in the said acid.

And here are twenty cases (and it is far from diffic find others) in which the two elements of the pair a mersed in the same liquid, and the metal the least acted is positive with respect to the other. And it may b that the experiments of the Genevan physicist must hav restricted within very narrow limits, if not one of these came under his notice; since, if the above facts are tr which whoever will repeat them may assure himself, it ap to me impossible longer to admit, with M. de la Rive "le sens du courant est toujours d'accord avec la t chimique, et que le métal sur lequel l'action chimique du liquide est la plus vive est toujours positif par rapport à l'autre." (From the work above cited, p. 14.)

I should believe, moreover, that one only of the above-mentioned facts would suffice to justify the proposition which may be read at the end of § XXVII. of my memoir already quoted, "*that the new theory is not sufficient to explain all the phænomena presented by simple electromotors, when the two plates of which they are formed are immersed in the same liquid.*"

XIV. Then when the two elements of the voltaic pair are immersed in different liquids, the cases are very many which contradict the new theory, and as elsewhere I have observed, they may be multiplied almost at the will of the experimenter. These were considered by M. de la Rive as apparent anomalies, and to explain them, I supposed that the two electric principles tend to reunite themselves immediately in both liquids, that such reunion is more easy in the liquid which gives a positive action, and therefore the metal there immersed is negative.

Concerning this hypothetical explanation, after having pointed out in § XXX. of my memoir three things to me incomprehensible in it, I demonstrated, with the assistance of experiment, that admitting those immediate reunions of the electric principles, very many facts are found in contradiction to the theory. And I concluded it "*a thing not easy to be admitted that nature availed herself of those immediate recompositions of the two electric principles, except in those experiments which do not in any manner square with the new theory.*" And I terminated that section with some experiments derived from two then recent observations of Signor Becquerel, from which was inferred clearly "*that in the voltaic pair is a cause which gives origin to electricity which cannot be confounded with the chemical action, and which is more powerful than it.*"

Now I would ask of Dr. Fusinieri what reply can be found in the work of M. de la Rive to these objections? Does he think it may suffice to find the same things (confuted by me) repeated in it, and without any notice whatever of the confutation? The silence of M. de la Rive would suffice certainly to reply where objections evidently erroneous were treated of; but where I bring forward experiments easy to be repeated, why had not Dr. Fusinieri, acute observer as he is, the curiosity to see them before pronouncing a judgment so disadvantageous with respect to them?

XV. Let us see, finally, what may be replied to the arguments with which I proved that the Delarivian theory was not sufficient to explain the phænomena of the comp tromotors.

The chemical theory being shown insufficient to the phænomena of the simple electromotors, the insu as to the explanation of the phænomena of the comp tromotors was naturally deduced as a corollary from I opposed besides the argument of the invariability of sion, whatever may be the liquid in which the volt are immersed, when neither their number nor their vary. And to anticipate the reply which would pe made to that argument, that is, that although the fluids may not exert equal chemical actions upon th pairs, yet the difference of the actions exerted upon elements of the pair being equal, the tension also invariable; I described an experiment made with t tromotors of eight pairs, which had equal tension, the differences of the chemical actions exercised upon elements of each pair in each of the electromotors thing but equal; for in one all the plates were imm distilled water, and in the other the plates of zinc mersed in distilled water, and those of copper in di phuric acid. (§ XXXIV.)

I undertook to examine also the truly ingenious with which M. de la Rive attempted to explain th of the piles dependent upon the number of the pairs. plain such effects, M. de la Rive supposes that the t which the two electric principles have to neutralize eac acts so, that when the poles are not united by any co it is the pile itself which serves them as a conductor, ducing the effect of one meeting the other. Thus the the tension will depend upon the greater or less facili the voltaic apparatus shall present to the transmission two fluids. And since it is known that the greater the of the plates to pass through, the more difficult is th mission, so the electricity accumulated at one pole affect the condenser, except in so far as the pile itself composed of such a number of pairs that the resista posed by the apparatus to the reunion of the two flu be sufficiently great for the tension to become sensibl

And examining in the abstract this tendency, supp M. de la Rive, in the two electric fluids, to run to n each other, even by the way which the electromot offers to them, I permitted myself to suggest the f dilemma. Either this tendency exists also in the sim tromotors, that is formed by one pair alone, or it exi in the complex electromotors. If it be admitted th tendency exists even in one pair alone, it is not intelligible how it happens that the two electricities do not avoid passing the damp conductor, the metallic way, which is so much more *conductive*, being open to them. If, on the other hand, it be admitted that that tendency exists solely in the complex electromotor, then it is not intelligible how such a property arises in it, if the elements of which it is formed (which are also so many electromotors) are all without it. And to him who might have objected to such an argument, that the greater part of the two electric principles takes the metallic way to go to neutralize itself, and that adding pairs to pairs the tension increases, because the quantity which can pass through the electromotor is less;—to him who, I say, might have thus objected, I recalled the fact that the alternations of moist and metallic conductors diminish certainly the quantity of electricity which in a given time passes through the voltaic apparatus, but do not alter the tension.

I asked, in the second place, why the least indication may not be had of such currents in a direction contrary to the usual one. And finally, I found it difficult to be admitted that the two electric principles tend to retrocede in the pile in order to neutralize each other, whilst the virtue of the pile consists, on the contrary, in the tendency to *accumulate* one of such principles at the positive pole, and the other at the negative. (§ XXXV.)

The examination instituted by me respecting this manner of explaining the effects of the pile was not limited to the preceding abstract considerations, but was pursued also with experiments. If the tension of a pile is, as M. de la Rive says, relative to the greater or less difficulty which the pile itself opposes to the passage of the electricity, it will be sufficient, I said, to render the passage of the electric through the electromotor more difficult, to see the tension at the poles augmented; and therefore, after having watched the tension of a *couronne de tasses*, I disposed the pairs in other cups, so much more ample than the first, that the liquid stratum (which was, as in the first apparatus, rain water) interposed between each pair was about six times greater; and although here the resistance might be much greater which the electricity must encounter in moving itself in the apparatus, yet the tension was found to be not at all increased.

In the work of M. de la Rive no reply is given either to the experiments or to the reasonings here above-mentioned, with which I intended to prove the insufficiency of his theory in explaining the phænomena of the complex electromotors.

But behold us finally at some of my experiments, which the learned Genevan undertakes to examine.

Such experiments are only variations of the last one above recorded. In order to render the transmission of the electric fluid through the electromotor more difficult, I put between each two pairs several inactive arcs, that is, formed of one sole copper wire, and the tension was equal to that which was observed when the electromotor was arranged as usual. Sometimes in such experiments the transmissiveness of the apparatus becomes so much enfeebled, that there is no longer an indication of a current even *to* the galvanometer, as, for example, when I introduced between the three active pairs three hundred and ten inactive ones. Yet, notwithstanding, I found no difference in the tension of those three pairs from when they were arranged as usual, and the current excited by them did not pass through the water of three hundred other glasses, and the metallic arcs which put them in communication with each other. And since M. de la Rive said that the decomposing power of the electromotor apparatus must vary according to the relation which exists between the conductivity of the liquid which connects the two poles and the apparatus itself, I therefore brought forward experiments which showed that not to be true, because every time that I rendered the passage of the current sensibly more difficult, by adding now twenty, now forty, now a hundred inactive pairs, the decomposition taking place at the *poles* became always slackened. (§ XXXVI. and XXXVII.) And see how M. de la Rive discourses about these experiments at pages 151 and 152 of the work quoted.

" La principale objection du savant italien a été dirigée contre le principe que j'avais admis, savoir que les deux fluides électriques accumulés aux deux pôles de la pile peuvent se neutraliser directement par l'intermédiaire de la pile elle-même qui lui sert de conducteur. D'après ce principe, dit il, si l'on diminue la conductibilité de la pile on doit augmenter la tension de ses deux pôles; or, on ne produit pas ce dernier effet en interposant dans le liquide qui sépare les couples, un plus ou moins grand nombre de diaphragmes de cuivre, interposition, qui cependant doit diminuer la conductibilité de la pile. Il y a plus; cette interposition non seulement n'augmente pas la tension, mais elle diminue même le pouvoir chimique de la pile dans la décomposition de l'eau; cependant lorsque les pôles sont réunis par un conducteur imparfait, s'il est vrai que la proportion d'électricité qui passe par ce conducteur et par la pile dépende de leur conductibilité rélative, il doit en pas-

ser d'autant plus à travers le conducteur que la pile conduit plus mal. A l'appuis de ces observations M. Marianini cite plusieurs experiences."

"Je suis tout-à-fait d'accord avec M. Marianini sur les consequences qu'il tire du principe que j'ai admis, mais je diffère sur l'application qu'il en fait; je reconnais avec lui que tout ce qui diminue la conductibilité de la pile, doit augmenter la tension de ses pôles pourvu qu'on n'altère en rien la quantité d'électricitè développée par chacun de ses couples; j'ai montré par des faits que c'était une condition indispensable. Mais la manière dont M. Marianini diminue la conductibilité de la pile, rentre-t-elle bien dans ce cas? Non, car le zinc et le cuivre entre lesquels il place les diaphragmes de cuivre ou de tout autre métal, n'étant plus dans les mêmes conditions que le zinc et le cuivre des autres couples, il est facile de comprendre que l'électricité positive du premier et la négative amenée par le second, se réunissent en beaucoup moins grande proportion à cause de la diminution de conductibilité qui résulte pour le liquide qui les sépare, de l'interposition des diaphragmes. Dès lors d'après la théorie que nous avons donnée, l'électricité libre de tous les autres couples diminue dans le même rapport que celle du couple que nous venons de considérer, de sorte que si d'une part les deux principes électriques accumulés aux deux pôles ont moins de facilité à se réunir d'autre part ils sont développées en moindre quantité. On conçoit que lorsqu'il s'agit de la tension, cas dans lequel l'élément du temps n'entre pour rien, puisque le condensateur reste en contact avec le pôle aussi long temps qu'on le veut, les deux effets que nous venons de signaler puissent se compenser: mais il ne peut en être de même pour les décompositions opérées par le courant et en général pour tous les effets dynamiques, car il n'y a pas de temps suffisant pour l'accumulation des deux principes électriques, et tout ce qui diminue la quantité de l'electricité libre dégagée en un temps donné par chaque couple, et par conséquent aux deux pôles, doit diminuer l'intensité des effets produits par la circulation de cette électricité."

M. de la Rive agrees, therefore, with me, that if the principle admitted by him be true, all that which diminishes the conductivity of the pile must augment the tension of the poles; but he suggests further, that what diminishes the conductivity of the pile may in no degree alter the quantity of electricity developed by each one of its pairs.

We will observe here directly, that before these *instructions* of M. de la Rive, the quantity of electricity developed by each couple was never said, that I know, to influence the tension.

Every one knows that an electromotor of ten pairs has
the same tension, whether the plates are large or small, o
some small and others large. And it is known likewis
the quantity of electricity developed varies much with t
riation in the dimensions of *all* the pairs, or even only of
the galvanometers giving the most certain proofs of it.

But granted even that this might be, is it then true th
method by which I diminished the conductivity of the pil
not concern this case, that is, alter the quantity of elec
developed by each pair? M. de la Rive seems to sa
those pairs between the copper and the zinc, of wh
placed metallic diaphragms, are not in the same conditio
the other pairs. But the truth is, that it is not betwe
copper and the zinc of one pair that I placed the diaphr
but between the zinc of one pair and the copper of ano
put some inactive pairs, that is, some arcs, formed of c
wire, which were immersed in cups containing the liqui
ductor, as those which served for the active pairs. An
interpolation I did not make merely between some pair
in some experiments those arcs were interpolated be
every one of them; so that all came to be in the same
dition. If ever M. de la Rive should say that those pa
interpolated are not in the same condition as those of an
electromotor, where there may not be that interpolation
active pairs, and that, therefore, each pair of the interp
electromotor developes less electricity than each one
other, I would ask, *whence* the electricity is develope
cording to the new theory? Is it not perhaps by the c
cal action of the liquid upon the metal? and ought it
be proportional to the difference of the actions exerted
the zinc and upon the copper? Now, how do those in
arcs interrupt the action of the liquid upon the elements
pairs; and then how do they alter the quantity of elec
developed?

And if yet it should be pretended that the presence
inactive copper wire in the glass, where the zinc or the c
of a couple was immersed, might influence the action ex
by the liquid upon the zinc, or upon the copper of the
pairs; what influence will the copper wires placed in the
successive cups exert? The experiment shows that the
sion is always the same, when placing one only of those
tive arcs between the pairs of one pile, as when placing
eight, ten, or any other number whatever between them

And these observations, made relative to tension, apply
with the proper difference, to the decomposing power,
always diminishes, when, the other things being placed

larly, the conductivity of the pile (whatever may be the method) is diminished.

But wishing also to concede that those diaphragms or interpolated metallic arcs impart to the pairs the condition that M. de la Rive desires, how shall we explain the non-alteration of the tension, when the electromotor is rendered less conductive by disposing the pairs in much larger cups, by which the liquid strata, through which the electric fluid must pass, are more extended? This is the first and principal experiment which I oppose to M. de la Rive, regarding his manner of explaining the effects of tension and of decomposition, and of which he does not make mention.

I said that this was the *principal* of such experiments, because the insertion of liquid strata of great extent between the pairs occurring to me as difficult, I have applied to it again with series of metallic homogeneous arcs, knowing the difficulty which the electric current encounters in passing through alternate liquid and metallic conductors. Now, I have repeated that principal experiment also on a large scale, that is, with two electromotors; the one with small troughs, in which the plates were so disposed, that between the copper of one pair and the zinc of the next there was only a stratum of water of the size of one millimeter; and the other with great receivers, so that the stratum of water, which divided one pair from the other, was half a meter. The number of the pairs being equal in both apparatus, the tension was also equal in both.

I have tried also to oblige the current to pass through many cups of water connected by means of little siphons filled with the same liquid; but I did not succeed by this in obtaining increased tension.

Finally, in order not to vary in any way the conditions of the apparatus with respect to the development of the electricity according to the electro-chemical theory, I prepared a pile of three pairs, in one of which the plate of zinc communicated with that of copper by a small metallic wire a thousand meters in length, and in each of the other two the wire which connected the copper and the zinc was five hundred meters in length. The apparatus was arranged so that that communication could be easily taken away, and one substituted for it, not longer than four or five millimeters. Now, when the metallic communications between the zinc and the copper were formed of short wires, the deviation which the pile produced in the galvanometer was twenty degrees; and when the long were substituted for the short wires, it was only fourteen; yet the tension was always the same in both cases.

For if M. de la Rive, in all these experiments, found the conditions always varied in order that the chemical action upon each couple might excite the same electricity, I should desire very much that he would point out at least one experiment in which the couples remaining equal both in number and quality, but the conductivity of the pile varying, the tension might be seen likewise to vary. This experiment I expected to find described in the above-mentioned work, after his having said that he agreed with me, that all which diminishes the conductivity of the pile, must, according to his theory, augment the tension of the poles; I expected also, that he would only disagree with me in regard to the mode of diminishing the said conductivity. I find instead, that M. de la Rive says, that in my method of weakening the conductivity of the pile, if on one part the two electric principles met together at the poles have less facility of reuniting themselves, they are developed in less quantity. But truly he should agree that they might be developed not in less quantity, although with less tension, that the electric principles might show themselves *unvaried* in the tension at the poles of the pile, notwithstanding that its conductivity might vary.

[To be continued.]

LXXX. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. Francis *and* H. Croft.

[Continued from p. 442.]

Action of Chlorine on Oxalic Æther.

MALAGUTI has published a series of interesting experiments on the action of chlorine on oxalic æther. He has succeeded in preparing a chloroxalic æther, in which the entire quantity of the hydrogen of oxalic æther is replaced by an equivalent quantity of chlorine, $C^{12} Cl^{10} O^{4}$ *. The resulting chloroxalic æther affords with liquid ammonia oxamid, with gaseous ammonia chloroxamethane, a body comparable in every respect to oxamethane. The chloroxamethane is converted by the action of liquid ammonia into chloroxalovinate of ammonia, from which may be obtained the chloroxalovinic acid, which differs from oxalovinic acid solely from its containing chlorine in the place of hydrogen. This acid may be obtained in an anhydrous state by the action of alcohol on chloroxalic æther, which may again be made to give birth to a chlorated acid, especially by the oxidizing action of alkalies. One of the most remarkable results of these experiments is the preservation of the chemical properties of the oxalic æther after it has exchanged ten atoms of hydrogen for ten of chlorine. In examining the following table, in which are stated comparatively all the transformations of oxalic æther and of chloroxalic æther, we shall arrive at the conviction that the one series is merely a repetition of the other.

---

* [The system of formulæ in this abstract is different from that in the former abstracts; the small French value for carbon having been assumed.]

Oxalic Æther, and Compounds derived from it.	Chloroxalic Æther, and Compounds derived from it.
$C^{12} H^{10} O^4$ oxalic æther	$C^{12} Cl^{10} O^4$ chloroxalic æther
$C^{12} H^{10} O^4$, $C^4 O^3$ oxalovinic acid	$C^{12} \overline{Cl}^{10} O^4$, $C^4 O^3$ chloroxalovinic acid
$C^{12} H^{10} O^4$, $C^4 O^3$, $H^2 O$ hydrate of oxalovinic acid	$C^{12} Cl^{10} O^4$, $C^4 O^3$, $H^2 O$ hydrate of chloroxalov. acid
$C^{12} H^{10} O^4$, $C^4 O^3$, $B O$ oxalovinates	$C^{12} Cl^{10} O^4$, $C^4 O^3$, $B O$, chloroxalovinates
$C^{12} H^{10} O^4$, $C^4 O^2 N^2 H^4$ oxamethane	$C^{12} Cl^{10} O^4$, $C^4 O^2 N^2 H^4$ chloroxamethane
$C^4 O^2 N^2 H^4$ oxamid	$C^4 O^2 N^2 H^4$ oxamid.

Not long ago, if, after the inspection of this table, chloroxalic æther had been said to be, chemically speaking, nothing further than oxalic æther containing chlorine instead of hydrogen, no objection would have been made. But this is perhaps no longer the case at present; the mode of expression will nevertheless be the most simple and most pure as given by experiment.

Before concluding this notice, we may direct attention to a paper in the *Annales de Chimie et de Physique*, by De la Provostaye, on the Isomorphism between Oxamethane and Chloroxamethane, of which a translation has already been presented to the readers of this Journal (p. 372); his conclusions as to their isomorphism must, however, be confirmed before they can be adopted. Prof. H. Rose, in a paper recently published on the Combinations of the Volatile Chlorides with Ammonia, (Taylor's Scientific Memoirs, vol. ii. p. 32,) which contains some valuable remarks on Dumas's theory of substitutions, hints at the measurements of the crystals not being satisfactory.

State of Urea in Urine.

Very different views have been entertained respecting the state of urea in urine. Persoz considers it to be merely a product of sundry reactions, especially of heat, on urine, which he conceives to be proved from the syrupy mass obtained by freezing urine at −13° to −18° centigrade, when treated with nitric acid, only producing crystals of the nitrate of urea, after it has been for some time heated. Morin admits in urine solely a chlorine compound of his uril. Cap and Henry, on the contrary, maintain that urea is present in the urine of man as the lactate of urea. The experiments which M. Lecanu has re-

cently published speak in favour of none of these opinio
will here briefly give the conclusions which he has dra
his experiments, viz. that the extraction of urea, by the
methods in which heat is employed, is not at all, as
supposes, due to the production of this substance un
influence of heat; that Morin's protochloride of uri
thing more than an intimate mixture, or rather a com
of urea with the chloride of ammonium; that the met
which Cap and Henry supposed they had extracted la
urea give merely mixtures in which urea and lactic ac
in a free state; that, lastly, urea can be extracted fro
by means of alcohol nearly pure, without the employ
acids or alkalies, of which it might be believed that
stroy the natural state of combination of the urea.

Errata in Messrs. Francis and Croft's *Notices of the Results of th of Continental Chemists.*

To Richard Taylor, Esq.

Dear Sir,

At your request we furnish you with a list of the errata contai
abstracts. Most of them are such that every chemist will have
to detect and correct them immediately, for instance, Aq O instea
where a silver salt is spoken of; and Ag instead of Aq, where noth
silver occurs. We are inclined to believe that most of your re
have had the kindness to regard them as typographical errors, an
have considered their occurrence a fair occasion to make a displa
superior knowledge, as a certain Mr. W.A. M. has done, who infor
$\dot{K}$ I should be written without a dot over the K. We shall end
obviate the occurrence of such errata in future: at the same time i
recollected that the great distance prevents us from having pro
mitted.

Yours truly,

Berlin, May 1841.

W. F. and

Page	202,	10 lines from bottom,	*for*	Melates,	*read*	Oxa
„	—	7 „ „	„	H. Roſe,	„	J. Li
„	204,	5 „ „	„	$\dddot{Fe}^2$,	„	$\dddot{Fe}$
„	—	7 lines from top,	„	$C^2 H^8$,	„	$\bar{C}^2$ H
„	205,	5 „	„	valeric,	„	valer
„	206,	16 „	„	$\dot{K} I^2$,	„	K I
„	—	18 „	„	E F,	„	Æ I
„	—	— „	„	R F,	„	K I
„	—	15 lines from bottom,	„	mæthyl,	„	meth
„	207,	12 lines from top,	„	rose,	„	rises
„	209,	— „	„	oxide,	„	sesqu
„	—	6 lines from bottom,	„	mere,	„	pure
„	210,	15 lines from top,	„	$C^4 H$,	„	$C^4 H$
„	279,	8 lines from bottom,	„	$H N^6 O^6$,	„	H^{44} N
„	281,	21 lines from top,	„	$C^{24} H^{108}$,	„	C^{64} H
„	282,	last line,	„	$C^{27} H^{20}$,	„	C^{22} H
„	284,	13 lines from bottom,	„	improved	„	unpro
„	285,	lines 16 and 18 from bottom,	„	Ag,	„	Aq
„	289,	line 4 from top,	„	Aq O,	„	Ag O
„	291,	lines 3 and 4 from top	„	$\mathring{A}q$,	„	$\dot{A}g$

LXXXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 311*.]

Jan. 28, 1841. THE reading of a paper entitled, "On the action of certain Inorganic Compounds when introduced directly into the Blood." By James Blake, Esq., M.R.C.S. Communicated by P. M. Roget, M.D., Sec. R.S., was resumed and concluded.

The present paper is a continuation of a memoir read at the Académie des Sciences of Paris, in 1839, and entitled, "Effets de diverses substances salines, injectées dans le systême circulatoire †.

After some preliminary remarks on the mode in which the experiments were conducted, and on the assistance derived from the *hæmadynamometer* of Poiseuille (or instrument for measuring the pressure of the blood circulating in the vessels), the author gives a list of the various saline substances of which he noted the effects when they were severally injected either into the venous or the arterial systems, arranged according to the nature of those effects. He finds, in general, that all the salts having the same base exert similar actions when introduced directly into the blood. He carefully inquires into the phænomena apparently arising from the direct contact of each of the substances above enumerated with the animal tissues; and more particularly into the effects produced on the heart, on the muscular and the nervous tissues, and on the pulmonary and systemic capillaries.

The first series of experiments related are those on the action of the salts of magnesia: these are found, when introduced in any quantities into the blood, to arrest altogether the action of the heart; but a still more remarkable effect which results, is the complete prostration of muscular power. The salts of zinc have a similar operation under the same circumstances, but produce the same effects in smaller quantities. The action of the salts of copper, of lime, of strontia, of baryta, and of lead, are considered successively in the order in which they are more closely related by their physiological actions. The author particularly notices the peculiar action which the salts of the three last-named substances exercise on the muscular tissues, occasioning contractions in them during many minutes after death produced by their introduction into the blood. These muscular movements were, in some cases, observed forty-five minutes after the cessation of the heart's action. Experiments with the salts of silver and of soda are then detailed; substances, which exhibit a remarkable similarity in their actions on the pulmonary tissue, on the heart, and on the systemic capillaries: for while, in the case of all the other salts already mentioned, death seems to be produced by the destruction of the irritability of the heart, the fatal result with the salts of silver and of soda is the consequence of their action on the tissue of the lungs. The physiological actions of the salts of

---

[* Two notices of communications made to the Royal Society, accidentally omitted from their proper place, will be found with the Miscellaneous Articles in the present Number.]

† Published in the "Archives Générales de Médecine; Nov. 1839."

ammonia and of potass were found by the author not to correspond with any of the preceding. Although agreeing perfectly with one another in their action upon the heart and systemic capillaries, they differ extremely in their effects on the nervous tissue; ammonia being particularly distinguished from all inorganic compounds in this respect, and being very analogous to poisons derived from organic products, which it also resembles in its chemical properties.

The general conclusion which the author is led to draw from these researches is, that there exists a close relation between the chemical properties of the substances experimented upon, and their physiological effects; his experiments tending to prove, that, when introduced into the blood, substances which are isomorphous exert similar actions on the living tissues. He notices, however, two exceptions to this law; namely, the similarity of the actions exerted on the pulmonary tissue by the salts of lead with those of silver, although these salts are not isomorphous; and also the action on the nervous tissue of the salts of ammonia being different from that of the salts of potass. But he remarks that the oxide of lead bears a close analogy to the oxide of silver in its relation to organic compounds. The general fact previously announced by the author in his memoir read to the Academy of Sciences at Paris, namely, that salts with the same base have analogous actions, may be considered as a corollary of the above law.

February 4.—A paper was read, entitled, "On some Electro-Nitrogurets." By Wm. Robert Grove, Esq., M.A., F.R.S.

The author states that he has made many attempts to render permanent the ammoniacal amalgam, and that he has succeeded in freezing it by means of solid carbonic acid, during which solidification, and also while in its solid state, it underwent no chemical change. He subsequently attempted to procure a permanent compound by electrolyzing a solution of hydrochlorate of ammonia with an extremely fusible alloy at the cathode; but this attempt was unsuccessful. It then occurred to him, that by using an oxidable metal at the anode, which could be revived in conjunction with nascent hydrogen and nitrogen at the cathode, one or both of these elements might be combined with the solid metal, and so form permanent compounds.

The experiment made in this manner with the metals zinc, cadmium, and copper, was perfectly successful. A spongy mass collected at the cathode, which floated upon the liquid, and which, when washed and dried, was analysed by heating in a tube retort; five grains of the zinc compound gave 0·73 of a cubic inch of permanent gas, which on examination proved to be nitrogen with one-fourth hydrogen. The same quantity of the cadmium compound gave 0·207 cubic inch of nitrogen with no admixture of hydrogen. A like weight of the copper compound gave 0·107 of nitrogen. No ammonia was evolved from either; and the author is inclined to think that the hydrogen yielded by the zinc compound resulted from the reaction of the metal upon combined water. The specific gravity of specimens of these substances which the author tried were respectively 4·6, 4·8, and 5·9. A mixed solution of chloride of gold and hydrochlorate of ammonia, electrolyzed with platinum electrodes, gave a black powder of the specific gravity 10·3; five grains of which, being heated, gave only 0·05 cubic inch of gas. The author proceeds to observe, that the similarity in appearance and mode of formation of these compounds and of the mercurio-ammoniacal amalgam, is strong evidence of identity of constitution, and that the non-permanence of the latter substance is due to the mobility of the mercury; for if we place the compounds in similar circumstances, that is, solidify the mercurial one, or liquefy those of the other metals, the phænomena are perfectly analogous. The experiments also bear immediately upon those of Thénard, Savart, and others, where ammonia, passed over heated metals, was found to be decomposed more completely by the oxidable than by the inoxidable metals, and to alter their physical characters without materially increasing their weight*. On examining papers connected with this subject, the author found that Mr. Daniell had cursorily noticed a deposit somewhat analogous to those here treated of, which was formed upon the negative plate of his constant battery when this was charged on the zinc side with hydrochlorate of ammonia, and the nature of which that gentleman observed was worthy of further examination, but had not had time to investigate.

February 11.—A paper was read, entitled, "Contributions to Terrestrial Magnetism, No. 2." By Major Edward Sabine, R.A., V.P.R.S.†

This paper is the second of a series, in which the author purposes to communicate to the Royal Society the results of magnetic observations in different parts of the globe, having for their object to supply the requisite data for deducing the numerical elements corresponding to the present epoch of the general theory of terrestrial magnetism. It consists of two sections; the first comprises the observations of Captain Belcher, R.N., and the officers of H. M. S. Sulphur, at twenty-nine stations on the west coast of America, and the adjacent islands, between the latitudes of 60° 21′ N. and 18° 05′ S. The second contains a new determination, by the same officers, of the magnetic elements at Otaheite, made in consequence of the discrepancies in the results obtained by previous observers, and of a note in M. Gauss's Allgemeine Theorie, in which Otaheite is spoken of as a highly important station for the future improvement of the calculations of the theory. Abstracts are given of the original observations which are deposited in the Hydrographic Office of the Admiralty, as well as a full detail of the processes of reduction by which their results have been computed. The values of the horizontal and total intensities are expressed in terms by which the results of observation are immediately comparable with the maps of MM. Gauss and Weber in the "Atlas des Erdmagnetismus."

By an investigation into the "probable error" of a single inde-

---

[* Notices of the experiments of Savart and Despretz on this subject were given in Phil. Mag., Second Series (Phil. Mag. and Annals, N. S.), vol. iv. p. 155; vol. vi. p. 147.]

[† A notice of No. 1. appeared in Phil. Mag., Third or present Series (L. and E. Phil. Mag.), vol. xvii. p. 144.]

pendent determination of the magnetic intensity with Ha apparatus, derived from the data furnished by Captain Belch servations, the author shows the extreme improbability t differences in the results obtained at Otaheite by Messrs. FitzRoy and Belcher, should be occasioned by instrumenta servational error. They are also far greater than can, with gree of probability, be ascribed to periodical or accidental va in the magnetic force from its mean value. The only know adequate to their explanation is what may with propriety be *Station error*; that is, local disturbing influences, in an isla posed chiefly of volcanic rocks, and where the spot of obs selected by the different observers may not have been prec same.

By a reference to the magnetic survey of the British Isla occurrence of station error is shown to be frequent in cou far less decided igneous character than Otaheite; and that i ence may always be apprehended where rocks of that nature a to, or rise through, the superficial soil. The absolute determ of fixed observatories are as liable to station error as those of t netic traveller, since no continuance or repetition of the obse can lead to an elimination of the error; it consequently p practical difficulty to the proposed determination of the of the theory from exact observation at only a few selected on the globe. The remedy is to be found in the combi fixed observatories and magnetic surveys: the observation survey, being made in concert with, and based on those of t observatory, will be furnished thereby with corrections fo cular, periodical, and accidental variations of the elements, consequently determine *mean* values: and a proper combi the mean values thus determined, over a space sufficiently e to neutralize *district* anomalies, as well as those of a more local character, will furnish, in their turn, a correction for tion error, if any, of the fixed observatory.

A paper was also read, entitled, "On the Calculation of tions, and the Figure of the Earth." By C. J. Hargrea of University College. Communicated by John T. Grave F.R.S., Professor of Jurisprudence, University College, Lo

The principal object of the calculations contained in th is to investigate the figure which a fluid, consisting of varying in density according to any given law, would assum every particle is acted upon by the attraction of every othe a centrifugal force arising from rotatory motion. That been the original condition of the earth has been assum foundation of most of the mathematical calculations connec this inquiry; although the hypothesis itself may admit o The principal difficulty of this problem consists in the com of the attraction of a body of any given figure, and com strata varying in their densities according to any given law. ving it, the author follows the steps of Laplace as far as t where the equation, known by his name, first appears. however, since been discovered by Mr. Ivory, that the th

Laplace is true only of spheroids of a particular kind, and consequently it is to this kind that Laplace's solution of the problem is restricted. The method given in the present paper is not confined in its operation to any particular class of spheroids; the coefficients of the series into which the required function is developed being determined absolutely, and without reference to the form of the spheroid to which they are to be applied. The principal change consists in the different manner of treating the partial differential equation; and its integration, effected by the author, renders the analysis more direct, the operations more simple, and the theory complete.

February 18.—A paper was in part read, entitled, "Memoir on a portion of the Lower Jaw of an Iguanodon, and other Saurian Remains discovered in the strata of Tilgate Forest, in Sussex." By Gideon Algernon Mantell, Esq., LL.D., F.R.S.

February 25.—The reading of a paper, entitled, "Memoir on a portion of the Lower Jaw of an Iguanodon, and other Saurian Remains discovered in the strata of Tilgate Forest, in Sussex." By Gideon Algernon Mantell, Esq., LL.D., F.R.S., was resumed and concluded.

When the author communicated to the Royal Society, in the year 1825*, a notice on the teeth of an unknown herbivorous reptile, found in the limestone of Tilgate Forest, in Sussex, he was in hopes of discovering the jaws, with the teeth attached to it, of the same fossil animal, which might either confirm or modify the inferences he had been led to deduce from an examination of the detached teeth. He was, however, disappointed in the object of his search until lately, when he has been fortunate enough to discover a portion of the lower jaw of a young individual, in which the fangs of many teeth, and the germs of several of the supplementary teeth, are preserved. The present paper is occupied with a minute and circumstantial description of these specimens, and an elaborate inquiry into the osteological characters and relations presented by the extinct animals to which they belonged, as compared with existing species of Saurian reptiles; the whole being illustrated by numerous drawings. The comparison here instituted furnishes apparently conclusive proof that the fossil thus discovered is a portion of the lower jaw of a reptile of the Lacertine family, belonging to a genus nearly allied to the Iguana. From the peculiar structure and condition of the teeth it appears evident that the *Iguanodon* was herbivorous; and from the form of the bones of the extremities it may be inferred that it was enabled, by its long, slender, prehensile fore-feet, armed with hooked claws, and supported by its enormous hinder limbs, to pull down and feed on the foliage and trunks of the arborescent ferns, constituting the flora of that country, of which this colossal reptile appears to have been the principal inhabitant.

Some particulars are added respecting various other fossil bones found in Tilgate Forest, and in particular those of the *Hylæosaurus*, or Wealden Lizard (of which genus the author discovered the remains of three individuals), and of several other reptiles, as the

---

[* See Phil. Mag., Second Series, vol. ii. p. 444; v. p. 153; vii. p. 54: also Third Series, vol. v. p. 77; and present Number, p. 568.]

Megalosaurus, *Plesiosaurus*, and several species of *Steneosaurus*, *Pterodactylus*, and Chelonia, as also one or more species of a bird allied to the Heron. All these specimens are now deposited in the British Museum.

A paper was also read, entitled, "On a Theorem of Fermat." By Sir John William Lubbock, Bart., V.P., and Treas. R.S.

March 4.—A paper was read, entitled, "Miscellaneous Observations on the Torpedo." By John Davy, M.D., F.R.S.

The experiments described in this paper were made on a single fish, of middle size, recently taken out of the water. Portions of the electrical organs, cut transversely in thin slices, exhibited under the microscope many elliptical particles, apparently blood-corpuscles, the long diameter of which was about 1-800th, and the short about 1-1000th of an inch, and a few filaments, apparently nervous, irregularly scattered; some of them tortuous, and all about the 2000th of an inch in diameter. The latter bore no resemblance to muscular fibres. The blood contained some globular particles, having a diameter of the 4000th of an inch, mixed with the elliptical. The mucus for lubricating the surface was found to contain globules apparently homogeneous in substance, but of irregular outline, and in size varying from the 2000th to the 270th of an inch.

A paper was also read, entitled, "On a remarkable property of the Diamond." By Sir David Brewster, K.H., D.C.L., F.R.S.L., V.P.R.S. Ed.

On re-examining the phænomena of parallel bands of light and shade exhibited by reflexion at the plane surface of a diamond, which the author had noticed some years ago, he concludes that they result from the reflexions of the edges of veins or laminæ, of which the visible terminations are inclined at different angles, not exceeding two or three seconds, to the general surface. He gives an account of several analogous facts observable in other crystals, more especially those of carbonate of lime, artificially polished in surfaces inclined to the natural planes of cleavage.

March 11.—The following papers were read:

1. "On a Cycle of Eighteen Years in the Mean Annual Height of the Barometer in the Climate of London; and on a Constant Variation of the Barometrical Mean, according to the Moon's Declination." By Luke Howard, Esq., F.R.S.

For obtaining the general results communicated in the present paper, the author has followed the same method as that he had adopted in the two former papers laid before the Society on the connexion of the barometrical variation with the lunar phases and apsides. Tables are given of the barometrical averages on successive solar years, from 1815 to 1832, so constructed as to exhibit the variation of the moon's influence according to her declination; and also of these averages on successive cycles of nine solar years, classed according to the moon's place in declination, on either side of the equator. The results deduced from these comparisons are, first, that the barometrical mean in this climate is depressed by the moon's declination being to the south of the equator; and, secondly, that this depression takes place gradually, commencing with the moon's being in full north declination, and proceeding through her remaining positions to the time when she crosses the equator to resume the northern declination; at which season, the whole pressure that had been withdrawn from the atmosphere is suddenly restored. The author thinks there is evidence of a great tidal wave, or swell in the atmosphere, caused by the moon's attraction, preceding her in her approach to, and following her slowly as she recedes from these latitudes; so that were the atmosphere a calm fluid ocean of air, of uniform temperature, this tide would be manifested with as great regularity as those of the ocean of waters. But the currents uniformly kept up by the sun's varying influence effectually prevent this from taking place, and involve the problem in complexity. He finds that there is also manifested in the lunar influence a gradation of effect, which operates through a cycle of eighteen years. The mean pressure of the atmosphere during the first part of this period increases; and then, after preserving for a year its maximum amount, again decreases through the remaining years of the cycle, but exhibits, towards its minimum, some fluctuations before it again regularly increases*.

2. "On a remarkable depression of the Barometer in November 1840, agreeing very closely in its movements and results with that of December 1821." By Luke Howard, Esq., F.R.S.

The object of the author in the present paper is to show the close correspondence of the extraordinary depression of the barometer in the months of October and November of last year (1840), and of the remarkably stormy weather which prevailed at the same period, with similar phænomena occurring in December 1821, when the moon's place in declination underwent the same changes during those two periods, at an interval of nineteen years.

3. "General results of Meteorological Observations at Constantinople." By J. W. Redhouse, Esq. Communicated by John Davy, M.D., F.R.S.

4. "Term-Observations made at Prague in November and December 1840, and January 1841." By C. Kreil. Communicated by S. Hunter Christie, Esq., M.A., Sec. R.S.

March 18.—The following Magnetical and Meteorological Observations, taken in conformity with the Report drawn up by the Committee of Physics including Meteorology, for the guidance of the Antarctic Expedition, as also for the various fixed Magnetic Observatories, have been communicated by the Lords Commissioners of the Admiralty, and by the Master-General of the Ordnance, viz.—

1. "Magnetic-term Observations, taken at Kerguelen's Land, for May and June 1840." By Capt. James Clark Ross, R.N., F.R.S., Commander of the Expedition.

2. "Hourly Magnetic Observations taken at Kerguelen's Land, commencing May 25, and ending June 27, 1840." By Capt. James Clark Ross, R.N., F.R.S., &c.

3. "Meteorological Observations taken on board Her Majesty's Ship *Erebus*, for October, November, and December 1839, and from

---

[* On the subject of this paper see p. 555 and 559.]

January to August 1840." By Capt. James Clark Ros
F.R.S., &c.

4. "Meteorological Observations taken on board Her M
Ship *Terror*, for November and December 1839, and from Ja
July 1840." By Capt. T. B. M. Crozier, R.N.

5. A paper was also in part read, entitled, "On the Loca
fected by Hoar-frost, the peculiar currents of Air excited b
the Temperature during its occurrence at High and Low St
By James Farquharson, LL.D., F.R.S., Minister of the P
Alford.

March 25.—The following communications were read, vi

1. The reading of a paper entitled, "On the Localities
by Hoar-frost, the peculiar currents of Air excited by it,
Temperature during its occurrence at High and Low Statio
James Farquharson, LL.D., F.R.S., Minister of the Parish o
was resumed and concluded.

The author states that he has been accustomed, for the l
years, to make observations on the occurrence of hoar-frost,
circumstances under which it takes place, with a view of o
a correct explanation of the causes of that phænomenon. It
known, he observes, that the localities chiefly affected wi
frost are the bottoms of valleys, and land-locked places of a
whether natural or artificial. The altitude to which its effec
on the sides of the valleys is dependent on the mean temper
the day and night at the time of its occurrence: when that
rature is high, the lower places only are affected by the fr
when low, the frost extends to much higher grounds. Hoar-fros
only during a calm state of the air, and when the sky is cle
the stillness of the air in the bottoms of the valley is invari
companied by downward currents of air along all the slopi
of the valley; and it is to this fact, first noticed by the auth
he wishes more particularly to direct the attention of the So
affording a decisive proof of the correctness of the views he ent
being in accordance with the theory of Dr. Wells. He fin
after sunset, in all seasons of the year, and at all mean temp
of the air, and whether or not the ground be covered wit
whenever the sky is clear, although there may be a dead caln
bottoms of the valleys, currents of air, more or less strong and
run downwards on the inclined lands, whatever may be thei
with reference to the points of the compass. These currents
result of the sudden depression of temperature sustained by t
face of the earth in consequence of rapid radiation, by wh
stratum of air in immediate contact with that surface, becomi
cifically heavier by condensation, descends into the valley,
replaced by air which has not been thus cooled, and which th
prevents the formation of hoar-frost on the surface of these dec

2. "Term-Observations of Magnetic Observations, the Va
of the Magnetic Declination, Horizontal Intensity and Incl
at Prague; for June, July, September, and October 1840.
Prof. Kreil. Communicated by S. Hunter Christie, Esq., Se

3. "Term-Observations of the Variation of the Magnetic Declination, Horizontal Intensity and Inclination at Milan; for June 1840." By Francesco Carlini, For. Memb. R.S., Director of the Observatory.

4. "On Ground-gru, or ice formed, under peculiar circumstances, at the bottom of running water." By James Farquharson, LL.D., F.R.S., Minister of the Parish of Alford.

The author brings forward in this paper several recent observations on the formation of ice at the bottom of rivers, the conditions of which corroborate the views regarding the cause of that phænomenon, which he presented in a paper on this subject, published in the Philosophical Transactions for 1835 (p. 329) *, namely, that it occurs in consequence of the loss of heat by radiation from the bottom of the water, in a manner precisely analogous to the formation of hoar-frost on the surface of dry land, as first explained by Dr. Wells. He then answers some of the objections to that theory propounded in an article, under the title of GROUND-GRU, in the Penny Cyclopædia, and shows that those objections are founded in error, and possess no validity.

5. "Meteorological Observations made at the Magnetic Observatory at St. Helena, from February to October 1840." By Lieut. J. H. Lefroy, R.A.

6. "Meteorological Observations made at the Magnetic Observatory at Toronto, Upper Canada, from January to October 1840." By Lieut. E. J. B. Riddell, R.A.

7. "Observations on Magnetic Direction and Intensity made at the Observatory at Milan during the 24th, 26th and 27th of January 1841." By Prof. Carlini.

8. "Note on an irregularity in the Height of the Barometer, of which the argument is the Declination of the Moon." By Sir John William Lubbock, Bart., V.P. and Treas. R.S.

In the Companion to the British Almanac for 1839, the author inserted some results which were obtained with a view of ascertaining the influence of the moon on the barometer and on the dew-point. Mr. Luke Howard's researches on this subject (*ante*, p. 552) having recalled his attention to that paper, he found that some of the results he had given appeared to indicate that the moon's position in declination influences the barometer. In order to render this more manifest, he combines in the present paper all the observations he gave in the Companion to the British Almanac in three categories. These observations correspond to different angular distances of the moon from the sun, or times of transit; but as the inequality of the ocean, of which the argument is the moon's declination, is independent, or very nearly so, of the time of the moon's transit, it is probable that so also is that in the height of the barometer. In this case we may with propriety combine in the same category observations which correspond to similar declinations, although to different times of transit. The results stated by the author seem to indicate an ele-

---

[* An abstract of this paper will be found in Phil. Mag., Third Series, vol. vii. p. 137.]

vation of nearly one-tenth of an inch for 17 degrees of declination. The inequality has a contrary sign to the inequality of the same argument in the tides of the ocean.

April 1.—The following letter, addressed to the President, was read:—

"4, Trafalgar Square, London, March 25th, 1841.

"My Lord,—I have the honour of transmitting to Your Lordship for presentation to the Royal Society, an original portrait of *Sir Isaac Newton* by *Vanderbank*, a Dutch painter of some note in that age.

"This picture has now been many years in my possession, and the tenure by which I have kept it (as a collateral descendant of so illustrious a man) was too flattering not to have been a source of great personal gratification.

"But I consider such a portrait to belong of right to the scientific world in general, and more especially to that eminently distinguished Society of which Newton was once the head, and which is now so ably presided over by Your Lordship.

"I have, therefore, to request Your Lordship will do me the honour to present this original portrait of Sir Isaac Newton to the Royal Society in my humble name.

"Accident having destroyed some of the papers of my family, I am unable of myself to trace the entire history of this portrait, but I believe more than one member of the Royal Society is competent to do so, and it is well known to collectors; and a small mezzotinto engraving of it was published about forty years ago. It was painted the year before Newton died, and came into the family of the celebrated Lord Stanhope, who left it by his will to my grandfather, the late Dr. Charles Hutton, a distinguished member of the Royal Society, expressly on the well-authenticated account of that eminent mathematician having been remotely descended from Sir Isaac Newton, in the following way, as I find on a family manuscript; viz. 'that the mother of the well-known James Hutton and the mother of Dr. Charles Hutton were sisters; and the grandmother of James Hutton and the mother of Sir Isaac Newton were also sisters.'

"I have ever considered this very distant connexion with so great a man should not be an inducement to lead me into any but casual mention of the circumstance, that I might avoid the imputation of a vain boast; nor would it have been brought forward now, except to explain the cause by which this portrait came into the possession of an individual who is happy in relinquishing it to grace the Hall of Meeting of the Royal Society.

"I have the honour to subscribe myself,
"Your Lordship's very obedient humble Servant,
"Charles Vignolles."

"*The Right Honourable the Marquess of Northampton,*
&c. &c. &c.
President of the Royal Society."

The following papers were read, viz.—

1. "A Meteorological Journal for 1840, kept at Allenheads,

Northumberland, with a few remarks on the Rain-gauge." By the Rev. W. Walton, F.R.S.

The author shows that if the mouth of a rain-gauge be placed in any plane which is not perfectly horizontal, the results will be liable to inaccuracy, whatever may be the direction in which the rain falls. He thinks that, on many occasions, the drops of rain diminish in their size during their descent on entering warmer regions of the atmosphere, so as finally to disappear.

2. "The Scholar's Lute among the Chinese." By — Lay, Esq. Communicated by S. H. Christie, Esq., Sec. R.S.

The Kin, which is the stringed instrument here described, was the one played upon by Confucius and the sages of antiquity, and is therefore held sacred by men of letters. It is made of the Woo-tung, or *Dryandria cordifolia.* It is convex above and plane below, and is wider at one end than at the other; it has two quadrangular apertures in the plane surface, which open into as many hollows within the body of the instrument; and it is furnished with seven silken strings of different diameters, which pass over the smaller end, and are distributed between two immovable pegs below. A bridge within a short distance of the wider extremity gives these strings the necessary elevation and a passage to the under surface, where, by means of a row of pegs, they are tightened or relaxed at pleasure. The length of the sounding-board is divided by thirteen studs of nacre, or mother-of-pearl, as a guide for the performer; and they are placed so that the length of each string is bisected, trisected, &c., that is, divided into aliquot parts as far as the eighth subdivision, with the omission of the seventh, the number of sections being represented by the arithmetical series

2, 3, 4, 5, 6, 0, 8.

Thus the intervals, or magnitudes of the different tones sounded by this instrument, do not accord with those produced on our violin, but agree more with the old Scotch music. The study of this instrument, and the art of playing upon it, are rendered extremely difficult by the complexity of the Chinese notation of written music, which leads to frequent omissions and blunders. Thus every air which a Chinese plays has cost him the labour of many months to learn; and so tiresome is this acquisition, that the author has heard some extemporize very prettily without being able to play a single air. Their performance, however, is very graceful; and though the melody be simple, every scope is given to variety by the mode of touching the strings. The author enters into an examination of the musical theory of the sounds produced by this instrument.

April 22.—The following papers were read, viz.—

1. Magnetic-term Observations taken on board H.M.S. Erebus and Terror, at Hobart Town, on the 29th and 30th August, and the 23rd and 24th September, 1840, by, and under the direction of James Clark Ross, Captain R.N., F.R.S., and Commander of the Antarctic Expedition.

2. Magnetic-term Observations made at the fixed Magnetic Observatory, Van Diemen's Land, on the 28th, 29th and 30th August,

and the 23rd and 24th September, 1840; by James Cla
Captain R.N., F.R.S., Commander of the Antarctic Expedi

3. Hourly Magnetic Observations for August and Se
1840, taken at the Ship's Magnetic Observatory, Van I
Land, under the direction of James Clark Ross, Captai
F.R.S., Commander of the Artarctic Expedition.

The above papers were communicated by the Lords Com
ers of the Admiralty.

4. Variation de la déclinaison, intensité horizontal et in
magnétique, observés à Milan, pendant vingt-quatre heures
le 24 et 25 Fevrier et Mars, 1841, par Signior Carlini, Forn.
R.S.

5. "Remarks on the Birds of Kerguelen's Land." By R.
mick, Esq., Surgeon R.N. of H.M.S. Erebus. Communi
the Lords Commissioners of the Admiralty.

The birds usually met with by the author in this isla
petrels and penguins; and besides these, he found two sp
gull; a duck, a shag, a tern, a small albatros, and a species of
and also a remarkable nocturnal bird allied to the Procellari
notices are given of the forms and habits of these birds.

6. "Geological Remarks on Kerguelen's Land." By R.
mick, Esq., Surgeon R.N. of H.M.S. Erebus. Communi
the Lords Commissioners of the Admiralty.

The northern extremity of the island is described as being
of volcanic origin. The trap rocks, of which the headlands
posed, form a succession of terraces nearly horizontal. Basa
prevailing rock: it assumes the prismatic form, and pas
greenstone, and the various modifications of amygdaloid
phyry. The general direction of the mountain-ranges in
the south-west and north-east, and they vary in height from
2500 feet. Many of the hills are intersected by trap dykes,
of basalt. Several conical hills, with crater-shaped summ
found, evidently the remains of volcanic vents. Three or f
singular isolated hills, composed of an igneous slaty sandston
in Cumberland Bay, presenting very smooth outlines, and co
of piles of broken fragments, through which the mass protr
places, in prismatic columns. Vast quantities of *débris* a
mulated at the base of the hills, in many places to the heigh
or 300 feet or more, affording strong evidence of the rapid
gration this land is undergoing, from the sudden atmospher
situdes to which it is exposed.

The whole island is deeply indented by bays and inlets,
surface intersected by numerous small lakes and water
These, becoming swollen by the heavy rains, which altern
frost and snow, rush down the sides of the mountains and al
ravines in countless impetuous torrents, forming, in many
beautiful foaming cascades, wearing away the rocks, and
the platforms and valleys below with vast fragments of ro
slopes of rich alluvium, the result of their decomposition.

The most remarkable geological feature in the island is th rence of fossil wood and coal, and what is still more extraordinary, these are imbedded in the igneous rocks. The wood, which is for the most part highly silicified, is found enclosed in the basalt; whilst the coal crops out in ravines, in close contact with the overlying porphyritic and amygdaloidal greenstone.

A few outline sketches of the rocks and scenery, in various parts of the island, accompany this paper.

A paper was also in part read, entitled, "On the proportion of the prevailing Winds, the mean Temperature, and depth of Rain in the climate of London, computed through a cycle of eighteen years, or periods of the Moon's Declination." By Luke Howard, Esq., F.R.S.

April 29.—The reading of Mr. Howard's paper was resumed and concluded.

In this paper the author investigates the periodical variations of the winds, rain and temperature, corresponding to the conditions of the moon's declination, in a manner similar to that he has already followed in the case of the barometrical variations, on a period of years extending from 1815 to 1832 inclusive. In each case he gives tables of the average quantities for each week, at the middle of which the moon is in the equator, or else has either attained its maximum north or south declination. He thus finds that a north-east wind is most promoted by the constant solar influence which causes it, when the moon is about the equator, going from north to south; that a south-east wind, in like manner, prevails most when the moon is proceeding to acquire a southern declination; that winds from the south and west blow more when the moon is in her mean degrees of declination, going either way, than with a full north or south declination; and that a north-west wind, the common summer and fair weather wind of the climate, affects, in like manner, the mean declination, in either direction, in preference to the north or south, and most when the moon is coming north.

He finds the average annual depth of rain, falling in the neighbourhood of London, is 25·17 inches.

From his observations on the temperature, he deduces the following conclusions:—1. That the pressure of an atmospheric tide, which attends the approach of the moon to these latitudes, raises the mean temperature 0·35 of a degree. 2. That the rarefaction under the moon in north declination lowers the temperature 0·13 of a degree. 3. That the northerly swell following the moon as she recedes to the south further cools the air 0·18 of a degree. 4. That this cold continues while the moon is away south, reducing the mean temperature yet lower by 0·04 of a degree *.

The following papers were then read:—

1. "A new Method of solving Numerical Equations." By Mr. Thomas Weddle, of Stamfordham. Communicated by S. H. Christie, Esq., M.A., Sec. R.S.

The object of this paper is to develope a new and remarkably

[* On the subject of this paper see *ante*, p. 552 and p. 555.]

simple method of approximating to the real roots of numerica
tions, which possesses several important advantages. Af
scribing the nature of the transformations which are subse
employed, the author proceeds to develope the process he u
obtaining one of the roots of a numerical equation. Passi
the difficult question of determining the limits of the roots,
poses the first significant figure (R) of a root to have been asce
and transforms the proposed equation into one whose roots
roots of the original, divided by this figure $\left(\text{or } \frac{x}{\text{R}}\right)$: one root
equation lying between 1 and 2, the first significant figure (r
decimal part is obtained, and the equation transformed into
whose roots are those of the former, divided by $1 +$ this
(or $1+r$). This last equation is again similarly transformed
transformations being readily effected by the methods first
Proceeding thus, the root of the original equation is obtaine
form of a continued product. After applying this method to
a root of an equation of the 4th, and likewise one of the 5th
the author applies it to a class of equations to which he cons
peculiarly adapted, namely, those in which several terms ar
ing. One of these is of the 16th degree, having only six
and another is of the 622nd degree, having only four terms.

2. "Additional Note on the Contraction of Voluntary Mu
the living body." By William Bowman, Esq., F.R.S., Demo
of Anatomy in King's College, London, and Assistant Sur
King's College Hospital*.

This communication contains a short account of some rec
aminations made by the author on the human muscular fibre
by tetanus. The effect of the violent contractions which ch
ize this disease, is to produce, in many parts of the muscle
siderable ecchymosis, which gives the contiguous portions
and gray aspect. In other places the muscles lose, in a gre
sure, their fine fibrous character, and exhibit a soft mottled
which is easily torn. The primitive fasciculi, when microsc
examined, present indications of strong contraction, ap
swollen into a fusiform shape, and having their transverse
some parts much more closely approximated, and in others se
to much greater distances than in the natural state, or even alt
obliterated, in consequence of the whole texture being bro
into those primitive elements of which the discs are construct
frequently they are broken across without a corresponding
of the sarcolemma.

The author is led from his observations to the conclusion
that the contraction of a muscle is the essential cause of its r
2ndly, that there is no repellent force between the contract
ments of muscular fibre; and, lastly, that the contraction of

---

[* An abstract of Mr. Bowman's former paper appeared in Ph
Third Series, vol. xvii. p. 386.]

tary muscle is not a sustained act of the whole congeries of contractile elements composing it, but a rapid series of partial acts, in which all duly share, becoming by turns contracted and relaxed.

The paper is accompanied by drawings of the microscopic appearances therein described.

May 6.—The following papers were read, viz.—

1. "Investigation of a New and Simple Series, by which the Ratio of the Diameter of a Circle to its Circumference may easily be computed to any required degree of accuracy." By William Rutherford, Esq., of the Royal Military Academy, Woolwich. Communicated by Samuel Hunter Christie, Esq., M.A., Sec. R.S.

Among various formulæ for the rectification of the circle discovered by the author, he has found the one given in this paper to be that best fitted for computation; and he has been enabled by means of it, with comparatively little labour, to extend the number, expressing the ratio of the diameter to the circumference, to 208 places of decimals, a degree of accuracy hitherto unattainable, without a great amount of labour, by means of any of the series which have yet been employed.

The celebrated series of Mr. John Machin, for the rectification of the circle, is derived from the formula

$$\frac{\pi}{4}=4\tan^{-1}\frac{1}{5}-\tan^{-1}\frac{1}{239},$$

which converges with considerable rapidity, but gives rise to tedious computations, in consequence of the divisor 239 being a prime number. But by converting the above formula into the following,

$$\frac{\pi}{4}=4\tan^{-1}\frac{1}{5}-\tan^{-1}\frac{1}{70}+\tan^{-1}\frac{1}{99},$$

a series is obtained by which the extended computation above mentioned was readily effected.

The methods of computation are then stated in detail, and the resulting value of π is given to 208 places of decimals, which is presumed to be accurate to the last figure, the computations having been actually carried as far as 210 figures.

2. "On the Phænomena of thin plates of Solid and Fluid Substances exposed to polarised light." By Sir David Brewster, K.H., D.C.L., F.R.S., & V.P.R.S. Ed.

From a theoretical investigation of the phænomena described in this paper, the author deduces the important law, that when two polarized pencils, reflected from the surface of a thin plate, lying on a reflecting surface of a different refractive power, interfere, half an undulation is not lost, and white-centred rings are produced. When the inclination is exactly 90°, the pencils do not interfere, and no rings are produced.

GEOLOGICAL SOCIETY.

[Continued from p. 527.]

June 10, 1840 (*Continued*).—On the mineral veins of the Sierra Almagrera, in the province of Almeria, in the South of Spain, by J. Lambert, Esq., F.G.S.

The Sierra Almagrera extends from the mouth of the Almanzora

(lat. about 37° 17′, long. about 15° 40′) in a N.N.W. direc
twelve miles. Its width is about a mile and a half, and its
height 1400 feet. It is composed of clay-slate, resting up
slate, accompanied by beds of milky quartz, and crossed by n
ferruginous veins containing sulphate of barytes and gypsu
strata of the clay-slate are generally horizontal, but are so
inclined from 15° to 20°, and even more, where disturbed b
of greenstone.

The vein of the Barranco Jaroso was the first discove
it is now of considerable richness. Its excavations exte
than 200 yards in length, with every indication of the l
tinuing. The direction of the vein is north to south,
one and one and a half hours, or 15° to 22½° east of north ;
inclination is from 65° to 70° east. The breadth of the vei
it was commenced at the surface, was half a yard, but it
creased to three yards at the depth of forty yards, the point
it had been carried in April 1840. The mineral content
vein consist of parallel divisions of several varieties of galen
stallized, radiated with an antimonial aspect, brilliant large
fine steel-grained, and black, of oxide and carbonate of l
argillaceous iron ore ; carbonate of iron and carbonate o
also occur ; and sulphates of barytes and gypsum are abun

Old workings, supposed to have been conducted by the
occur in great numbers, principally at the mouths of the B
or ravines of Pinalbo del Frances and de la Torre. Quantities
timber, decayed iron tools and lamps of clay, have been
them ; but in no case does it appear that gunpowder was
making the excavations. Large heaps of slags and scor
frequent occurrence ; one of the most important being sit
tween the confluence of the Almanzora and the Rambla d
at the foot of the little hill Cabéza de las Herrerias (Hea
Forges).

This hill, Mr. Lambert says, presents the aspect of a
crater, and has disturbed the tertiary deposits of the neighb
He states, that it is an enormous mass of oxide of iron, wit
titude of veins of sulphate of barytes ; and that it is a
honey-combed by old excavations in the barytes veins, the
of which, he believes, were used as a flux in smelting th
tiferous minerals. The tertiary beds at this locality consist
resting upon conglomerates, and are all charged with iron
are stated to contain also "ferruginous and jaspery dendrite
of felspar and crystals of barytes.

A tertiary formation extends from the foot of the Sierra A
to the Sierras de Filabres, de Alhamilla and Cabrera. T
part consists of an arenaceous conglomerate alternating wi
and beds of quartz and other pebbles of various sizes. The c
tain gypsum and sands very similar to those of the vicinity
and numerous organic remains, belonging principally to th
Ostrea, Pecten, and Dentalium ; likewise corals. The for
disturbed in many parts by protruded masses of greenst

by porphyries, trachytes and basalts, which are stated to present very singular phænomena. Gneiss projects above the tertiary strata in many places.

The paper was accompanied by specimens of galena, of which the following analyses are given in the paper itself:—

Specimen, No. 1. ………… 70·8 per cent. Lead 1·05 per cent. Silver 16 oz. per gal.
———— 2. Radiated 62·1 ———— 0·65 ———— $10\frac{4}{10}$ —
———— 3. Black … 22·25 ———— 0·325 ———— $5\frac{9}{10}$ —

About 400 tons of numbers 1 and 2, have been extracted from the mine during the months it had been worked; and the produce about the time the paper was written was fifteen tons a day.

A notice on the Sierra de Gador, and its lead mines, by Josias Lambert, Esq., F.G.S.

The Sierra de Gador, celebrated for its lead mines, is situated between the Sierra Nevada and the Mediterranean. Its length from west to east is nearly forty miles; its breadth varies from five to ten miles, and its highest point,—the Collado de los Valientes, near its western extremity,—is upwards of 6000 feet above the sea. From that point eastward, the height gradually decreases till it is reduced near the Almeria to the level of that river. The southern face is precipitous, and from its base to the Mediterranean extends the tertiary plain of Dalias. The western flank is also precipitous, but the northern face rises more gently from the river Almeria, which separates the Sierra de Gador from the Sierra Nevada.

The principal mass of this range of mountains is composed almost exclusively of a limestone, considered by Mr. Lambert to belong to the lowest of the transition rocks, because its stratification is in general conformable to that of the nucleus of the Sierra Nevada, and because it is believed to contain no organic remains. It is of a grey or dark colour, and of a compact or finely grained texture, but it is sometimes, though rarely, friable. It passes downwards by alternations and transitions into clay-slate, talcose or mica-slate, and in the upper part, it is connected with breccias and limestone conglomerates.

The limestone is generally traversed by veins of calcareous spar and fluor spar, frequently so arranged as to resemble the stripes in the skin of a zebra. It is also occasionally magnetic, on account of disseminated particles of magnetic iron. The stratification is regular in some places, but it generally presents great inflexions and disruptions, and curves of a thousand different natures. The prevailing strike of the beds is from east to west, but from the western base of the mountain at Castala to near the highest point in the Loma del Sueño and Collado de los Valientes, it is from north to south. The inclination of the beds varies very much both in direction and inclination; the former being sometimes south, but often north, and occasionally east; and the latter differing from 15° to 45°. The beds are for the greater part compact, but the higher consist of a breccia, mixed with slate-clay and clayey ochres. The stratification in these beds is sometimes well pronounced, and at

others less so, being of great thickness. The fragments of the
reous breccia are angular and of various sizes, and are cement
carbonate of lime.

In the ravine of Cartala, protruded masses of trap, cont
veins of asbestos, amphibole, and porcellanite, are stated to ha
located the strata; an inconsiderable vein of trap is also des
as interposed between two beds of limestone. The slate
near the masses of trap, are said to be frequently of a green c

At the eastern extremity of the chain, the limestone is o
by beds of gypsum, containing masses and strings or small v
native sulphur.

There is no doubt that mines in this mountain chain were w
by the Romans. The ore is generally found in nests or ma
inconsiderable size; also in veins and branches of limited ext
any constant direction, crossing each other, and forming
always communications between the nests. Mr. Lambert is
fore induced to consider these metallic accumulations as of co
poraneous origin with the limestone. At the mine of Arm
the western edge of the Sierra, the ore occurs between two b
limestone, having the same direction east, 20° north, and d
with them 80° to the N.E. It is one foot thick, and is accom
by clay. The same agreement has been found in Santa Rosa,
Catalina, Cruzados, Trinidad primero, and in all the mines si
upon the western declivity, looking towards Berja.

In the Loma del Sueno, and the interior parts of the Sierra,
the beds incline only 20° to 30°, but frequently exhibit gre
locations, which change their position entirely, and often
crests, fissures, and hollows filled with argillaceous substanc
found the greatest masses of ore, lying between the beds, an
forming to all their modifications. Fluor almost always a
panies the galena.

One level has been carried nearly 600 yards in length, fro
bottom of a precipice of nearly equal altitude, in order to
mine the rich deposits on the edge of the Loma del Sueñ
hitherto nothing has been met with but compact and slaty
stone. Mr. Lambert therefore infers, that the ore is more
ficial, and he adds, there is no instance of its having been fo
a greater depth than 200 yards from the surface.

The lower parts of the fissures which traverse the limesto
frequently filled with fragments of ore enveloped in a red
soil, and associated with angular as well as rounded fragme
limestone. In the alluvial detritus of the ravines, and t
deltas at their mouth, fragments and masses of ore have be
tracted, often in considerable quantities, and at the Pecho
Lastras to the extent of 100,000 tons.

In the limestone mountains which stretch westward fro
Sierra de Gador to Marbella, within forty miles of Gibralta
ore is found in variable quantities, but not so abundantly as
Sierra de Gador.

In conclusion, Mr. Lambert observes, that the improvide thod of working the ore in that mountain is fast destroying the best mines; that new trials have not been attended with anything like success; and that the hardness of the rock renders sinkings very expensive, and compels adventurers with limited funds to abandon their undertakings, unless ore be speedily obtained.

On the polished and striated surfaces of the rocks which form the beds of Glaciers in the Alps, by Professor Agassiz.

This paper was accompanied by a series of plates intended to represent the effect of glaciers upon the rocks over which they move.

These effects, consisting of surfaces highly polished, and covered with fine scratches, either in straight lines or curvilinear, according to the direction of the movement of the glacier, are constantly found, not only at the lower extremity, where they are exposed by the melting of the glaciers, but also, wherever the subjacent rock is examined, by descending through deep crevices in the ice. Grains of quartz and other fragments of fallen rocks, which compose the moraines that accompany the glaciers, have afforded the material which, moved by the action of the ice, has produced the polish and scratches on the sides and bottom of the Alpine valleys through which the glaciers are continually, but slowly descending. It is impossible to attribute these effects to causes anterior to the formation of the glacier, as they are constantly present and parallel to the direction of the movement of the ice. They cannot be considered as the effects of an avalanche, for they are often at right angles to the direction in which an avalanche would descend; they are constantly sharp and fresh beneath existing glaciers, but less distinct on surfaces which have for some time been left exposed to atmospheric action by the melting of the ice. In the valley of the Viesch, the direction of the scratches is from north to south, or towards the Rhone; the direction of those which accompany the glacier of the Rhone is from east to west; that of those beneath the glacier of the Aar is first from west to east, as far as the Hospice of the Grimsel; and then from south to north, from the Grimsel to the Handeck. If we would account for these scratches by the action of water, we must imagine currents of enormous depth filling these highest Alpine valleys, and descending in opposite directions from the narrow crest that lies between them. In the upper part of the valley of the Viesch, is a glacier, beneath which runs a rapid torrent, co-extensive in length with the great current, to which the above hypothesis would attribute the polish and scratches on the rocks of the valley. This small torrent corrodes the bottom of the valley into sinuous furrows and irregular holes, and polishes the sides of its bed; but the polish is of a different aspect from that produced by the action of the ice, and of the stones and sand which it carries with it. The polished surfaces beneath the ice are often salient and in high relief. The sides also of the valleys adjacent to the actual glaciers are frequently polished and scratched at great heights above the ice, in a manner identical with the surface beneath it, but different from the polish of the bed of the torrent.

The amount of polish and scratches varies with the n the rocks. In the valley of Zermatt and Riffelhorn, rocks pentine are most exquisitely polished; so also are the gra the sides of the glacier of the Aar, where they have not be exposed to the action of the air. Gneiss and limestone do serve their polish under similar exposure, but retain it while protected by ice or a covering of earth.

These facts seem to show, that the striated and polished c of rocks beneath and on the sides of glaciers, is due to the the ice, and of the sand and fragments of stone forming raines which accompany it.

On a bed of lignite near Messina, by Dr. R. Calvert.

About thirty years ago, Dr. Calvert discovered a bed of l quarter of a mile from Messina, up a Fiumera to the left Gonzago. It cropped out to the north at an angle of ab and was at least a yard in thickness. The lignite was swep a precipice by the country people to make room for sticks lime with; a superb quarry of which was then worked on t site side of the field. Dr. Calvert laid in a winter stock lignite; the dragoons used it in their forge, and the comm the forces in his kitchen. Owing, however, to the unskilfu the people who dug the lignite (soldiers and officers' servan roof fell in, and the property above being injured, the exc were stopped. Some of the lignite emitted a bad effluv burned.

A letter from Richard Greaves, Esq., addressed to Dr land, and dated June the 6th, 1840, on the discovery of b birds, fishes, and mammalia, in the limestone cliff at Eel Caldy Island, and about eighty feet above the sea.

A note from Mr. Hamilton, Sec. G.S., addressed to Dr land, on the irregular occurrence of rounded fragments crystal, throughout the Hastings sands, in the neighbour Tunbridge Wells. Mr. Hamilton's principal object is to ca tion to the inquiry whence the fragments were obtained; the assistance which this knowledge would afford in determi origin of the other materials forming the Hastings sands.

A letter, dated May 6th, 1840, from M. Roemer, of heim, to Dr. Fitton, on the chalk and the subjacent forma the Purbeck stone inclusive in the north of Germany.

a. Chalk with flints.—This formation, presenting characte exactly agree with those of the chalk of England, is found the island of Rugen. It there consists of a white limestone, quent layers of flints, and includes the same fossils.

M. Roemer is of opinion that the Rugen deposit is of the ag Maestricht beds, though most geologists believe it to be you

In the north of Germany there are very thick deposits stone and sandy marls, which correspond, M. Roemer the upper subdivision of the chalk formation. The chara fossils are *Callianassa* (*Pagurus*) *Faujasii*, *Belemnites* small corals, &c. No Ammonites have been found in it. The localities where it occurs, are Gehrden near Hanover, Goslar, Quedlenburg, and Halberstadt.

b. Chalk without flints exists near Piena and Luneberg, with the same external characters as in England. Near Ilseburg, Lemforde, Dulmen, and other localities, it is represented by sandy marls and sandstones. It contains *Belemnites mucronatus*, many Scyphia, some Ammonites, &c.

c. Chalk marl (Pläner Kalk).—This formation is extensively distributed and well exposed in the north of Germany, and exhibits everywhere the same characters as in England. It contains no *Belemnites mucronatus*, but *Ammonites varians*, *A. Mantelli*, *A. Gutoni*, *Turrilites costatus*, *T. undulatus*, *Plicatula inflata*, &c.

d. Upper greensand.—This formation, as a greyish green marl, with grains of silicate of iron, occurs only near Dresden and near Wal in Westphalia; also in the neighbourhood of Hildesheim. It contains *Ammonites falcatus*, *Terebratula biplicata*, *Ostrea carinata*, *Spatangus subglobosus*, &c.

The chalk marl in general gradually becomes more sandy, and passes into a sandstone with veins of oxide of iron, but which contains no fossils. The sandstone constantly accompanies the chalk marl.

e. Gault.—This deposit has not been detected with certainty in the north of Germany, but M. Roemer thinks it may be represented by a marl which occurs between Hanover and Hildesheim, and contains *Hamites compressus*; and by a blue clay near Ottbergen. He has not been able to find it near Aix-la-Chapelle.

f. Lower greensand.—The mineralogical characters of this formation are the same as in England. It occurs in Saxony, at the foot of the Hartz, near Celfeld, near Bilefeld, and near Hattern in Westphalia, also near Aix-la-Chapelle. Its fossils are not very numerous.

g. Hils conglomerate.—To M. Roemer, geologists are indebted for first pointing out the existence, as distinct deposits, of the Hils conglomerate and the Hils clay.

The *Hils conglomerate* consists of a yellowish or brownish marl, containing grains of quartz, schist, and oxide of iron. It forms very thick beds, and includes in some localities very rich iron ores. It occurs near Brunswick, at Goslai, and near Essen on the Ruhr in Westphalia. Its fossils are very numerous, and partly identical with those of the lower greensand of England; for example, it contains *Terebratula latissima*, *T. depressa*, *T. oblonga*, *T. sella*, *Ostrea carinata*, *O. macroptera*, *Pecten quinquecostatus*, &c. M. Roemer asks if it be the equivalent of the Neocomien.

h. The *Hils clay* is a bluish pure clay, 100 feet thick. M. Roemer considers it to be the Speeton clay of England, as it contains *Mya depressa* (Phillips), *Glyphæa ornata* (*Astacus ornata*, Phillips), and *Isocardia angulata* (Phillips), with a great quantity of other fossils, which are in part Jurassic species, namely, *Ammonites sublævis*, *A.*

mutabilis, and *A. coronatus*. M. Roemer has noticed the H
near Hildesheim, near Celfeld, where it contains a consi
layer of iron; at the foot of the Deister near Hanover, an
Trenndorf.

i. Weald clay.—A stiff bluish or brownish clay, seldom c
ing subordinate beds of limestone and sandstone. The fos
almost exclusively the same as those enumerated in Dr.
memoir on the beds below the chalk in the south-east of En
and are entirely freshwater, with the exception of an Astarte

k. Hastings sandstone.—In the north of Germany this for
is composed of a white, grey or fawn-coloured sandstone, son
alternating in the upper part with greyish clay, and contain
seven to ten beds of coal. Its general thickness amounts to
800 feet. The beds of coal vary from one to three feet in thi
and are separated by sandstone, which is sometimes only a f
thick. The fossils belong to the genera *Paludina*, *Unio*,
nites, *Abies*, *Sphenopteris*, and *Lonchopteris*, and M. Roen
found every species mentioned in Dr. Fitton's memoir bef
luded to. The sandstone is generally less ferruginous
England.

l. Purbeck strata.—These beds are described by M. Ro
consisting of shelly limestones alternating with thin layers
stone, and concretional masses of grit. He has observed t
beds, but as yet no Cycadeoidea. The shells which he has
are partly marine, partly freshwater, and belong to the
Paludina, *Ostrea*, *Cyrena*, *Gervilia*, *Serpula*, &c.

m. Portland stone.

The Wealden formation, M. Roemer states, is exhibit
Helmsted. He hopes it will be exposed near Hildesheim.
westward it extends from Hanover by Minden, to Iburg and
near Munster in Westphalia; furnishing almost everywher
good coal. The fossils found in the strata below the lowe
sand, M. Roemer has accurately figured and described in his
and they proved the identity of the Wealden deposits of
and the north of Germany.

A letter from H. B. Mackeson, Esq. to Dr. Fitton, dated
June 7th, 1840.

On the 19th of May, Mr. Mackeson discovered some por
a large saurian, he believes an Iguanodon, near the bottom
lower greensand in the vicinity of Hythe. On the 6th o
he revisited the quarry, and ascertained that the work-peo
followed the remains for upwards of fifteen feet. On that o
Mr. Mackeson superintended the disinterment of probably
This bone, and others previously obtained, with their bulky
required a cart and two horses for their removal. Up to
of the letter, no vertebræ, teeth, phalangeal or smaller bone
extremities had been found by the workmen. In the same
Mr. Mackeson has obtained a large Ammonite, *Gervillia avi*
and other shells characteristic of the lower greensand.

Nov. 4.—A paper was read on Glaciers, and the evidence of their having once existed in Scotland, Ireland, and England, by Professor Agassiz, of Neuchâtel.

M. Agassiz commences by observing, that the study of glaciers is not new, as Scheuchzer visited, and even drew, most of the glaciers of Switzerland; and as, at a later period, Gruner and De Saussure examined them in great detail, and left few of their phænomena uninvestigated. Hugi also, in his account of the Alps, and Scoresby, in his descriptions of the arctic regions, have communicated much valuable information respecting glaciers, but without giving rise to any important geological results. Venetz and De Charpentier first ascribed to the agency of glaciers, the transport of the erratic boulders of Switzerland, supposing that the Alps formerly attained a greater altitude than at present, and that the glaciers extended to the plains of Switzerland, and even to the Jura. This assumed greater height of the Alps M. Agassiz dissents from, as no geological phænomena compel him to admit it; and the arrangement of the boulders proves that the blocks were not pushed forward by the glaciers, as conjectured by M. de Charpentier. Moreover, the phænomena of erratic boulders extend over all the temperate and northern regions of Europe, Asia and America, and, consequently, could not depend upon so local an event as a greater altitude of the Alps. The consideration of these difficulties induced M. Agassiz to resume the study of glaciers; and after devoting the suitable portion of five successive summers to the study of their details, and all that has been written respecting their structure, he has arrived at the conviction, that the formation of glaciers did not only depend upon the actual configuration of the globe, but was also connected with the last great geological changes in its surface, and with the extinction of the great mammifers which are now found in the polar ice. He is also convinced that the glaciers did not advance from the Alps into the plains, but that they gradually withdrew towards the mountains from the plains which they once covered. In this belief, he says, he is supported by many considerations which escaped previous observers, depending chiefly on the form and relative position of the erratic blocks, and the commonly called diluvial gravel, the former being in Switzerland always angular, and resting on the latter, which consists of rounded materials. Considered in this point of view, glaciers assume an entirely new importance, for they introduce a long period of intense cold between the present epoch and that during which the animals existed, whose remains are buried in the usually called diluvial detritus.

Having established his theory as completely as he could, by repeated investigations of Switzerland and the adjacent portions of France and Germany, M. Agassiz became desirous of investigating a country in which glaciers no longer exist, but in which traces of them might be found. This opportunity he has recently enjoyed, by examining a considerable part of Scotland, the north of England, and the north, centre, west and south-west of Ireland; and he has arrived at the conclusion, that great masses of ice, and subsequently

glaciers, existed in these portions of the United Kingdom a
immediately preceding the present condition of the globe,
his belief upon the characters of the superficial gravels an
blocks, and on the polished and striated appearance of the roc

M. Agassiz does not suppose that his views respecting
will at once meet with the general concurrence of geolog
he admits that the study of the phænomena of glaciers in
latitudes, as well as at different altitudes, together with th
nation of their different effects where in contact with the
introduce many modifications in the consideration of a
phænomena in countries where glaciers have disappeared;
prepared to discuss his theory within the limits of obser
conscious of having searched for truth solely to advance the
of science.

To avoid useless discussion, he states, that in attributi
action of glaciers a considerable portion of the results
ascribed exclusively to that of water, he does not wish to
that everything hitherto assigned to the agency of water
produced by glaciers; he only wishes that a distinction may
in each locality between the effects of the different agents
adds, that long-continued practice has taught him to di
easily, in most cases, the effects produced by ice from those
by water.

Proceeding to the consideration of facts, he says the dis
of blocks and gravel, as well as the polished and striated su
rocks *in situ*, do not indicate the action of a mighty curren
from north-west to south-east, as the blocks and masses
everywhere diverge from the central chains of the country,
the course of the valleys. Thus in the valleys of Loch Lon
Loch Long, they range from north to south; in those of L
and Loch Awe from north-west to south-east; of Loch E
Loch Leven from east to west; and in the valley of the Fo
north-west to south-east, radiating from the great mountai
between Ben Nevis and Ben Lomond. Ben Nevis in the
Scotland, and the Grampians in the south, are considere
author to constitute the great centres of dispersion in that
and the mountains of Northumberland, Westmoreland,
land, and Wales; as well as those of Ayrshire, Antrim, th
Ireland, and Wicklow, to be other points from which bl
gravel have been dispersed, each district having its peculia
traceable in many instances to the parent rock, at the hea
valleys. Hence, observes M. Agassiz, it is plain the cau
transport must be sought for in the centre of the mountai
and not from a point without the district. The Swedish b
the coast of England do not, he conceives, contradict this
as he adopts the opinion that they may have been transp
floating ice.

In describing the phænomena presented by erratic bl
gravel, M. Agassiz first insists upon the necessity of disting
between stratified gravel and mud containing fossils, whi

not have been accumulated by true glaciers, although the materials may have often been derived from them; and unstratified masses, composed of blocks, pebbles, and clay. These stratified deposits he considers to be of posterior origin to the glacier epoch. The till of Scotland, or the great unstratified accumulation of mud and gravel, containing blocks of different size heaped together without order, and containing no organic remains but bones of Mammalia and insignificant fragments of shells, he is of opinion was also not produced by true glaciers, although intimately connected with the phænomena of ice. The polished and striated surfaces of the blocks leave no doubt on M. Agassiz's mind that these masses have been acted upon by ice in the same manner as the blocks which are observed under existing glaciers, and which are more or less re-arranged by water derived from the melting of the glaciers.

Similar detritus fills the bottom of all the Alpine valleys, as that of the Rhone from its mouth to its junction with the Lake of Geneva, and the valley of Chamounix: it is found between the Hospice de Grimsel and the borders of the lower glacier of the Aar; thence to the neighbourhood of Goutharen in the valley of Oberhasli, at Im Grund, in the plains of Meiringen, and in Interlasken; also between Thun and Berne. At all these localities, M. Agassiz considers, the blocks were left, when the glaciers extended to them.

With respect to the valley of the Aar, M. Agassiz says it is easy to prove that the rounded pebbles of Alpine rocks spread along its whole course, were not transported to their present position by that river, because between the glacier from which it issues and Berne, the flowing of the stream is interrupted by the barrier of Kirchet, the Lake of Brienz, and the Lake of Thun; and because between these lakes its velocity is so small, that it transports only mud and very fine gravel, and that the pebbles over which the river flows below Thun do not issue from the lake. Supposing that the volume of the Aar was formerly greater, why, asks M. Agassiz, are not the lakes of Brienz and Thun filled in the same manner as the plain of Meiringen and the bottom of the valley which separates the two lakes? All difficulties, however, he is of opinion, vanish, if the pebbles be considered the detritus of retreating glaciers, and that the hollows occupied by the lakes of Brienz and Thun were filled with glaciers.

The existence of a glacier in this valley is not imagined by the author to explain the origin of the detritus, as its having existed is proved by the polish on the rocks *in situ*, from the glacier of the Aar to Meiringen, a distance of twenty English miles, at the height of 8000, 7000, and 6000 feet successively above the level of the sea; and even on the shores of the Lake of Thun. Similar phænomena have been noticed by M. Agassiz in Scotland, in the valleys of Loch Awe and Loch Leven, near Ballachalish, and in England in the neighbourhood of Kendal.

The author then proceeds to describe the moraines of Switzerland, or the accumulations of blocks and pebbles deposited longitudinally on the borders, and transversely in front, of glaciers, and successively abandoned by them in their retreat. The longitudi raines differ from glacier-detritus remodelled or spread out b in being disposed in ridges with a double talus, one flank of presented to the glacier, and the other to the side of the vall their continuity and parallelism at the same height easily dist them from the debris disposed along the bottoms of valleys rents. They occur on the flanks of all glaciers, but they ha also observed by M. Agassiz where no glaciers exist, as in leys of the Rhone, the Arve, the Aar, &c.; likewise in Scotla Inverary, at Muc Airn, at the outlet of Loch Traig, at St and on the borders of the bay of Beauley; in Ireland to the east of Dublin, and near Enniskillen; and in England in th of Kendal, as well as near Penrith and Shap.

The common origin of moraines, and of accumulations of pebbles and of blocks, M. Agassiz says, cannot be doubted former are simple ridges formed on glaciers; the latter, m rounded and polished under glaciers, or great masses of ice, posed by the melting of the ice, and re-disposed by the wat produced.

The author next describes the differences in the internal a ment of the various accumulations. In the stratified depo materials are comparatively much smaller than in glacier-d the pebbles also are elongated, and fine gravel and mud or form the upper beds. On the contrary, in the detritus of large and small materials are associated without order, the blocks being often in the upper part; and where very large blocks occur, they rest on the surface. In moraines there is a distinction, blocks of all dimensions and every form are interm and this difference, he says, is easily understood, by recollecti moraines are composed of the angular blocks which fall on t face of the glacier, as well as of pebbles with rounded edges.

The striated and polished surfaces, so often observed o rocks *in situ*, are next described by M. Agassiz. Without d absolutely the power of water to produce such effects, he sa he has sought for them in vain on the borders of rivers an and on sea-coasts; and that the effects produced by water are furrows proportioned to the hardness of the rocks; not even u polished surfaces, such as those presented by the rocks un cussion, and which are independent of the composition of the moreover wherever the moveable materials which are pressed ice on rocks *in situ* are the hardest, there occur, independent polish, striæ more or less parallel, and in the general dire the movement of the glaciers. Thus in the neighbourhood ciers are found those rounded bosses which Saussure distin by the name of "roches moutonnés." These phænomena M. siz has traced under the glacier of the Aar, and he has o them in the valley of the Rhone, and of Chamounix; also in land, on the banks of Loch Awe and Loch Leven; and he sa are very remarkable in the environs of Kendal.

The most striking points in the distribution of the striæ, a diverging at the outlets of the valleys, and their being oblique, and never horizontal on the flanks, which they would be, were they due to the agency of water, or floating masses of ice. The cause of this obliquity the author assigns to the upward expansion of the ice, and the descending motion of the glacier.

The most remarkable striated rocks in the Alps are near Handeck, and near the cascade of Pissevache; and the best examples M. Agassiz has seen in Scotland, are those of Ballahulish, and in Ireland those of Virginia.

If the analogy of the facts which he has observed in Scotland, Ireland, and the north of England, with those in Switzerland, be correct, then it must be admitted, M. Agassiz says, that not only glaciers once existed in the British Islands, but that large sheets (*nappes*) of ice covered all the surface.

The author then details the proofs that glaciers did not descend from the mountain summits into the plains, but are the remaining portions of the sheets of ice which at one time covered the flat country. It is evident, he says, if the glaciers descended from high mountains, and extended forward into the plains, the largest moraines ought to be the most distant, and to be formed of the most rounded masses; whereas the actual condition of the detrital accumulations is the reverse, the distant materials being widely spread, and true moraines being found only in valleys connected with great chains of lofty mountains.

It must then be admitted, the author argues, that great sheets of ice, resembling those now existing in Greenland, once covered all the countries in which unstratified gravel is found; that this gravel was in general produced by the trituration of the sheets of ice upon the subjacent surface; that moraines, as before stated, are the effects of the retreat of glaciers; that the angular blocks found on the surface of the rounded materials were left in their present position at the melting of the ice; and that the disappearance of great bodies of ice produced enormous debacles and considerable currents, by which masses of ice were set afloat, and conveyed, in diverging directions, the blocks with which they were charged. He believes that the Norwegian blocks found on the coast of England have been correctly assigned by Mr. Lyell to a similar origin.

Another class of phænomena connected with glaciers, is the forming of lakes by the extension of glaciers from lateral valleys into a main valley; and M. Agassiz is of opinion, that the parallel roads of Glen Roy were formed by a lake which was produced in consequence of a lateral glacier projecting across the glen near Bridge Roy, and another across the valley of Glen Speane. Lakes thus formed naturally give rise to stratified deposits and parallel roads, or beds of detritus at different levels.

The connexion of very recent stratified deposits with glacier-detritus, M. Agassiz observes, is difficult to explain; but he conceives that the same causes which can bar up valleys and form lakes, like those of Brienz, Thun and Zurich, may have formed analogous barriers at the point of contact with the sea sufficiently extensive to have produced large salt-marshes to be inhabited by anim remains of which are found in the clays superimposed on th Scotland; and he adds, that the known arctic character of th sils ought to have great weight with those who study the vast of glaciers.

In conclusion, the author remarks, that the question of forms part of many of the great problems of geology; that it a for the disappearance of the large mammifers inclosed in th ice, as well as for the disappearance of the organic beings of called diluvian epoch; that in Switzerland it is associated v elevation of the Alps, and the dispersion of the erratic bloc that it is so intimately mixed up with the subject of a gener nution of the terrestrial heat, that a more profound acqua with the facts noticed in this paper will probably modify the o entertained respecting it.

Nov. 18.—The reading of the first part of a Memoir on t dences of Glaciers in Scotland and the North of England, Rev. Prof. Buckland, D.D., Pres. G.S., commenced on the November, was resumed and concluded.

Dr. Buckland's attention was first directed by Prof. Ag October 1838 to the phænomena of polished, striated, and fu surfaces on the south-east slope of the Jura, near Neuchâtel, as to the transport of the erratic boulders on the Jura, as the of ice; but it was not until he had devoted some days to the nation of actual glaciers in the Alps, that he acquiesced in t rectness of Prof. Agassiz's theory relative to Switzerland. return to Neuchâtel from the glaciers of Rosenlaui and Grind he informed M. Agassiz that he had noticed in Scotland an land phænomena similar to those he had just examined, but he had attributed to diluvial action: thus in 1811 he had o on the head rocks on the left side of the gorge of the Ta Dunkeld, rounded and polished surfaces; and in 1824, in co with Mr. Lyell, grooves and striæ on granite rocks near the e of Ben Nevis. About the same time Sir George Mackenzie poin to the author in a valley near the base of Ben Wyvis, a high gravel, laid obliquely across, in a manner inexplicable by any of water, but in which, after his examination of the effects of in Switzerland, he recognizes the form and condition of a mo

After these general remarks, Dr. Buckland proceeds to d the evidence of glaciers observed by him in Scotland last a partly before and partly after an excursion, in company wit Agassiz; but he forbears to dwell on the phænomena of paral races, though he is convinced that they are the effects of lak duced by glaciers.

The district which Dr. Buckland examined previously to cursion with Prof. Agassiz, lay in the neighbourhood of Du and the line of country which he investigated subsequently, ex in Scotland from Aberdeen to Forfar, Blair Gowrie, Dunke by Loch Tumel and Loch Rannoch to Schiehallion and Tay and thence by Crief, Comrie, Loch Earn Head, Callender an ling, to Edinburgh; and in England by Berwick, Wollar, the Cheviots, Penrith, and Shap Fell, to Lancashire and Cheshire.

Moraine near Dumfries.—The picturesque ravine of Crickhope Linn, about two miles north of Closeburn, and one mile east of Thornhill, intersects nearly horizontal strata of new red sandstone, and is traversed by the Dolland rivulet. On emerging from the upper end of the ravine a long terminal moraine is visible, stretching nearly across the mountain valley, from which the Dolland Burn descends to fall into Crickhope Linn; and it resembles, when viewed from a distance, a vallum of an ancient camp, being covered with turf. It is formed principally of an unstratified mass of rolled pebbles, derived from the slates of the adjacent Lowder Hills, with a few rounded fragments of granite, the nearest rock of which *in situ* is that of Loch Doon, in Galloway, thirty miles to the north-west. Its height varies from twenty to thirty feet; its breadth at the base is about one hundred feet, and its length is four hundred yards. At the southern extremity it is traversed by the Dolland rivulet, and at the northern by the Crickhope Water; and in the centre it is intersected by a road.

Moraines in Aberdeenshire.—Dr. Buckland considers the gravel and sand which cover the greater part of the granite table-land from Aberdeen to Stonehaven to be the detritus of moraines; and the large insulated tumuli and tortuous ridges of gravel, occupying one hundred acres, near Forden, a mile east of Achinbald, to be terminal moraines; also the blocks, large pebbles, and small gravel spread over the first level portions of the valley of the North Esk, after emerging from the Sub-Grampians, to be the residue of moraines re-arranged by water.

Moraines in Forfarshire.—The cones and ridges of gravel at Cortachy and Piersie, near Kirriemuir, and at the confluence of the Carity valley with that of the Proson, are considered by Dr. Buckland to have been produced by glaciers, and modified in part subsequently by water. The polish and striæ on a porphyritic rock near the summit of the hill, on the left side of the main valley, and immediately above the moraines, he is of opinion must also be assigned to glacier action. The vast longitudinal and insulated ridges of gravel, extending for two or three miles up the valley east of Blair Gowrie, and the transverse barriers forming a succession of small lakes in the valley of the Lunanburn, to the west of that town, he considers to be moraines; likewise the lofty mounds comprising the ornamental grounds adjacent to Dunkeld Castle; the detritus covering the left flank of the valley of the Tay, along a great part of the road from Dunkeld to Logierait; that on the left flank of the Tumel valley from Logierait to Killicrankie; and on the left flank of the Garrie, from Killicrankie to Blair Athol.

The vast congeries of gravel and boulders on the shoulder of the mountain, exactly opposite the gorge of the Tumel, Dr. Buckland is of opinion was lodged there by glaciers which descended the lateral valley of the Tumel from the north side of Schiehallion and the adjacent mountains, and were forced across the valley of the Garry, in the same manner as modern glaciers of the Alps (that of the Val

de Bagne, for example,) descend from the transverse, ai
across the longitudinal valleys. Dr. Buckland mentions
millated, polished and striated slate rocks, about one mile
falls of the Tumel, on the left portal of the gorge of the
the effects of a glacier which descended the gorge: he nc
the rounded outline and polish on veins of quartz, whi
eight or ten inches above the weathered surfaces of mass
slate near the same locality. Similar mammillated masse
slate retaining striæ and flutings are visible at Bohaly, c
half miles east of Tumel Bridge.

Evidences of Glaciers on Schiehallion.—The north and
shoulders of the mountain present rounded, polished, an
surfaces, many of which have been recently exposed by the
tion of new roads. On the left flank of the valley called
of Foss, and near the thirteenth milestone, a newly-expose
ry dyke, forty feet wide, exhibited a polished surface anc
parallel to the line of descent which a glacier from Schiehal
take; and on the right flank, one hundred yards north of th
milestone, another and smaller dyke of porphyry present
phænomena. In the intermediate space the recently uncov
rocks and quartzite are rounded, polished, grooved, anc
parallel to the direction which a glacier would assume w
surface is situated.

Moraines at Taymouth.—Two lofty ridges of gravel, wl
the park at right angles to the sides of the valley betwe
lage of Kenmore and Taymouth Castle, the hill, on which
ornamental dairy-house, and the gravel, on which are sit
woods overhanging the left bank of the lower end of l
Dr. Buckland considers to be moraines, or the detritus of
also the deeply-scored and fluted boulders of hornblende
other debris near Fortingal, at the junction of Glen Mc
Glen Lyon.

Moraines in Glen Cofield.—A remarkable group of morai
on the high lands which divide the valleys of the Tay and
and between the sixteenth and fourteenth milestones thirt
round-topped moraines, from thirty to sixty feet high, are
together like sepulchral tumuli. These mounds, compos
stratified gravel and boulders, Dr. Buckland says cannot b
to the action of water, as they are placed precisely where
descending from the adjacent high lands would have actec
greatest velocity; and they exactly resemble some of the
in the valley of the Rhone, between Martigny and Löek.
lage of Amulrie is considered by the author to stand on
low moraines; and the road for two or three miles from it
Glen Almond, to traverse small moraines or surfaces of n
rounded by glaciers. A few conical moraines appear als
high lands between Glen Almond and Crieff.

Proofs of Glaciers in and near Strath Earn.—This part c
ley of the Earn is flanked irregularly with ridges and t
gravel, the detritus of moraines; and on its north side, in

adjacent to Lawers House, near Comrie, hard slaty rocks of the Devonian or old red sandstone system have been rounded and striated. At the west end of Comrie, near the bridge, blue slate rocks have been also rounded and guttered.

Evidence of Glaciers near Comrie.—In this district Dr. Buckland tested the value of the glacial theory by marking in anticipation on a map the localities where there ought to be evidences of glaciers having existed, if the theory were founded on correct principles. The results coincided with the anticipations. On a hill above the gorge, called the Devil's Caldron, near Fentallich, are rounded surfaces of greenstone, partially covered by moraines; and at Kenagart, also immediately above the Devil's Caldron, is a small cluster of moraines, easily separable into lateral and terminal. Two miles up the valley a medial moraine forms a ridge on the level ground, in front of the confluence of Glen Lednoch and Glen Garron. The farm-house of Invergeldy is stated to stand on the detritus of a moraine, and the glen descending to it from Ben-na-cho-ny to be partially obstructed with moraines. The surface of the granite at Invergeldy, which supplied the stone for Lord Melville's monument at Crieff, is rounded and mammillated, but too much weathered to present a polish or striæ. On a hill of trap, however, half a mile south of the farm of Lurg, there is a distinct polish, striated in the direction which a glacier descending the subjacent valley would assume. In Glen Turret, on the shoulder of the mountain immediately above the south-west extremity of Loch Turret, a very deep ravine intersects a vast lateral moraine, which Dr. Buckland shows must have been lodged there whilst the Loch was a mass of ice, and the valley above it filled with a glacier more than five hundred feet above the present level of the lake. At the falls of the Turret, at the lower extremity of the gorge, is an extensive lodgement of moraines; and at the upper end, on the left bank of the Turret, near a gate which crosses the road, the slate-rocks are polished and furrowed; and at both these localities Dr. Buckland had anticipated that glacial action ought to be found.

Evidence of Glaciers near Loch Earn.—On the north bank of the Loch rounded and furrowed surfaces and portions of lateral moraines are exposed by the roadside; and at Loch Earn Head is a group of conical moraines at the junction of Glen Ogle with Loch Earn, and at the very point where, had they been brought by a rapid current, they would have been propelled into the Loch. It is nevertheless the exact position where the terminal moraine of a glacier would be deposited.

Moraines near Callender.—Moraines are stated to cover more or less the valley of the Teith from Loch Katherine to Callender, and the lofty terraces flanking the valley from Callender to Doune are considered to be the detritus of moraines, modified by the great floods which accompanied the melting of the ice. One of them, near Callender, has been mapped as the vallum of a Roman camp. The little lakes on the right bank of the Teith, four miles east of Callender, Dr. Buckland considers due to moraines obstructing the

drainage of the country; and the greater part of the first tal
on the right bank of the Teith, between Callender and Do
cluding the portion on which stands Mr. Smith's farm, to b
posed of re-arranged glacial detritus.

Proofs of Glacial Action at Stirling and Edinburgh.—Havi
shown that glaciers once existed in the glens and mountaino
tricts of Scotland, Dr. Buckland proceeds to point out the e
of glacial action at points but little raised above the level of
and distant from any lofty group of mountains. In 1824
noticed that the trap-rock then recently exposed on the summ
hill [at Stirling], between the castle and the church, was polis
striated, but at his last visit in 1840 these evidences had beco
terated by weathering. The grooves and scratches describe
James Hall on the Costorphine hills near Edinburgh, and on
face of Calton Hill, Prof. Agassiz is of opinion cannot be expl
the action of water; but they resemble, he says, the effects p
by the under-surface of modern glaciers. In his recent exam
in company with Mr. McLaren, of the Castle Rock at Edi
Dr. Buckland found further proofs of the correctness of th
theory, by discovering at points where he anticipated the
occur, namely, on the north-west angle of the rock, distin
upon a vertical polished surface; and at its base a nearly ho
portion of rock, covered with deep striæ; also on the sou
angle obscure traces of striæ and polished surfaces[*]. Some
effects may be imagined to have been produced by stones pr
from the sides or bottom of floating masses of ice; but it i
sible, Dr. Buckland observes, to account by such agency for th
and striæ on rocks at Blackford Hill, two miles south of Edi
pointed out to him by Lord Greenock in 1834. On the so
of this hill, at the base of a nearly vertical cliff of trap, is a
vault, partly filled with gravel and sand, cemented by a rec
tration of carbonate of lime. The sides and roof of the v
highly polished, and covered with striæ, irregularly arran
respect to the whole surface, but in parallel groups over lim
tents. These striæ, Dr. Buckland says, cannot be referre
action of pebbles moved by water; 1st, because fragments
set in motion by a fluid cannot produce such continuous
lines; and 2ndly, because if they could produce them, the lin
be parallel to the direction of the current: it is impossible,
to refer them to the effects of stones fixed in floating ice, as

---

[*] In October 1840, Mr. McLaren found a polished surface on
of rock near the south-west base of Arthur's Seat.

Dr. Buckland has in his possession lithographs copied from draw
by Mr. James Hall, of distinct west and east furrows which exte
portion of the north side of the summit of Calton Hill, and on th
of the carboniferous sandstone at Craig Leith Quarry. Dr. Buc
similar dressings in 1824 in a sandstone quarry near the house of
frey, two miles west of Edinburgh; and in 1840, in a railway
Bangholm Bower, one mile north-east of Edinburgh, he found in
till and sand many striated and fluted boulders.

masses could have come in contact with the roof of a low vault. On the contrary, it is easy, he says, to explain the phænomena of the polish by the long-continued action of fragments of ice forced into the cave laterally from the bottom of a glacier descending the valley, on the margin of which the vault is placed; and the irregular grouping of the parallel striæ to the unequal motion of different fragments of ice, charged with particles of stone firmly fixed in them, like the teeth of a file. The cave is not three hundred feet above the level of the sea, and the proving of glacial action at this point justifies, the author states, the belief that glaciers may also at that period have covered Calton Hill and the Castle Hills of Edinburgh and Stirling.

A paper "On the Geological Evidence of the former existence of Glaciers in Forfarshire," by Charles Lyell, jun., Esq., F.R.S., F.G.S., was commenced.

Dec. 2.—Mr. Lyell's memoir "On the Geological Evidence of the former existence of Glaciers in Forfarshire," commenced on the 18th of November, was concluded.

Three classes of phænomena connected with the transported superficial detritus of Forfarshire, Mr. Lyell had referred, for several years, to the action of drifting ice; namely, 1st, the occurrence of erratics or vast boulders on the tops and sides of hills at various heights, as well as in the bottoms of the valleys, and far from the parent rocks; 2ndly, the want of stratification in the larger portion of the boulder formation or till; and 3rdly, the curvatures and contortions of many of the incoherent strata of gravel or of clay resting upon the unstratified till*. When, however, he attempted to apply the theory of drifting ice over a submerged country to facts with which he had been long acquainted in Forfarshire, he found great difficulty in accounting for the constant subterposition of the till with boulders to the stratified deposits of loam and gravel; for the till ascending to higher levels than the gravel, and often forming mounds which nearly block up the drainage of certain glens and straths; for its constituting, with a capping of stratified matter, narrow ridges, which frequently surround lake-swamps and peat-mosses; and for the total absence of organic remains in the till.

Since, however, Professor Agassiz's extension to Scotland of the glacial theory, and its attendant phænomena, Mr. Lyell has re-examined a considerable portion of Forfarshire, and having become convinced that glaciers existed for a long time in the Grampians, and extended into the low country, many of his previous difficulties have been removed. There are, nevertheless, facts connected with the ridges of stratified materials resting upon till, which he is unable to explain. He also states, that though he had for years inferred from the evidence of fossil shells sent to him from Canada by Capt. Bayfield, that the climate of North America, in the latitude of Quebec, was far more intensely cold at one period than it is n that his thoughts had been diverted from the consideration continued covering of snow on the Scottish mountains, by ledge that the climate of Great Britain during the sever epochs was warmer than it is at present. He is of opi during a period immediately antecedent to the existing, s cillations of temperature may have occurred in the north sphere.

---

* See Mr. Lyell's paper on the Norfolk Drift, Phil. Mag., May 1840, [Third Series, vol. xvi. p. 345,] and the Abstract of the paper in the Proceedings of the Society, vol. iii. p. 171.

Forfarshire, Mr. Lyell divides geologically into three districts: 1st, the Grampians, composed of granite, gne slate, and clay-slate, flanked by a lower range of vertic old red sandstone, associated with trap; 2ndly, the gre nal trough of Strathmore, occupied by the middle and ne bers of the old red sandstone; and 3rdly, the anticlinal the Sidlaw Hills, consisting of the inferior or grey beds c red sandstone, usually accompanied by trap. He furth that it represents, on a small scale, both geologically and the portion of Switzerland where erratic blocks are most the Grampians with their crystalline rocks being compar Alps, the secondary chain of the Sidlaw Hills to the Strathmore to the great valley of Switzerland; and that t blance is increased by the occurrence in Strathmore a Sidlaw Hills of angular and rounded blocks of Grampian

The superficial detritus of Forfarshire, Mr. Lyell di three deposits: 1st, the thin unstratified covering on the G derived from the disintegration of the subjacent strata, wi intermixture of pebbles traceable to rocks at a higher lev distant; 2ndly, the unstratified materials enclosing boul occur at the base of the hills on both sides of every gle due to taluses formed by landslips, but constituting t transported debris, with a nearly flat top, and sometime steep sides, one towards the river, and the other of less wards the mountain; and 3rdly, the stratified gravels, clays which overlie the unstratified detritus. Mr. Lye his observations principally to the second and third divisi

The terraces or lateral mounds very generally increase and depth as they descend from the higher to the lower gle ing in the latter sometimes a thickness of 100 feet, sionally so great a breadth as to leave only sufficient roo river to pass. The inferior part is always unstratified, co mud and sand, in which large angular and rounded fr rocks are imbedded. These boulders are more and mor as their distance increases from the hills whence they c been detached; but they are more frequently flat-sided th which have been rounded by water; and they become mo fied in character by the junction of every tributary gle upper part the mounds often consist of 40 to 80 feet o materials as the lower, but regularly stratified. Mr.

---

* See Proceedings, vol. iii. p. 119 [or L. & E. Phil. Mag. vol. x

proceeds to illustrate his subject by describing in detail the phænomena presented by the valley of the South Esk and those of its tributaries.

The South Esk springs from a shallow lake nearly 3000 feet above the level of the sea, and twenty miles from Strathmore. For six miles the river flows through a district composed partly of gneiss, traversed by veins of granite or eurite, and partly of granite. The fragments derived from this high region may be traced downwards continuously for twelve miles to Cortachie; and as a proof that the detritus forming the lateral mounds has followed the same downward course, Mr. Lyell states that it preserves throughout, as well in the main as in the lateral glens, an uniformly grey colour; while the detritus of the lower zone of mica-slate is invariably tinged red, this colour being also imparted to the debris of the still lower portions of the glens, notwithstanding the intermixture of pale brown materials obtained from the clay-slate of that district. Another proof of the detritus not having been drifted upwards, is the absence in the higher portions of the glens of the blocks of pure white quartz which abound in the region of mica-schist, and have been derived from the numerous veins and beds of quartz belonging to that formation. The chief exception to this arrangement is a boulder of conglomerate in the bed of the Proson, evidently derived from hills two miles to the south, but which are considerably above the level of the glen. A few other similar exceptions have been noticed, but the distances to which the stragglers have been traced are inconsiderable. The phænomena exhibited by the lateral mounds, Mr. Lyell states, agree well with the hypothesis of their being the lateral moraines of glaciers; and he adds, that he had never been able to reconcile these phænomena, particularly the want of stratification, with the theory of the accumulations of the detritus during submergence, and the removal by denudation of the central portions of a deposit which had by that means filled the glens. The distribution of an enormous mass of boulders on the southern side of Loch Brandy, and clearly derived from the precipices which overhang the Loch on the three other sides, is advanced as another proof in favour of the glacial theory. It is impossible to conjecture, Mr. Lyell says, how these blocks could have been transported half a mile over a deep lake; but let it be imagined that the Loch was once occupied by a glacier, and the difficulty is removed. Loch Whorral, about a mile to the east of Loch Brandy, is also surrounded on its north, east and western sides by precipices of gneiss, and presents on its southern an immense accumulation of boulders with other detritus, strewed over with angular blocks of gneiss, in some instances twenty feet in diameter. This moraine is several hundred yards wide, and exceeds twenty feet in depth, terminating at the borders of the plain of Clova in a multitude of hillocks and ridges much resembling in shape some terminal moraines examined by Mr. Lyell in Switzerland.

The great transverse barrier at Glenairn, where the valley of the South Esk contracts from a mile to half a mile in breadth, and is flanked by steep mountains, Mr. Lyell formerly regarded as very

difficult of explanation. Seen from below, this barrier res
artificial dam 200 feet high, with numerous hillocks on it
On the eastern side it appears to have been denuded to t
of about 300 yards by the Esk. Its breadth from nortl
is about half a mile. The lower part, 30 feet in depth,
in the river cliff, consists of impervious, unstratified mu
boulders; but the total vertical thickness of this deposit is
be from 50 to 80 feet; and the upper part of the barrie
posed of from 50 to 100 feet of very fine stratified materia
not possible, Mr. Lyell observes, to account for the accum
this barrier by the agency of water, particularly as no
joins the Esk at this point; but if the barrier be supposed
large terminal moraine of a receding glacier, then its
position, he says, are easily to be understood. M. Agas
work on glaciers, shows, that when these masses of ice en
row defile from a broader valley, the lateral moraines
towards the centre, and the mass of transported matter
more uniformly over the whole. Such a terminal moraine
receding glacier in a defile, Mr. Lyell states, would dam
waters of the glacier, and produce a lake; and the phæno
sented by the barrier of Glenairn, and the plain which e
its rear, are fully explicable on the assumption of their ha
produced by a glacier. The stratification of the upper
the barrier is also shown to be partly in accordance with
produced by the formation of ponds of water on the surfa
raines; but Mr. Lyell states, that the accumulation of s
capping of stratified materials is still the most obscure ch
the deposits under consideration.

At Cortachie, about four miles below the barrier of Gle
South Esk enters the country of old red sandstone, and a
a half lower it is joined by the Proson, and a mile yet lov
Carity. In the district in which these streams unite ther
siderable thickness of unstratified matter full of Grampian
and covered for the greater part with stratified gravel and
some cases the latter exhibit the diagonal laminæ commo
aqueous formations; and in others the strata are so conto
a perpendicular shaft might intersect the same beds three t
the latter instances the surface of the subjacent red bou
has not partaken of the movement by which the stratifi
was contorted; and in consequence Mr. Lyell ascribed t
when he first beheld it in 1839, to the lateral pressure
masses of drifted ice repeatedly stranding upon a shoal of
terials*. In the middle of the tract between the South Es
Proson is a dry valley, and to the south of this valley, nea
son, an excavation was made ten years ago, which exposed
contorted beds overtopped by others perfectly horizonta
been formed by tranquil deposition after the disturbance
previously deposited. The phænomena exhibited by the t

---

* See Proceedings, vol. iii. p. 178.

district, Mr. Lyell conceives, might be well accounted for by supposing the union of three or four large glaciers; but he considers it difficult to explain the accumulation of the overlying stratified materials, the top of which must be 600 feet above the level of the sea, and facing the Strath. In following out the narrow ridge which intervenes between the Proson and the Carity, during last October, in company with Dr. Buckland, the latter drew the author's attention to a spot half a mile south-west of the House of Pearsie, where the surface of a porphyry rock was polished, furrowed, and scratched. The quarrymen of Forfarshire also state as a general fact, that rocks of sufficient hardness, when first laid bare, are smooth, polished and scored; and Mr. Blackadder has found on the Sidlaw Hills large boulders of sandstone grooved and polished. Another general fact mentioned by Mr. Lyell is, that the unstratified boulder-clay becomes more and more impervious in the lower part of the Grampian glens, not in consequence of the influx of distinct materials, but in the author's opinion of the grinding down by the ice of the mud and other detritus.

Mr. Lyell then describes the phænomena of the second district, or Strathmore. Though this district may be considered as one great strath, yet it is divided into many longitudinal ridges and valleys. The former, sometimes 300 feet in height, are for the greater part parallel to the strike of the old red sandstone, and are generally covered to the depth of sixty or more feet with till and erratics, derived from the Grampians and the subjacent strata. This covering is so general, that the structure of the district can be detected only in the ravines through which the principal rivers pass. The till constitutes invariably the oldest part of the detritus. The boulders which it contains sometimes exceed three feet in diameter: on the north muir of Kerriemuir is a block of trap-rock, six feet by five feet, and near it is a mass of mica-schist, nine feet long by four feet wide and three high. The till has been ascertained by Mr. Blackadder to fill, in many places, deep hollows in the sandstone, which would become lakes or peat-mosses if the till were extracted. This distribution of the detritus, Mr. Lyell observes, may be explained on the supposition that, if the cold period came on slowly, the advance of the glaciers would push forward the detritus accumulated at their termination, and fill up, wholly or in part, the lakes or other cavities which they would encounter in their progress. Along most of the river courses, and in the lowest depressions of Strathmore, the till is covered by stratified sand and gravel.

One of the most remarkable peculiarities of the transported materials of Forfarshire and Perthshire is a continuous stream, from three to three and a half miles wide, of boulders and pebbles, traceable from near Dunkeld, by Coupar, to the south of Blairgowrie, then through the lowest part of Strathmore, and afterwards in a straight line through the lowest depression of the Sidlaw Hills from Forfar to Lunan Bay, a distance of thirty-four miles. No great river follows this course, but it is marked everywhere by lakes or ponds, which afford shell-marl, swamps, and peat-mosses, commonly surrounded by ridges of detritus from fifty to seventy feet hig sisting in the lower part of till and boulders, and in the u stratified gravel, sand, loam and clay, in some instances cu contorted. The form of the included spaces is sometime sometimes quadrangular. The finest examples are in the tract, which has the Dean for its southern boundary, and t from the bridge of Ruthven to the south of the grounds of L for its northern. The Grampian boulders are throughout the but there are associated with them masses of actinolite schist Mr. Blackadder has ascertained could be derived only from ley of the Tay. The fragments of secondary rocks belong formations of the districts in which they occur. Though the occupied by these marl-loch lakes is not traversed longitudin any river, yet it is so low, that if the transported matter w moved, a very slight depression would cause the sea to flo Lunan Bay by Forfar to Blairgowrie and Dunkeld. Mr therefore formerly conceived that an estuary might have e in that direction, and that the till might have been dri masses of ice floated from the Grampians and contiguous hill overlying ridges of sand and gravel he thought might hav bars formed one after the other, in the same manner as th sand and shingle, which now crosses the mouth of the Tay inland ridges of sand with boulders, which Mr. Lyell not Sweden, and certainly produced under the sea, confirmed this view. These Swedish ridges are from fifty to several h yards broad, but sometimes so narrow on the top as to lea more than room for a road; they are from fifty to a hund high, and they may be often traced in unbroken lines fo leagues, ranging north and south. In his account of these in a memoir published in the Philosophical Transactions Lyell states his belief that they were thrown down at the bo the Gulf of Bothnia, in lines parallel to the ancient coast, and the successive rise of the land. They usually consist of s sand and gravel, the layers being often at high inclinatio where they are composed of boulders, no stratification is obs After a long search, Mr. Lyell succeeded in finding shells in of marl belonging to a ridge in the suburbs of Upsala, about feet below the summit of the ridge, and eighty above the sea shells consisted of *Mytilus edulis*, *Cardium edule*, *Tellina* *Littorina littorea*, and *Turbo ulvæ*, the most common specie Baltic, and they constituted the greater part of the layer. summit of the ridge, at a short distance, he noticed angular of gneiss and granite, from nine to sixteen feet long, wh evidently been lodged when the ridge was submarine.

In Forfarshire Mr. Lyell never succeeded, as in the above Sweden, in finding marine shells in the ridges of sand; nor remember to have seen in Sweden transverse ridges at right to the north and south. The glacier theory, the author appears to offer a happy solution of the problem of the marl-loch gravels, the longitudinal banks being regarded as lateral and medial moraines, and the transverse ridges as terminal. The chief objections are the stratification of the upper part of the banks, and the necessity of assuming a glacier thirty-four miles in length, with a fall of only 300 or 400 feet of country.

---

* 1835, pp. 15, 16.

It has always appeared to Mr. Lyell and Mr. Blackadder remarkable, that the marl-loch gravels at Forfar are nearly 100 feet above the tract of till which separates them from the valley of South Esk, in Strathmore. In the present configuration of the country, water could not deposit the Forfar gravels without extending to the South Esk, the detritus of which is distinct, and separated by a low district of till without gravel. The only explanations of these phænomena Mr. Lyell considers to be either that the till is the moraine of a glacier, or that there has been a local change of relative levels of lands, by which the gravel of Forfar was uplifted, or the till to the northward depressed.

Another line of stratified detritus ranges at a higher level from the Loch of Lundie, along the Dichty Water, to the sea at Moray Firth, a distance of thirteen miles; and it is stated that many others might be enumerated. It is only on the coast to the east and west of Dundee, at heights varying from twenty to forty feet, that stratified clay and gravel have been found by Mr. Lyell to contain marine shells, all belonging to known existing species, except a *Nucula*. Although these remains prove a certain amount of upheaval subsequent to the deposition of the till, or to the commencement of the glacial epoch, including an equal movement in the interior, still Mr. Lyell objects to a general submergence of that part of Scotland, since the till and erratic blocks were conveyed to their present positions; as the stratified gravel is too partial and at too low a level to support such a theory; and he would rather account for the existence of the stratified deposits, by assuming that barriers of ice produced extensive lakes, the waters of which threw down ridges of stratified materials on the tops of the moraines. With respect to the geological age of the beds containing the marine shells, Mr. Lyell is of opinion that it is synchronous with that of the older of the recent formations on the Clyde, examined by Mr. Smith of Jordan Hill, and Mr. E. Forbes; and with respect to the age of the till and stratified gravel last formed, he is of opinion that it is very modern, because these accumulations constitute exclusively the dams of certain marl-lochs to the very bottom of the sediment formed, in which all the Testacea and skeletons of quadrupeds, as well as the remains of plants which have been found, are of existing species.

The third district, or that of the Sidlaw Hills, claimed Mr. Lyell's attention more particularly on account of the Grampian boulders with which it abounds. This range, whose greatest height is 1500 feet above the sea, is composed of anticlinal strata of grey sandstone, belonging to the old red sandstone, with associated trap. It is covered, as well as the whole of the country between Strathmore and the Tay, with the impervious till, containing Grampian bould fragments of the subjacent grey sandstone. The finest inst erratics observed by Mr. Lyell occur on Pitscanly Hill, 700 f the adjacent hill of Turin, 800 feet above the level of the sea. forty feet below the summit, on the southern side of the fo a block of mica-slate thirteen feet long, seven broad, and height above the ground. Four smaller and equally angular from three to six feet in diameter, lie close to its north end, vered from it. One of the nearest points at which this gneis *in situ*, is the Craig of Balloch, fifteen miles distant, on the extremity of the Creigh Hill, and between these points in the great valley of Strathmore and the hills of Finhaven. Grampian boulders, from three to six feet in diameter, occu hills between Lumley Den and Lundie, at the height of 10 and Mr. Blackadder has found fragments of mica-schist one diameter on the summit of Craigowl, the highest point of law Hills, and exceeding 1500 feet above the level of the se

In conclusion, Mr. Lyell offers some remarks on the co under which glaciers may have existed in Scotland, and th ences between them and those of the glaciers of Switzerla states that the glaciers of the latter country being situa further to the south, they can present but an imperfect analo permanent masses of ice in Forfarshire, and that it is to Georgia, Kerguelen's Land and Sandwich Land that we must the nearest approach to that state of things which must have in Scotland during the glacial epoch. In those regions of th ern hemisphere the ice reaches to the borders of the sea, temperature of summer and winter being nearly equalized, ciers probably remain almost stationary, like those of the winter, and can be diminished by only the first two of th causes which tend to check an indefinite accumulation of Switzerland; viz. 1st, evaporation without melting; and 2n descent of glaciers by gravitation, considered by M. Agass not very influential:—the third cause, the descent of glacier from alternate liquefaction and freezing, he conceives must be suspended in these regions.

As the tertiary strata prove that a warm climate certai ceded the assumed glacial epoch in the northern hemisphere a mild climate has since prevailed, Mr. Lyell says, there a distinct phases of action to be considered in studying the s glaciers of Scotland: 1st, the coming on of the epoch; 2nd, tinuance in full intensity; and 3rd, its gradual retreat. commencement of the first condition, only the higher mo would send down glaciers to be melted in the plains below, a sent in Switzerland, and in Chili between the 40th and 50th of latitude. The ice would therefore thus be constantly advanc retreating, but progressively, century by century, gaining in consequence of diminishing summer heat; and pushing its t moraines forward, it would fill up lakes and other inequali it finally reached the sea. During the second condition, w motion of the ice would be very small, there would be, Mr. Lyell states, vast accumulations of snow filling the plains and valleys to a great height, and leaving bare only the higher peaks and precipices of the mountains. From these points, he conceives the erratic blocks were detached and conveyed almost imperceptibly along the surface of the frozen snow to great distances. Lastly, at the breaking up and gradual retreat of the glaciers during the third period, he is of opinion, the boulders were deposited in the various situations in which they are now found, and that moraines, or lateral and transverse mounds, were successively deposited, and lakes formed, by which stratified materials were accumulated in certain positions.

The second part of Dr. Buckland's Memoir on the Evidence of Glaciers in Scotland and the North of England, was then read.

The first part of the Memoir concluded with an account of glacial phænomena in the neighbourhood of Edinburgh (see *antè*, p. 569); and the line of country more particularly described in this portion extends southward from Edinburgh by Berwick, Newcastle, the Cheviots, the lake districts of Cumberland and Westmoreland, Kendal and Lancaster, to Shap Fell. A large portion of the low lands between Edinburgh and Haddington is composed of till or unstratified glacier-mud containing pebbles. In the valley of the North Tyne, about one mile east of Haddington, is a longitudinal moraine midway between, and parallel to, the river and the high road; and Dr. Buckland directs attention to the trap-rocks which commence a little further eastward, and are intersected by the Tyne for four or five miles above Linton, as likely to present scored and striated surfaces, where the valley is most contracted. Four miles west of Dunbar another long and lofty ridge of gravel stretches along the right bank of the river; and for three miles to the south-east of Dunbar extends a series of terraces or modified lateral moraines. In the high valleys at the east extremity of the Lammermuir hills, between Cockburn's Path and Ayton, moraines dispersed in terraces are also visible at various heights on both sides of the river; and on the left margin of the estuary of the Tweed, three miles north of Berwick, round tumuli and oblong mounds of gravel are lodged on the slope of a hill 300 or 400 feet above the level of the sea.

Moraines in Northumberland.—On many parts of the coast of Northumberland, especially near Newcastle, deposits of till rest upon the carboniferous rocks. At the village of North Charlton, between Belford and Alnwick, Mr. C. Trevelyan pointed out to Dr. Buckland in 1821, a tortuous ridge of gravel which was supposed to be an inexplicable work of art; but which he became convinced, after an examination in 1838 of the upper glacier of Grindelwald and that of Rosenlaui, is a lateral moraine. Dr. Buckland was prevented from examining the gorges through which the Burns descend from the eastern extremity of the Cheviots, but he directs attention to them as points where striæ and other proofs of glacial action may be found. Immediately below the vomitories of the eastern valleys of the Cheviots, enormous moraines are stated to cover a tract four miles from north to south, and two from west to

east; and the high road to wind among cultivated mounds
from near Woller, through North and South Middleton,
West and East Lillburn to Rosedean and Wooperton. On
bank of the College Burn[*], immediately above the bridge a
newton, Dr. Buckland discovered last autumn a moraine thi
high, stratified near the top to the depth of a few feet, b
posed chiefly of unstratified gravel, inclosing fragmentary p
of a bed of laminated sand about three feet thick. Some o
fragments were in a vertical position, others were inclined,
laminæ of which they were composed, were, for the great
variously contorted. He is of opinion that these detached p
were severed from their original position, moved forward, a
torted by the pressure of a glacier, which descended the deep
of the College Burn from the northern summit of the Chevi

Evidence of Glaciers in the mountains of Cumberland an
moreland.—Proofs of glacial action, Dr. Buckland says, are a
dant throughout the lake districts and in the districts in fron
great vomitories through which the waters of the lakes are disc
as in Scotland and Northumberland. Thus, in the vicinity
rith, near the junction of the Eden with the Eamont and the L
are extensive moraines loaded with enormous blocks of porph
slate, brought down, Dr. Buckland observes, by glaciers,
descended from the high valleys on the east flanks of Helvell
in the mountains around Patterdale, into the lake of Ull
(considered to be then occupied by ice), and from the val
which the tributaries of the Lowther descend from the east
Martindale, from Haweswater and Mardale. A remarkabl
of these moraines is by the road side near Eden Hall four mi
of Penrith; and the detritus of moraines is stated to occ
greater part of the valley of the Eamont, from Ulleswate
junction with the Eden. On the southern frontier of these
tains in Westmoreland and Lancashire similar moraines occu
extensive scale. Thus, immediately below the gorge throug
the Kent descends from the mountains of Kentmere and Lon
dale, many hundred acres of the valley of Kendal are cover
large and lofty insulated piles of gravel; and smaller mora
their detritus, nearly fill the valley from Kendal to Morecom
Five miles north-east of Kendal, on the high road from Shap
shoulder of the mountain in front of the valley of Long Sledd
at an elevation of 500 feet, a group of moraines occupies ab
acres, and is distinguished from the adjacent slate rocks by a
fertility. On the south of Kendal, the high roads from Bur
Milnthorpe to Lancaster, pass for the greater part over mor
their detritus; and Lancaster Castle, placed in front of the v
of the Lune, is stated to stand on a mixed mass of glacial
probably derived from the valley of the Lune. The districts
ness, Ulverston, and Dalton are extensively covered with d
posits of glacier origin, derived from the mountains surroun

---

[*] For a notice by the late Mr. Cully, of a sudden flood in this di
1830, see Proceedings of the Geological Society, vol. i. p. 149.

upper ends of Windermere and Coniston lakes; and they contain a large admixture of clay, in consequence of the slaty nature of many of the mountains. A capping of till and gravel, thirty to forty feet thick, overlies the great vein of hæmatite near Ulverston. The numerous boulders upon the Isle of Walney also indicate the progress of the moraines from Windermere and Coniston to the north-west extremity of Morecombe Bay.

Dr. Buckland was prevented from personally examining, during his late tour, the south-west and west frontiers of the Cumberland mountains, but he conceives that many of the conical hillocks laid down on Fryer's large map of Cumberland, in the valley of the Duddon, at the south base of Harter Fell, are moraines; that some of the hillocks in the same map on the right of the Esk, at the east and west extremities of Muncaster Fell, are also moraines formed by a glacier which descended the west side of Sca Fell; and that many of the hillocks near the village of Wastdale were formed by moraines descending westward. Dr. Buckland is likewise convinced that moraines exist near Church in the Valley; also between Crummoch Water and Lorton, in the valley of the Cocker; and near Isle, in the valley by which the Derwent descends from Bassenthwaite lake towards Cockermouth, though there are no indications of them on Fryer's map.

Near the centre of the lake district are extensive medial moraines on the shoulder of the hill called the Braw Top, and formed by glaciers at the junction of the valley of the Greta with that of Derwent Water.

Dr. Buckland had no opportunity of seeking for polished and striated surfaces in the high mountain valleys of the lake district; but he found them on a recently exposed surface of greywacke in Dr. Arnold's garden at Fox Howe near Ambleside; likewise near the slate quarry at Rydal; and on newly bared rocks by the side of the road ascending from Grassmere to the Pass of Wythburn; he is also of opinion that many of the round and mammillated rocks at the bottom of the valley, leading from Helvellyn by the above localities to Windermere, owe their form to glacial action.

The remarkable assemblage of boulders of Criffle granite at Shalkbeck, between Carlisle and Cockermouth, Dr. Buckland conceives may have been transported across the Solway Frith on floating masses of ice, in the same manner as the Scandinavian blocks are supposed to have been conveyed across the Baltic to the plains of Northern Germany.

Dispersion of Shap Fell Granite by Ice.—The difficulties which had long attended every attempt to explain the phænomena of the distribution of the Shap Fell boulders, Dr. Buckland considers, are entirely removed by the application of the glacial theory. One of the principal of these difficulties has been to account for their dispersion by the action of water; northwards along the valley, descending from Shap Fell to Shap and Penrith; southwards in the direction of Kendal and Morecombe Bay; and eastward, over the high table-land of Stainmoor Forest, into the valley of the Tees, as far

as Darlington. A glacier descending northwards from th
would, on the contrary, carry with it, Dr. Buckland says, bl
the village of Shap, and strew them thickly over the space
they are now found; another, taking a southern course,
drop the boulders on the hills and valleys over which the r
scends by High Borough Bridge to Kendal; and a third gre
cier, proceeding eastwards betwixt Crosby, Ravensworth, and
would cross transversely the upper part of the valley of the
near Brough, and accumulate piles of ice against the opposite
ment until they overtopped its lowest depression in Sta
Forest, and disgorged their moraines into the valleys of the
and the Tees. There are abundant proofs, Dr. Buckland st
the existence of this glacier in large mud and boulder morai
the ascent of the gorge between Shap Fell and Birbeck Fell,
the furrows and striæ, as well as the mammillated forms
rocks at the portals of the gorge, particularly on the norther
In the physical structure of this neighbourhood, Dr. Bu
points out other conditions which would have facilitated th
mulation of glaciers, as the lofty mountains of Yardale Head
overtop Shap Fell on the north-west, and the still higher mo
to the west, whose snows must have nourished enormous gl
and he concludes by stating that Professor Agassiz, during a
pendent tour, arrived at similar conclusions respecting the m
which the Shap boulders were distributed.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 156.]

March 13, 1840.—The following communications were re
On the Regulator of the Clock-work for effecting uniform
ment of Equatoreals. By G. B. Airy, Esq. Astronomer Roy

The subject of this communication is a mathematical invest
of a mechanical problem of great importance in practical astr
The author remarks, that the accuracy given to a most delic
valuable species of observations, by the use of clock-work a
to equatoreals, is so great, and the importance of the applica
evident, that any investigation which assists in elucidating th
ciples on which such apparatus should be constructed, and esp
any which points out the nature of one important defect to
it may be liable, cannot but be regarded as interesting to th
tical astronomer and the instrument-maker.

After adverting to the different methods of giving motio
train of wheel-work to the polar axis of the equatoreal, whic
been adopted in the principal instruments hitherto erected,
Dorpat telescope, the Kœnigsberg heliometer, the Cambridg
toreal, &c., the author proceeds to consider the various
which have been put in practice for effecting the regulatio
the mountings constructed by Fraunhofer, the axis of the re
is vertical; it carries a horizontal cross-arm, to the extrem
which are attached springs nearly transverse in direction

cross-arm, carrying at the ends small weights. When the regulator is made to revolve with a certain velocity, the centrifugal force of the balls bends the springs till the balls just touch the inner surface of a drum which surrounds the regulator: the smallest additional velocity causes the balls to press against the drum and create a friction which immediately reduces the velocity; and the drum is made slightly conical, so that by raising or depressing it the velocity may be altered at pleasure. This construction not only partakes of the defects common to all the others, but is liable besides to this peculiar objection, that the determinate rate will depend most essentially on the strength of the springs, and will therefore depend on temperature and other varying causes. The other constructions (which were practically introduced by Mr. Sheepshanks) depend upon the same principle as that of the governor of the steam-engine. Two balls suspended from the upper part of a vertical axis, by rods of a certain length, are made to expand by the rotatory velocity of the axis; and this expansion, when it reaches a certain extent, is made to press a lever against some revolving part, and thereby to create a friction which immediately checks the velocity. In some cases the balls are suspended by rods from the extremities of a horizontal arm carried by the vertical axis. This construction, adopted in the south equatoreal of the Royal Observatory, may be considered analogous to Fraunhofer's, substituting for the springs the gravity of the balls;—a change which can hardly fail to be advantageous.

Now, the uniformity of rotatory motion of the spindle, in these various constructions, depends entirely on this assumption; that if, upon the whole, the retarding forces are equal to the accelerating forces, the revolving balls will move in a circle and in no other curve. But this assumption is not correct. If, for instance, we consider the case of balls, suspended as in the governor of the steam-engine; the motion of each of the balls may be the same (omitting the moments of inertia of the various parts of the machine, which are trifling) as that of a ball, suspended by a string, and put in motion by an arbitrary impulse; and a ball so suspended may move in a curve differing insensibly from an ellipse. Now this elliptic motion actually takes place. In some instances observed by the author, the balls of the regulator, instead of revolving in a circle, revolved in an ellipse of considerable excentricity, and the rotatory motion of the spindle was therefore exceedingly variable. The effect of this irregularity on the motion of the equatoreal, whether the inequalities of motion are followed by the polar axis, or merely communicate a general tremor to the frame, must be injurious.

The inequality now mentioned is only one case of a very extensive theorem, which may be thus enunciated:—"Whenever the equilibrium of forces requires that a free body be brought to a determinate position, either absolute or relative to other parts of the mechanism with which it may be connected, the body will not remain steadily in that position of equilibrium, but will oscillate on both sides of that position, and (so far as the action of those forces affect it) will have no tendency to settle itself in the position of equilibrium." This theorem supposes that some cause of disturbance has once put the body into a state of oscillation; and renders it necessary to take account of such oscillation in planning any mechanism which depends upon assuming the position of equilibrium to be nearly preserved.

If we examine the theory of the regulator, we shall see that the friction which checks the motion takes place when the balls are most distant from the axis, and (as the equable description of areas is nearly observed) this occurs when the angular motion is least. The whole maintaining force acts without check when the balls are nearest to the axis, that is, when the angular motion is greatest. Therefore, when the angular motion is least, the acting forces tend still to diminish it; when greatest, they tend to increase it. Hence the inequalities of angular motion will increase till some new forces come into play, which act in some different manner: and thus is explained the obstinate adherence of the governor balls in some cases to their elliptic motion.

The author next proceeds to consider the ways in which an attempt may be made to counteract the injurious effects of such oscillations. These appear to be only two; one, to make the oscillations of velocity much slower (or to make their periodic time longer); the other, to make the oscillations quicker (or to make their periodic time shorter). The first of these methods has the effect of giving greater smoothness to the motion (an object of great importance); and it is the principle which was adopted with success in the clock-work of the Cambridge equatoreal. The second method endangers the smoothness of the motion; but, as the error has but a short time for accumulation, it ensures that the object shall remain steady under the wire of the telescope far more completely than the first. The construction attached to the clock-work of the south equatoreal of the Royal Observatory is on this principle; and it appears to answer extremely well.

The mathematical problem proposed by the author in the present communication is an investigation into the motion of governor balls, for the purpose of deducing the time of rotation corresponding to a given expansion of the balls, and the periodic time of their oscillations, and the consequent oscillations in the angular speed of the spindle; and the subject is discussed on four different suppositions, which, with their several principal results, are as follows: 1. When the balls are supposed to be acted upon by no force. The result is, that the periodic time of oscillation is somewhat greater than half the time of rotation. 2. When the axis which carries the balls has a fly-wheel attached to it. In this case the periodic time of the oscillations cannot be less than half the time of rotation, and may be in any proportion greater. 3. When the balls are suspended by rods from a horizontal arm carried by the regulator-spindle. The result is, that the periodic time of the oscillations may be made small in any proportion to the time of rotation. 4. On an assumed

w of accelerating force and retarding friction. The result is, that
he effect of these forces is to increase continually the inequality
f motion.

Note on an Arabic Globe belonging to the Society. By R. W.
lothman, Esq. M.A., Foreign Secretary.

The instrument in question is a small bronze globe, about six
ches in diameter, brought some time ago from the East, having
he positions of the principal stars marked by silver studs, with
heir Arabic names engraved; and the object of the present note is
o point out the differences between the names of the stars as found
n the globe, and those given in the Catalogue of Ulugh Beg, with
hich, in general, the globe agrees, though in some instances the
ifferences are worthy of notice. From the position of the colour,
c. it is inferred that the globe is not of ancient date; but it bears
o mark indicative of the precise period of its construction.

Elements of Galle's Second Comet; computed by M. Petersen,
nd communicated by Professor Schumacher.

April 10.—The following communications were read:—

Observations made at the Cape of Good Hope, in the year 1838,
ith Bradley's Zenith Sector, for the Verification of the Abbé De
acaille's Arc of the Meridian. By Thomas Maclear, Esq. F.R.S.
ommunicated by the Lords Commissioners of the Admiralty*.

The present paper is the second which has been received from
Ir. Maclear on the subject of the important and interesting opera-
ons now going on at the Cape, relative to the measurement of an
rc of the meridian.

In the former communication, an account of which is given in the
Ionthly Notice for April 1839 (Phil. Mag. Third Series, vol. xiv.
. 522), Mr. Maclear detailed his proceedings for the purpose of
lentifying the terminal station of Lacaille's arc; the present con-
ins the sector observations, with their reduction, and the deter-
ination of the amplitude of the arc.

The sector was delivered to Mr. Maclear in Table Bay, by Cap-
in Maitland, of H. M. ship Wellesley, on the 9th of December,
837, and was conveyed the same day to the Royal Observatory
nder his personal superintendence; and on the 10th the instru-
ent was put together and erected in the centre room. As this
oom was originally constructed for a zenith-tube of limited dimen-
ons, it became necessary to enlarge the apertures by sawing
rough a portion of the iron bars of the grating forming the floor
the lantern, and of the rafters above. In this tedious operation
early a month was consumed; but, in the meanwhile, a tent and
tripod for the support of the sector were prepared, a list of stars
lected, and a variety of details settled, in which the author states
ir J. Herschel cordially assisted with his advice.

The site of Lacaille's Observatory in Cape Town being covered
a large building, erected since 1752, the sector could not be
aced exactly over Lacaille's station; it was accordingly raised in
e court-yard of the house, under a tent, and every disposition

---

* [See Phil. Mag. Third Series, vol. xvi. p. 594.]

made, which the confined locality admitted of, to secure the canvass against the effects of the wind. The instrument was erected and adjusted on the 29th of January, and the observations commenced the same evening. They were carried on until the 19th of February, but under very disadvantageous circumstances, principally from the violence of the north-east wind acting on the unsettled canvass, and the showers of sand carried into the tent from the street. This series of observations proved to be unsatisfactory, and was not used for the determination of the amplitude.

As one of the objects for which the present observations were undertaken was to determine the influence of Table Mountain on the direction of the plumb-line, the sector was next transferred to a station close up towards the precipitous front of the mountain, on its north side, and about 1000 feet above the level of the sea. Previous, however, to the commencement of the observations at this station, Mr. Maclear removed the bisecting wires, which had been found too thick for several of the stars employed, and substituted cobweb. The observations began on the 24th of February, and were continued till the 13th of March, when the sector was dismounted and carried, as before, by coolies to the office of the Engineer Department in Cape Town.

The next step in the proceedings was to transport the sector to Klyp Fonteyn, the northern extremity of the arc. The party arrived at the station on the 24th of March, and immediately prepared to erect the sector on the corn-floor described in the former Notice as Jacobus Cotsee's foundation, which is situated at, and rather within, the south extremity of the ruin supposed by Captain Everest to be the granary of Lacaille. Before the instrument was set up, the several pieces forming the bearing and upper adjustments of the tube were separated, and carefully cleared from sand and dust. A tent having been raised, and fixed to iron pins driven into the floor, the tube was placed on its bearings, and the two barometers suspended from the sector tripod. At the distance of twenty-one feet exactly, and due east of the sector axis, a nail was driven into the floor, on which the axis of the repeating circle was placed.

The observations for zenith distances began on the 28th of March, and were continued to the 21st of April, a sufficient number having then been made for settling the question of the amplitude. Before leaving the station plans were made of the place, and of the foundations which had been discovered by Lieutenant Williams and the sappers; a base-line was measured, and the nature of the country to the north of the station examined. It was on the 6th of April that the foundation was discovered, whose dimensions correspond in some measure to the description given by Lacaille in his Journal of the granary he had occupied. The sector having been taken down, and a bottle containing an inscription deposited in a hollow chiseled into the solid rock about three feet below the surface, to mark the spot over which it had been erected, the party quitted Klyp Fonteyn on the 25th of April, and arrived at the Observatory on the 1st of May.

A cursory comparison of the observations having shown that those made at the southern station did not deserve the confidence required in a work of this kind, where the length of a few feet is a quantity of importance, and experience having proved that good observations could not be obtained under a tent in Mrs. De Witt's yard, it was resolved to look out for some solid building close to the station, where the sector could have fair play. Mr. Maclear fixed upon the Raggebay Guardhouse, and the necessary permission having been obtained, a hole was made in the roof, the floor taken up and sunk to the requisite depth, and the sector erected on the 7th of May, one week after the return of the party from Klyp Fonteyn.

From the state of the weather, no observations could be obtained until the 12th, and frequent interruptions afterwards occurred from the same cause; so that six weeks were expended on a a work which,i n favourable weather, might have been accomplished in two.

The sector was dismounted on the 30th of June, and carried back to the Observatory, where it was again set up in the sector-room on the 2nd of July. On examination, it was found to be as perfect as when first received, without the slightest mark of injury.

The author next proceeds to give the reduction of the observations. The barometers employed at Klyp Fonteyn were made by Mr. Thomas Jones, and from a comparison of their results with the Journal kept at the Royal Observatory, it appears that the station at Klyp Fonteyn is 485 feet above the mean level of the sea*. The station in the Guardhouse is close to the sea beach; the feet of the sector tripod could not be more than two or three feet above high-water mark. The chronometer employed at both stations was by Arnold, and beats half-seconds. Its performance at Klyp Fonteyn was good; at the Guardhouse, the reverse. At the former place, its rate was actually obtained by altitudes near the prime vertical; in Cape Town, by means of journeymen pocket-chronometers, carried to and from the transit-clock at the Observatory.

The collimation of the middle wire was deducted from the successive transits of stars in the alternate position of the limb, east and west.

The corrections for aberration, precessions, and nutation were calculated by means of the constant in the Royal Astronomical Society's Catalogue, recomputed for 1838.

The number of stars observed was 40; of which 20 were to the north, and 20 to the south of the zenith at the Cape. The number of observations at Klyp Fonteyn is 464, and at the Guardhouse 669; in all, 1133. It may therefore be supposed that errors of observation are reduced to nothing.

The final results are deduced as follows:—The amplitude being found from the mean of the reduced zenith distances of each star observed at both stations, and each result having a *weight* assigned to it equal to the product obtained by multiplying the least number of observations of the star in question at one station, and in one position of the sector, into the least number for the same star at the other station, the resulting mean amplitude from the stars north of the zenith is 1° 13′ 1″·173, and from the stars south of the zenith, 1° 13′ 14″·961.

---

* In the former communication the height of the station is stated to be nearly 400 feet.—Monthly Notices, vol. iv. p. 194. [Phil. Mag. S. 3, vol. xiv. p. 528.]

By assigning to the amplitude found from each star a weight equal to the quotient of the square of the number of observations of the star by twice the sum of the squares of the errors at both stations, the group to the north of the zenith gives 1° 13′ 14″·173, and the group to the south 1° 13′ 14″·953. The results of the two methods of computation may be regarded as identical; and the stars north of the zenith give the amplitude *less* by 0″·78 than the stars south of the zenith.

It is not easy to assign the cause of this discrepancy. Mr. Maclear inclines to ascribe it to the probable expansion of the tube at Klyp Fonteyn from the high temperature. While the observations were made the thermometer was sometimes as high as 93°, while at the Guardhouse the range was between 57° and 63°. Lacaille (*Mém de l'Acad.* 1752) states that his southern group of stars gave the amplitude greater by 0″·8 than the northern. This near coincidence with Mr. Maclear's result is remarkable.

If the expansion of the tube be the cause of the discrepancy, the mean between the north and south groups is as correct as if no expansion took place. This mean is 1° 13′ 14″·56, with a probable error not exceeding 0″·03.

The axis of the sector on the corn-floor at Klyp Fonteyn was 216 feet (reduced to the meridian) *south* of the centre of the foundation discovered on the 6th of April, and supposed to be Lacaille's sector station. The axis of the sector in the Guardhouse was 45 feet on the meridian *north* of Lacaille's sector station in Mrs. De Witt's yard. Now 261 feet = 2″·56; which added to 1° 13′ 14″·56 gives 1° 13′ 17″·12 for the amplitude of Lacaille's arc. Lacaille's value is 1° 13′ 17″·33*.

The author remarks, in conclusion, "that although this work does not clear up the anomaly of Lacaille's arc, it redounds to the credit of that justly distinguished astronomer, that, with his means, and in his day, his result from 15 stars is almost identical with that from 1133 observations on 40 stars, made with a powerful and celebrated instrument. Our field of inquiry is now limited to the terrestrial measure, which every friend to science must wish to see undertaken without delay, as a portion of a greater arc to extend so far as to neutralise local attractions, and leave no doubt upon the true curvature of this portion of the southern hemisphere."

The Longitude of Madras, computed from Moon-culminating Observations. By Edward Riddle, Esq.

Mr. Riddle having undertaken, at the request of Mr. Baily, to compute the longitude of the Madras Observatory from a mass of corresponding moon-culminating observations made there and at Greenwich, Cambridge, and Edinburgh, in 1834, 5, 6, and 7—observations which have not before been used for the purpose,—has availed himself of the occasion to enter at considerable length into the practical details of the method of computing the longitude; and has given all the requisite formulæ, with examples of their application from the observations under discussion.

---

* In the *Fundamenta Astronomiæ* (page 184), Lacaille states that the amplitude deduced from the observations corrected for errors subsequently discovered in the divisions of the sector, and recomputed, is 1° 13′ 17″·5.

The general result of the computations, of which a further account appears in the Monthly Notices for April, is that the longitude of Madras from Greenwich is,

h m s
5 20 54·9 by 54 observations at Greenwich and Madras
53·9 .. 56 „ „ Cambridge and Madras
58·0 .. 65 „ „ Edinburgh and Madras.

Ephemeris and Elements of the third Comet discovered by Galle. By Mr. Rumker, Superintendent of the Observatory at Hamburg. Communicated by Dr. Lee.

Approximate elements of the same comet have been received from Professor Schumacher.

It has been remarked that these elements resemble those of a comet observed in China in 1097, and computed by Burckhardt; and of one which appeared in 1468, of which observations are recorded by Pingré. Supposing the three appearances to have been of the same comet, the periodic time is thus about 371 years.

On the Present State of our Knowledge of the Parallax of the Fixed Stars. By the Rev. R. Main.

This paper was in part read.

May 8.—Among the presents announced at this meeting, was a 7-feet Newtonian Reflecting Telescope, the work of the late Sir William Herschel, and presented by him to his sister, Miss Caroline Herschel; in whose name, and that of the President, it was now presented to the Society.

The reading of Mr. Main's paper on the Present State of our Knowledge of the Parallax of the Fixed Stars, was resumed and concluded.

This memoir was read to the Council of the Society at their meeting in January of the present year; the object of it being a review of the parallax of the *Cygni*, recently obtained by Professor Bessel. In presenting it as a memoir, to be read before the Society, the author determined to allow it to remain in its original form of a report addressed to the Council; feeling that, if it were given in any other shape, his discussion of the results of eminent contemporaneous astronomers on the subject of annual parallax might seem presumptuous. He recommends to the notice of astronomers a very complete historical summary of astronomy, as connected with this subject, by Fockins (which work was printed at Leyden in 1835), entitled *Commentatio Astronomica de Annuali Stellarum Parallaxi*, which, he remarks, very materially assisted him in the prosecution of the historical part of his work. The author proposes the following divisions of his report:—

1. Abstracts of theoretical papers which have appeared on the subject of annual parallax.

2. A statement of the results of observations which have been made since the time of Bradley for the purpose of detecting parallax.

3. A review of the results of contemporaneous astronomers on this subject.

4. A discussion of Bessel's observations and results.

Under the first division, he gives abstracts of the following papers:—

A Memoir by Clairaut.—Mémoires de l'Académie, 1739, p. 358.

A Memoir by Sir W. Herschel.—Phil. Trans. 1782, lxxii. P. I. p. 82.

A Memoir by Schubert.—Berliner Astronomisches Jahrbuch, 1796, p. 113.

A Memoir by Sir J. Herschel on the Detection of Parallax by the Variation of the Angle of Position of Double Stars.—Phil. Trans, 1826, P. II. p. 266; and 1827, P. I. p. 126.

A Memoir by Struve, forming one of the introductory chapters to his large work on double stars.

A discussion by Bessel of Bradley's Observations of Right Ascensions of Stars differing by nearly 12^h.—Fundamenta Astronomiæ, Introduction.

Under the second head he gives, briefly, the results obtained by Piazzi, Calendrelli, Brinkley, Pond, Bessel, and Struve, referring to the following works:

For Piazzi and Calendrelli:—

Memoirs of the Italian Society, vol. xii.

Calendrelli's Opusculi Astronomichi, vol. for 1806.

Zach's Monthly Correspondence, vols. xviii. and xix.

For Brinkley and Pond:—

Phil. Trans., vols. for 1810; 1817; 1818, P. II.; 1821, P. II.; 1823, P. I.; 1824, P. I. and II.

Memoirs of the Astronomical Society, vol. i. P. II.

Transactions of the Royal Irish Academy for 1815, vol. xii.

The Introduction to the Konigsberg Observations for the year 1816.

The Dorpat Observations, vols. i., ii., and iii.

Under the third head, the following memoirs on the subject are considered:—

On the Parallax of α *Aquilæ*, by Mr. Henry Taylor, Astronomer to the East India Company at Madras.

On the Parallax of α *Lyræ*, by the Astronomer Royal.—Mem. Ast. Society, vol. x.

On the Parallax of α^1 and α^2 *Centauri*, by Professor Henderson, Mem. Ast. Society, vol. xi.

On the Parallax of Sirius, by Professor Henderson*. Not yet printed in the Society's Memoirs.

[* An abstract of Prof. Henderson's paper will be found in Phil. Mag. Third Series, vol. xvi. p. 148.]

Of these memoirs, it is sufficient to say that Mr. Henderson's parallax of *α Centauri* is discussed at some length; and the result is, that there seems a strong probability of a sensible parallax in this remarkable star, which is strongly recommended to the attention of southern astronomers.

Lastly, in discussing Bessel's parallax of 61 *Cygni*, the author proceeds as follows*:—

That the indication of a parallax in the agreement between the fourth and fifth columns of the preceding tables may be rendered more evident, a graphical projection of them is added. The time being set off by proportional spaces on the line of the abscissæ, inclined lines are drawn through the origin (Jan. 1, 1838), whose ordinates represent the effects of Bessel's correction of the proper motion with contrary signs. From these lines the above differences are set off in the direction of ordinates to the abscissa. The curve, therefore, which passes through the extremities of these ordinates, represents the periodical effect of parallax; and accompanying curves being given, exhibiting the true effect of Bessel's assumed value of the constant, the agreement between the two is shown to be most complete. In the case of measures of distance from star (*a*), the maximum and minimum and vanishing ordinates of the curve are shown with almost as much regularity by the observed differences as by the assumed parallax; and in the second case, though the agreement is not so close, yet the general law of the curve of sines is well preserved.

In arguing on the evidence afforded by the foregoing tables and graphical illustrations, the author concludes, not only that a real parallax has been detected, but that its amount is very approximately given in Bessel's investigation, who is enabled, by repeating the same process, to diminish at pleasure the residual errors of the determination. This feature separates completely this from all former attempts, in some few of which an amount, rather greater than the limiting probable errors, would seem to announce a parallax, of which the evidence is yet so slight as to leave the mind quite unsatisfied of its existence; while the uncertainty of its amount (supposing its existence to be proved) prevents its application to ulterior objects in sidereal astronomy.

To the memoir are annexed two appendices, the first of which contains the investigation of formulæ for computing the coefficients of the constant of parallax in the two cases; and also for finding the variation in the angle of position of two stars very near each other, one of which is affected by parallax.

The second contains a translation of the most important parts of Bessel's description of his Heliometer, from the *Astronomische Nachrichten*, vol. viii. No. 189.

An extract was read from a letter from Professor Bessel to the President, stating that the observation on 61 *Cygni* had been continued through the last year to the end of March 1840, and that the most probable values of the parallax resulting from the measured distances of the double star from each of the two stars of comparison, (*a*) and (*b*), are as follows:—

---

* Bessel's letter is contained in vol. iv. No. 17, of the Notices of the Astronomical Society [and in Phil. Mag., Third Series, vol. xiv. p. 68.].

188 obs. $61-a \ldots 0''{\cdot}3584-0''{\cdot}0756\,k$; weight, 64·66
214 .. $61-b \ldots 0\;{\cdot}3289-0\;{\cdot}0276\,k$; ... 78·89

(*k* being a small indeterminate correction depending on the effects of temperature on the micrometer-screw).

The sums of the squares of the errors cannot, on the supposition of a vanishing yearly parallax, be made less than —

$$61-a \ldots 12{\cdot}7282-3{\cdot}2445\,k+0{\cdot}6330\,k^2$$
$$61-b \ldots 15{\cdot}6507-1{\cdot}6094\,k+1{\cdot}7029\,k^2;$$

but on assuming the annual parallax to have the value before assigned to it, they become —

$$61-a \,.\,.\, 4{\cdot}4245+0{\cdot}2579k \quad 0{\cdot}2627k^2=4{\cdot}3614+0{\cdot}2637(k+0{\cdot}489)^2$$
$$61-b \,.\,.\, 7{\cdot}1171-0{\cdot}1768k+1{\cdot}6426k^2=7{\cdot}1123+1{\cdot}6426(k-0{\cdot}0{\cdot}54)^2$$

On deducing the value of *k* from the observations, those of the first star give, therefore, $k=-0{\cdot}489$; and those of the second $k=0{\cdot}054$. M. Bessel shows that the last of these values is more deserving of confidence than the first; nevertheless, the true value of the correction is still very doubtful, and it is accordingly left indeterminate.

Assuming the relative parallax of (*a*) and (*b*) to be equal, and having due regard to the mean error of both results, the most probable value of the annual parallax is found

$$0''{\cdot}3483-0''{\cdot}0533\;k$$

and its mean error $\pm 0\;{\cdot}0141$.

This result is greater by 0″·0347 than was found from the first series of observations.

Assuming $k=0$, it results from this determination, that the distance of the star 61 *Cygni* from the sun is 592,200 times the mean radius of the earth's orbit—a distance which light would require 9¼ years to pass through.

He divides the two sets of observations of the distance of 61 *Cygni* from the comparison stars (*a*) and (*b*), into separate groups, monthly and half-monthly, and takes the mean of the measures in each group as corresponding to the mean of the days in that group. These means are compared with Bessel's mean distances, which he has derived from the solution of his equations, and the differences tabulated. The effect of Bessel's assumed parallax is then computed for each mean day, and the resulting numbers placed in an adjacent column. The agreement between the computed effect of parallax and the above-mentioned differences is remarkably close, especially for the measures of distance of the star (*a*), in which the solution of the equations shows the error of the assumed proper motion to be very small indeed.

The following are the tabulated monthly groups:—

Results from the Measures of Distances of 61 *Cygni from Star* (*a*).

Mean day.	Mean of each group.	Bessel's mean distance.	Differences.	Effect of a parallax of 0″·369.
1837.				
August.... 23.	461″·406	461″·609	+0″·197	+0″·212
September 14.	·709	...	+ ·100	+ ·100
October .. 12.	·649	...	+ ·040	— ·057
November 22.	·395	...	— ·214	— ·258
December 21.	·287	...	— ·322	— ·317
1838.		...		
January .. 14.	·233	...	— ·376	— ·318
February 5.	·386	...	— ·223	— ·264
May...... 14.	·854	...	+ ·245	+ ·238
June...... 19.	·969	...	+ ·360	+ ·332
July ...... 13.	·825	...	+ ·216	+ .332
August.... 19.	·760	...	+ ·151	+ ·227
September 19.	·649	...	+0·040	+0·073

Results from the Measures of Star (*b*).

Mean day.	Mean of each group.	Bessel's mean distances.	Differences.	Effect of a parallax of 0″·261.
1837.				
August.... 22.	706″·504	706″·291	+0″·213	+0″·133
September 15.	·479	...	+ ·188	+ ·196
October .. 12.	·419	...	+ ·128	+ ·227
November 22.	·186	...	— ·105	+ ·187
December 20.	·283	...	— ·008	+ ·100
1838.		...		
January .. 11.	·177	...	— ·114	+ ·014
February.. 9.	·084	...	— ·207	— ·100
March .... 12.	705·900	...	— ·391	— ·196
May ...... 13.	706·206	...	— ·985	— ·209
June...... 20.	·414	...	+ ·123	— ·102
July ...... 21.	·446	...	— ·155	+ ·016
August .. 22.	·588	...	+ ·297	+ ·133
September 5.	·498	...	+ ·207	+ ·175
22.	·712	...	+0·421	+0·212

June 12.—The following papers were read :—

Continuation of the Investigation for the correction of the Elements of the Orbit of Venus. By Mr. Glashier, of the Royal Observatory, Greenwich.

This paper is a continuation of Mr. Main's investigations, communicated to the Society in June 1837 and April 1838, and printed in vols. x. and xi. of the Memoirs. Mr. Main corrected the orbit from Mr. Airy's observations of 1833–36. Mr. Glashier recomputed the observations of 1836 (for errors of assumed semi-diameter), and computed the observations of 1837 and 1838, and gives corrections of the elements of the orbit for all the years: then, substituting the observations, he deduces the residual errors (as compared with Lindenau's tables), which he finds not so small as might have been expected from so fine a series of observations. The author, however, feels confident that the results are correctly derived, from the pains taken to ensure accuracy: and the whole is given in a detailed shape, in order that any suspected error may be more readily detected.

An Account of some Experiments made with an Invariable Pendulum, at the Cape of Good Hope. By T. Maclear, Esq.

When Mr. Maclear was appointed Astronomer at the Cape of Good Hope, he was desirous of repeating the experiments there with an invariable pendulum; but it was some time before he had sufficient leisure to prosecute this measure. The pendulum had been previously swung in London by Mr. Baily, and also after its return to this country. The method of proceeding in such cases is so similar, and has been so often described, that it is unnecessary to enter further on that part of the subject. The result of these experiments shows, that, on the assumption that the pendulum made 86400 vibrations in London, in a mean solar day, at the temperature of 62° *in vacuo*, and at the mean level of the sea, it made only 86332·92 vibrations, under the same circumstances, at the Cape of Good Hope; which is almost identical with the experiments of Mr. Fellows, and differing very little from the experiments of Captain Foster and Freycinet. A new pendulum, consisting of a thick brass bar, without any bob, and furnished with *four* knife edges, is about to be forwarded to Mr. Maclear, which he proposes to swing at the principal stations of the triangulation that is now carrying on in that colony.

An Account of some Experiments made with three Invariable Pendulums, by Lieut. Murphy, R.E., during the late Expedition down the Euphrates. By Mr. Baily.

When Colonel Chesney undertook the expedition down the Euphrates, three invariable pendulums were placed under his care, for the purpose of their being swung at positions more *inland* than had been hitherto practised. Two of these pendulums (iron and copper) belonged to this Society, and the other (brass) to the Admiralty; and they are the same that were taken out by the late lamented Captain Foster. They had been previously swung in this country, before the expedition just mentioned, by Mr. Baily, and also subsequent thereto. Only two places presented favourable opportunities for swinging these pendulums during the expedition: the first, at Port William, near Bir, on the Euphrates; and the other, at Bussora. The experiments were made by Lieutenant Murphy, and were conducted with his usual caution and ability: the details are recorded in printed skeleton forms, with which he was furnished previous to his departure; but none of the computations were made till after Lieutenant Murphy's decease. The reductions have since been made by Mr. Baily, on the same data as those already mentioned in the seventeenth volume of the Memoirs of this Society. On the assumption that each of these pendulums made 86400 vibrations in a mean solar day in London, at the temperature of 62° *in vacuo*, and at the mean level of the sea, it is found that they respectively made the following number of vibrations at the two stations above-mentioned, viz.

Pendulums.	Port William.	Bussora.
Brass	86340·66	86318·98
Copper	86341·30	86317·26
Iron	86338·96	86317·66
Mean =	86340·31	86318·26

The Elements of the Annular Eclipse of the Sun that will happen on October 8, 1847. By Mr. George Innes.

These elements have been deduced from Carlini's "Solar Tables" (1832), and Burckhardt's "Lunar Tables" (1812). The time of apparent conjunction in right ascension at Greenwich will be Oct. 8^{d} 19^{h} 28^{m} 36^{s}·593; at which time the sun's apparent semidiameter will be 16′ 2″·529, and the moon's augmented semidiameter, 14′ 43″·968;

Greenwich mean time.	Sun's right ascension.	True right ascension.	Declination south.	Equation of time.	Semi-diameter.
18 hours	195° 19′ 31″·390	194° 6′ 41″·844	62° 20″·693	12′ 29″·507	16′ 2″·512
19 do.	195 21 59·783	194 8 59·511	63 17·975	12 30·245	16 2·524
20 do.	195 24 28·183	194 11 17·180	64 15·247	12 30·930	16 2·536
21 do.	195 26 56·616	194 13 34·914	65 12·525	12 31·597	16 2·546
21^{h} 6^{m} 37^{s}·755......	195 27 13·036	194 13 40·125	65 18·861	12 31·670	16 2·549
22 hours	195 29 25·054	194 15 52·614	66 9·797	12 32·271	16 2·566

Greenwich mean time.	Moon's true longitude.	Longitude north.	Equatorial horizontal parallax.	Horizontal semidiameter.	True right ascension in arc.	Declination south.
18 hours	193° 54′ 30″·843	22′ 49″·641	53° 54′·130″	14′ 41″·300	192° 57′ 44″·323	5° 8′ 47″·322
19 do.	194 24 59·768	25 32·833	53 54·197	14 41·316	193 26 45·329	5 17 42·208
20 do.	194 54 28·771	28 16·446	53 54·214	14 41·320	193 64 27·307	5 26 35·565
21 do.	195 23 57·790	30 59·751	53 54·325	14 41·351	194 22 50·127	5 35 27·710
21^{h} 6^{m} 37^{s}·755	195 27 13·036	31 17·610	53 54·326	14 41·351	194 25 55·127	5 40 34·132
22 hours	195 54 26·993	33 42·861	53 54·409	14 41·428	194 51 13·894	5 44 18·646

Obliquity of the Ecliptic	23° 27′ 2″·514
Sun's semi-diameter	16 2·529
——— horizontal parallax ...	8·5925
——— latitude	+ 0·179

On the Comparison of the Neapolitan Standard Yard with the Standard Yard of this Society. By Mr. Simms.

The Neapolitan Government having directed Mr. Simms to construct a new standard scale, on a principle similar to that which was made for this Society, the object of the present communication was to place on record the results of the comparisons that were made with the centre yards of each scale. It appears that 215 comparisons were made on nine several days by three different persons; and the mean of the whole showed that the centre yard of the Neapolitan scale was longer than the standard yard of the Society's scale, by ·002680 of an inch: the greatest differences from the mean being +·000520 and −·000365 of an inch. This new scale is marked No. 6; it being the sixth scale of this construction that has been made.

On the Difference of Longitude between the Observatories of Madras and the Cape of Good Hope, deduced from Moon-culminating stars. By T. Maclear, Esq.

The observations extend from February 19, 1834, to October 10, 1837, both inclusive, and contain all the corresponding observations within that period, except two which appear to be erroneous; and amount to seventy in number. Of these, only three were of the second limb. The result of the whole shows that the difference of longitude is $4^{h}\ 7^{m}\ 1^{s}{\cdot}56$, with a probable error of $\pm\ 0^{s}{\cdot}53$.

LXXXII. *Intelligence and Miscellaneous Articles.*

ON THE LIMIT TO THE ACTION OF CERTAIN CHEMICAL REAGENTS.

M. P. HARTING having examined the action of certain chemical reagents, has given the following statement as the result.

1. The sensibility of *starch* as a reagent for *iodine*.

Iodide of potassium slightly acidulated with nitro-muriatic acid, tested by a diluted solution of starch, gave the following results.

1. Containing 1-500dth iodine gave a black precipitate, the upper surface was brownish yellow.

2. Containing 1-1000dth iodine gave nearly the same colour.

3. Containing 1-2000dth iodine gave the precipitate the same colour, but the solution slightly coloured.

4. Containing 1-3000dth iodine, precipitate bluish black, the solution nearly clear.

5 and 6. Containing 1-4000dth to 1-5000dth iodine, precipitate bluish black, the solution quite clear.

7 to 11. Containing 1-10,000dth to 1-40,000dth iodine, very dark blue.

12 and 13. Containing 1-50,000dth to 1-60,000dth iodine, blue with a shade of violet.

14 and 15. Containing 1-80,000dth to 1-100,000dth iodine, the upper stratum violet blue, the under stratum violet.

16. Containing 1-120,000dth iodine, the upper stratum violet, the under stratum rose-colour.

17. Containing 1-150,000dth iodine, the whole precipitate rose-colour, with a shade of violet.

18 and 19. Containing 1-200,000dth to 1-250,000dth iodine, rose-colour, the upper surface only with a slight shade of violet.

20 to 22. Containing 1-300,000dth to 1-400,000dth iodine, the whole precipitate of a rose-colour.

23 to 25. Containing 1-450,000dth to 1-550,000dth iodine, the upper stratum of the precipitate was slightly rose-colour, the under stratum was white.

The action took place immediately on the addition of the starch as far as No. 19. The following numbers required some period to elapse first: it required some hours in the solution of 1-500,000dth to 1-550,000dth before any effect was produced.

2. The sensibility of reagents *on acids*.

For sulphuric acid, specific gravity 1·829, containing 75·83 per cent. real acid.

Syrup of violets did not detect less than 1-250th sulphuric acid of the above specific gravity, or 1-310th real acid.

Paper stained by Brazil wood was acted upon by 1-10,000dth, or 1-12,500dth real acid.

Paper stained by tincture of red cabbage was reddened by 1-15,000dth, or 1-18,750dth real acid.

Paper stained by logwood was changed to a golden yellow colour by 1-50,000dth, or 1-62,500th real acid.

Paper stained by litmus was immediately reddened by 1-20,000dth, or 1-25,000dth real acid, and after some hours was slightly reddened by 1-50,000dth, or 1-62,500dth real acid.

Carbonate of potash occasioned a slight effervescence with 1-250th, or 1-310th real acid.

For phosphoric acid.—Brazil wood paper and paper stained with red cabbage detected 1-10,000dth of anhydrous phosphoric acid.

Litmus paper was immediately reddened by 1-10,000dth, and after some hours by 1-30,000dth phosphoric acid.

Peculiar reagents for various acids.

For free sulphuric acid.—A concentrated solution of chloride of calcium occasioned a precipitate, after some hours, in a solution containing 1-310th of real sulphuric acid.

A solution of acetate of lead gave a precipitate with 1-50,000dth real acid.

A solution of chloride of barium gave a precipitate with 1-75,000dth.

For combined sulphuric acid.—Acetate of lead produced a precipitate in a solution of sulphate of soda containing 1-36,000dth acid.

Chloride of barium in a solution of the same salt containing 1-45,000dth acid.

For nitric acid.—By means of hydrochloric and gold leaf 1-240th of nitric acid, spec. grav. 1·32 was detected: the gold leaf was dissolved in 24 hours.

For phosphoric acid.—Acetate of lead produced an immediate precipitate with 1-10,000dth anhydrous acid, and with 1-20,000dth, after remaining for half an hour.

Lime water produced exactly the same effect.

Chloride of barium did not occasion a precipitate in less than 1-10,000dth.

For arsenious acid.—Lime water in excess produced a precipitate in a solution containing 1-4000dth of this acid.

Ammoniacal solution of oxide of copper detected 1-8000dth.

Sulphate of copper and ammonia detected 1-12,000dth.

The two last reagents occasioned precipitates in still more diluted solutions, but the precipitates did not possess their characteristic green colour.

Hydrosulphuric acid produced a precipitate in 1-30,000dth.

Ammonia nitrate of silver formed a yellow precipitate with 1-30,000dth; with a more diluted solution the colour of this precipitate was not sufficiently apparent.

3. The sensibility of reagents *for metals and their oxides.*

For free alkalies in general.—Turmeric paper detected the presence of 1-3000dth caustic alkali.

Paper stained with red cabbage detected the presence of 1-7500dth caustic alkali.

Brazil wood paper was coloured slightly violet with 1-20,000dth.

Litmus paper reddened by acetic acid was distinctly rendered blue by 1-80,000dth.

Hydrate of potash contains 16 per cent. of water, therefore the quantity of real potash detected by the above reagents was 1-3600dth, 1-9000dth, 1-24,000dth, and 1-95,000dth.

For potash.—An alcoholic solution of chloride of platinum occasioned a precipitate in a solution of nitrate of potash containing 1-205th of this base; a solution containing 1-200dth was not precipitated by it.

A very concentrated solution of tartaric acid produced a precipitate with 1-220th, but none with 1-230th. The sensibility of these reagents was tried at a temperature of 59° F.

For lime.—Oxalate of ammonia produced a cloudiness after a few minutes in a solution of chloride of lime containing 1-400,000dth of lime.

For baryta.—Fluo-silicic acid produced a slight precipitate in a solution of chloride of barium containing 1-3800dth of baryta.

A solution of sulphate of soda produced in half an hour a cloudiness in a solution containing 1-71,000dth.

For magnesia.—A solution of phosphate of soda indicated in 24 hours the presence of 1-200,000dth of magnesia in a solution of sulphate of magnesia. This reagent must be very concentrated, and a quantity equal to the solution examined, must be added. These conditions are absolutely necessary, as otherwise this reagent will not indicate the presence of even 1-1000dth magnesia in solution. This is probably the reason that M. Roth fixes the delicacy of this reagent at 1-4000dth of magnesia.

A solution of ammonia produced after some minutes a slight precipitate in a solution containing 1-6000dth of magnesia.

For protoxide of iron.—Tincture of galls and a solution of ferroprussiate of potash, acidulated with a few drops of hydrochloric acid, indicated after some minutes the presence of protoxide of iron in a solution containing 1-440,000dth of crystallized sulphate of iron.

For peroxide of iron.—Tincture of galls indicated the presence of 1-300,000dth of peroxide of iron in a solution of sulphate of peroxide of iron by rendering it of a slight violet colour.

A solution of ferroprussiate of potash indicated the presence of 1-420,000dth of the same salt.

For copper.—A solution of ammonia gave after several hours a slight blue colour to a solution of sulphate of copper containing 1-9400dth of oxide of copper.

A solution of prussiate of potash rendered the presence of 1-78,000dth of the same salt visible.

Polished iron showed the presence of 1-125,000dth of oxide of copper, or 1-156,000dth of metallic copper, if the solution was acidulated by a drop of nitric acid.

For lead.—A piece of zinc precipitated lead from a solution of the nitrate when 1-3000dth of oxide was present.

An excess of sulphuric acid occasioned a precipitate in a solution of the same salt containing 1-20,000dth oxide.

A solution of chromate of potash occasioned a cloudiness in a solution containing 1-70,000dth of the same oxide.

A solution of hydrosulphuric acid blackened a solution containing 1-350,000dth.

For silver.—Chromate of lead produced a slight red precipitate in a solution of nitrate of silver containing 1-10,000dth of oxide. No reaction took place in a solution containing above 1-20,000dth.

Arsenite of potash produced a decided yellow precipitate with 1-6000dth oxide in solution, but none with 1-20,000dth.

Iodide of potassium indicated the presence of 1-4000dth oxide, but produced no action with 1-30,000dth.

A solution of hydrosulphuric acid precipitated a solution containing 1-35,000dth of oxide.

Chloride of sodium produces a cloudiness in a solution which contained only 1-240,000dth.—*Extracted by E. F. Teschemacher from the Journal für Praktische Chemie, No.* 1, 1841.

ANILIN.

The following are the additional particulars relative to anilin, referred to in p. 281.

Anilin is a base which with acids readily yields fine crystallized salts. When exposed to atmospheric air, it soon becomes yellow and eventually brown, and the same resinous body is formed which is separated from it by distillation. It must therefore be preserved out of the contact of air, and to prevent its action should be quickly distilled. Anilin may contain a little water, from which it is freed by distillation, taking care, when a third of it has been distilled, to change the receiver; by this method only it is obtained perfectly anhydrous. Boiling anilin dissolves sulphur and iodine in large quantity, and deposits them on cooling, in crystals. Nitric acid, under certain circumstances, converts anilin into a blue or green body, which does not appear to be indigo; this substance has, however, been hitherto obtained only uncertainly and in small quantity, as it undergoes fresh decomposition by the nitric acid. Chromic acid produces in solutions of anilin a precipitate which is sometimes of a deep green colour, and at others of a blackish blue: this acid appears to be a good reagent for anilin, since it is readily produced even in weak solutions; when the precipitate is obtained from concentrated solutions it leaves a considerable quantity of oxide of chromium when calcined. Oxymanganate of potash and the salts of anilin suffer mutual decomposition, brown oxide of manganese being deposited. Anilin yielded by analysis,

Hydrogen	7·54
Carbon	78·21
Azote..	14·83

Hydrochlorate of anilin is obtained by direct combination and crystallization; it is very soluble in water. It consists of

Hydrochloric acid	27·81	
Anilin..	72·19	——100·00.

Oxalate of anilin is obtained by mixing a spirituous solution of anilin with oxalic acid. This salt is a white powder, which is to be washed with alcohol and then dissolved in hot water; on cooling fine crystals of the salt are obtained of several lines in length. This salt appeared to consist of

Oxalic acid	25·92	
Anilin	67·64	
Water	6·44	——100·00.

L'Institut, No. 356.

Notice by Prof. Dove *respecting the Error in his Letter on the Law of Storms, pointed out by* Sir David Brewster *at* p. 514.

By some inadvertence I have confounded the notice of Reid's work in the Edinburgh Review, 1839, p. 406, with that in the Foreign Quarterly Review, 1839, p. 1. The paragraph mentioned by me occurs in the latter work, at p. 2. Those who have read my memoir "On Barometric Minima," which appeared in Poggendorff's *Annalen*, vol. xiii. p. 596, will be best able to judge of the share which I have had in the explanation of the phænomena of storms. As this memoir appears to be known only in Germany, I may observe that the still important empirical data contained in it, have been combined in my recent article on the law of storms (Poggendorff's *Annalen*, 1840, Jan., p. 1), with those for which we are indebted to Messrs. Reid and Redfield. Which of the theoretical derivations, whether that given by Mr. Redfield, at p. 33 on the storms of the American coast, or that published by me (analogous to the circular polarization of light), agrees best with the totality of the phænomena,—or whether the connexion suspected by Messrs. Reid and Piddington, with the magnetism of the earth, be more probable,—I leave to the opinion of those who may be inclined to test these views collectively.

W. Dove.

June 13th.

INDEX

INDEX TO VOL. XVIII.

ACIDS:—sericic, 102; stearic, 114, 191, 293; oleic, 114, 293; margaric, 115, 293; suberic, 116, 422; elaidic, 117, 293; sebacic, 117; palmitic, 187; chlorovalerosic, 205; chlorovalerisic, *ib.*; chlorindopten, 208; bromindoptenic, 209; uric, 210; anhydrous camphoric, 238, 441; thiomelanic, 278; proteinchlorous, 279; xanthoprotemic, *ib.*; benzoenitric and cinnamonitric, 367; ferric, 369; nitric, 417, 605; œnanthylic, 418; œnanthic, 421; lipinic, 422; anhydrous sulphuric, 441; sensibility of a reagent on sulphuric, 605; phosphoric, *ib.*; free sulphuric, *ib.*; combined sulphuric, *ib.*; arsenious, 606.
Æthers:—hydrotelluric, 78; formic, 209; aconitic, 286; itaconic, 287; veratric, 441; action of chlorine on oxalic, 544; chloroxalic, *ib.*
Æthyl, preparation of the telluret of, 210.
Agassiz (Prof.) on the polished surfaces of the rocks forming the beds of glaciers in the Alps, 565; on glaciers, and evidence of their having existed in Scotland, Ireland and England, 569.
Air, condensed, electrical phænomena attending the efflux of, 133, 328.
Airy (G. B.) on the diffraction of an annular aperture, 1; on an apparent new polarity in light, 139; on the resistance of the atmosphere to an oscillating sphere, 321; on the regulator of the clock-work for effecting uniform movement of equatoreals, 590.
Alcohol, voltaic decomposition of, 47, 241, 353; action of potassa on, 203.
Alkarsin, researches on the bodies derived from, 370.
Allenheads, meteorological journal kept at, for 1840, 556.
Alpharesin, 440.
Alps, glaciers in the, polished surfaces of the rocks forming the beds of, 565.
Amilen, 439; acetate of, *ib.*
Anhydrite, analysis of, 120.
Anilin, 280, 607; hydrochlorate of, 608; oxalate of, *ib.*
Animal substances, action of chlorine on, 278.
Antarctic expedition, notice respecting the, 58.
Antimony and arsenic, testing for, 442.
Ants, artificial oil of, 122.
Arabic globe, 593.
Armstrong (W. G.) on the electricity of effluent steam, 50, 328; on the electricity of expanding air, 133.
Arsenic and antimony, testing for, 442.
Astronomical Society:—annual meeting, 141; address of the president, 150, proceedings of the, 590.
Atmosphere, resistance of the, to an oscillating sphere, 321.
Atomic volume and crystalline condition of bodies, 255.
Attractions, calculation of, 550.
Austen (R. A. C.) on the bone caverns of Devonshire, 228.
Baily (Mr.) on some experiments made with three invariable pendulums, 602.
Barometer, cycle of eighteen years in the mean annual height of the, in the climate of London, 552; remarkable depression of the, in November 1840, 553; irregularity in the height of the, 555.
Barry (M.) on the physiology of cells, 308; on the chorda dorsalis, 309; on the corpuscles of the blood, 310.
Baryta, œnanthylate of, 420; sensibility of a reagent for, 606.
Barytes, on the preparation of the chlorate of, 210.
Battery, acid, construction of a constant, 520.
Benzoin, resins of, 439.
Berzeliit, description and analysis of, 157.
Berzelius, Baron von, on Gros's platina salts, 284; on double carbonate of lead and soda, 285; on the views of Liebig and Dumas relative to the constitution of organic acids, 290.
Betaresin, 440.
Binary systems of algebraic equations, 425.
Blake (James) on the action of inorganic compounds when introduced into the blood, 547.
Blood, action of inorganic compounds when introduced into the, 547; on the corpuscles of the, 310.
Blumenbach (J. F.), notice of the late, 70.
Books:—A system of Crystallography, 302; Elements of Chemistry, 304;

Scientific Memoirs, 319; collection of Letters illustrative of the progress of Science, 412.

Bowerbank (J. S.) on the siliceous bodies of the chalk, greensand and oolite, 220.

Bowman (J. E.) on the characters of fossil trees, and on the formation of coal, 212; on supposed moraines of ancient glaciers in Scotland, 337.

Bowman (W.) on the contraction of voluntary muscles in the living body, 560.

Brewster, (Sir D.) correction of an error in Prof. Dove's letter on the law of storms, 314; on a remarkable property of the diamond, 552; on the phænomena of thin plates of solid and fluid substances exposed to polarized light, 561.

Buckland (Prof.) on the evidences of glaciers in Scotland and the north of England, 574, 587.

Buddle (J.) on the great fault in the forest of Dean coal-field, 229.

Bunsen (Dr.) on the bodies derived from alkarsin, 370.

Butler (Dr. S.), notice of the late, 64.

Cacodyl, protochloride of, 436; protoiodide of, *ib.*

Cahours and Gerhardt on æthereal oils, 282, 415.

Calcareous-shelled Polythalamia, list of, 453.

Calvert (Dr. R.) on a bed of lignite near Messina, 566.

Cambridge Philosophical Society, 318, 520.

Camphoric acid, anhydrous, action of anhydrous phosphoric acid on, 238.

Cantraine (Prof.) on electric currents in warm-blooded animals, 271.

Capaun (C. F.) on the preparation of hyposulphite of soda, 211.

Carlisle (Sir A.), notice of the late, 65.

Cells, on the physiology of, 308.

Chalk, on the organic composition of, 220, 383.

Challis (Rev. J.) on the motion of a small sphere in a resisting medium, 130, 321; on the principles of the application of analysis to the motion of fluids, 477; on the motion of a small sphere submitted to the dynamical action of the vibrations of an elastic medium, 521.

Chelidonin, on the composition of, 32.

Chemical Society of London, 410, 515.

Chemical reagents, limit to the action of certain, 604.

Chemistry:—new compound of chlorine and cyanogen, 29; composition of chelidonin and jervin, 32; combination of hydrated sulphuric acid with nitric oxide, 81; new method of analysing ores of iron, 90; new acid from the butter of nutmegs, 102; constitution of fatty substances, 113; artificial oil of ants, 122; analysis of chyle and lymph, 156; oils of elemi and olibanum, 184; palm oil and cacao butter, 186; chemical types, 203; action of potassa on alcohols, 204, 296; oil from *Ruta graveolens*, *ib.*; action of chlorine and bromine on indigo, 297; formic æther, 209; telluret of æthyl, 210; uric acid in snails, *ib.*; brucin, *ib.*; new method of analysing metallic sulphurets, 211; preparation of chlorate of barytes, 210; of hyposulphite of soda, 211; dioxide of copper, 212; fossil wax of Gallicia, 235; minium, 237; action of anhydrous phosphoric acid on anhydrous camphoric acid, 238; voltaic decomposition of aqueous and alcoholic solutions, 241; atomic volume and crystalline condition of bodies, 255; black substance from sulphuric acid and alcohol, 276; action of chlorine on animal substances, 278; action of potash and soda on indigo, 280; palmitic acid, 281; æthereal oils, *ib.*; guaranin, 283; action of chlorine on marsh gas, 284; Gros's salts of platina, 284, 293; Prussian blue, 284; double carbonate of lead and soda, 285; acid products of citric acid at high temperatures, *ib.*; milk of the cow-tree, 291; croconate of copper, 292; state of the haloid salts in solution, 357; benzoenitric and cinnamonitric acids, 367; ferric acid, 369; researches on the cacodyl series, 370; isomorphism of oxamethane and chloroxamethane, 372; cumyle, 415; action of nitric acid on castor oil, 417; œnanthylic æther and œnanthylic acid, 418; œnanthylate of silver, 419; of baryta, 420; of potash, 421; of strontian, *ib.*; œnanthic acid, *ib.*; suberic acid, 116, 422; lipinic acid, *ib.*; on bleaching salts, *ib.*; on the bleaching salts of chlorine, *ib.*; protochloride of cacodyl, 436; protoxide of, *ib.*; oil of sabine, 438; of esdragon, *ib.*; of potatoe spirit, 439; acetate of amilen, *ib.*; on the resins of benzoin, *ib.*; betaresin, alpharesin, gammaresin, 440; action of anhydrous sulphuric acid on anhydrous camphoric acid, 441; manufacture of platinum, 442; on testing for arsenic and antimony by Hume's process, 442; preparation and formation of yellow prussiate of potash, 515; formation of mellon, 518; appearance of flashes of light during the crystallization of ni-

trate of strontian in the dark, 518; action of nitric acid on castor oil, *ib.*; bleaching salts, *ib.*; preparation of chlorate of potash, *ib.*; action of chlorine on oxalic æther, 544; chloroxalic æther, *ib.*; state of urine in urea, 545; limit to the action of certain chemical reagents, 604.
Chinese, the scholar's lute among the, 557.
Chlorindatmit and chlorindopten, preparation of, 207.
Chlorine, on a new compound of, with cyanogen, 29; action of, on animal substances, 278; action of, on marsh gas, 284; bleaching salts of, 422; action of, on oxalic æther, 544.
Chlorisatinic acid, 208.
Chlorospinelle, analysis of, 122.
Chlorovalerosic and chlorovalerisic acids, 205.
Chloroxalic æther, 544; compounds derived from, 545.
Chloroxamethane, isomorphism of oxamethane with, 372.
Circuits of zinc and iron, on the intensity of currents of, 42.
Citric acid, on the products of, at high temperatures, 285; crystallization of water of, 289.
Clock, description of the electro-magnetic, 139.
Coals, on the formation of, 211; self-combustion of, *ib.*
Connell (A.) on the voltaic decomposition of alcohol, 47, 241, 353.
Copper, dioxide of, 212; native phosphate of, 236; croconate of 292; sensibility of a reagent for, 606.
Corals, researches on, 375.
Cow-tree, on the milk of the, 291.
Crasso (G. L.) on the acid products of citric acid at high temperatures, 285.
Creuze (Mr.) on the structure of the Royal George, and on the condition of the timber, iron, &c., 231.
Croconate of copper, 292.
Croft and Francis's notices of the investigations of continental chemists, 202, 276, 367, 436, 544; remarks on, 296; errata in, 546.
Crystals, biaxal, conical refraction in, 343.
Cumyle and oil of cumin, researches on, 415.
Daguerreotype pictures, on the cause of the production of, 301.
Davy (Dr. J.), miscellaneous observations on the torpedo, 552.
De la Rive's theory of electricity, observations on, 193; Prof. Marianini's examination of an experiment adduced by Prof. Faraday in support of, 529.
Detmer (M.) on bleaching salts, 422.
Diamond mines of Golconda, present state of the, 308.
Diamond, remarkable property of the, 552.
Diffraction of an annular aperture, on the, 1.
Dove's (Prof.) letter on the law of storms, correction of an error in, by Sir D. Brewster, 514; notice respecting the error, 608.
Drach (S. M.) on some new and curious numerical relations of the solar system, 37.
Duflos (M.) on the preparation of the chlorate of barytes, 210.
Dumas and Stas on chemical types, 203.
Earth, figure of the, 550.
Ehrenberg (Prof.) on the coralline tribes, 375; on the organic composition of chalk and chalk marl, 383.
Electrical phænomena attending the efflux of steam, 50, 93, 95, 100, 265, 328; of condensed air, 133, 328.
Electrical Society of London, 520.
Electric currents in warm-blooded animals, 271.
Electricity, on steam considered as a conductor of, 14; observations on De la Rive's theory of, 193; production of heat by voltaic, 308.
Electricity and magnetism, Prof. Henry's contributions to, 482.
Electro-dynamic induction, 482; on apparently two kinds of, 492.
Electro-magnetic clock, description of the, 139.
Electro-nitrogurets, on some, 548.
Electrotype manipulation, 520.
Elemi and olibanum, analysis of the oils of, 184.
Epps (J.), notice of the late, 146.
Equations, algebraic, linear method of eliminating between double, treble, and other systems of, 425; binary systems of, 425; ternary systems of, 427; new method of solving numerical, 559.
Equatoreals, regulator of the clock-work for effecting uniform movement of, 590.
Erdmann (Prof.) on the action of chlorine and bromine on indigo, 247; on the black substance from sulphuric acid and alcohol, 276; on anilin, 280.
Esdragon, oil of, 438.
Faraday (Prof.), second letter to, from Dr. Hare, 465; Prof. Marianini on an experiment adduced by, in support of Prof. De la Rive's theory of electricity, 529.
Farquharson (J.) on ground-gru, or ice

formed, under peculiar circumstances, at the bottom of running water, 555; on the localities affected by hoar frost, the peculiar currents of air excited by it, and the temperature during its occurrence at high and low stations, 554.

Fatty substances, researches on the, 113.

Favio (Dr.) on electric currents in warm-blooded animals, 271.

Fibroferrite, analysis of, 397.

Fluids, principles of the application of analysis to the motion of, 477.

Foraminifers of the white chalk of the Paris basin, 456.

Forfarshire, existence of glaciers in, 579.

Formic æther, preparation of, 209.

Fossil remains from the chalk, 316.

Fremy (M.) on palmitic acid, 281; on ferric acid, 369.

Francis and Croft's notices of the investigations of continental chemists, 202, 276, 367, 436, 544; errata in, 546.

Fuchs (Prof.) on a new method of analysing the ores of iron, 90.

Fuss (Dr.) on brucin, 210.

Gammaresin, 440.

Geological Society, 212, 311, 398, 522, 561.

Geology:—on the characters of fossil trees found near Manchester, and on the formation of coal, 212; on the beds of clay below the coal seams of South Wales, 217; on the rocks which form the left shore of the bay of Loch Ryan, 219; on the siliceous bodies of the chalk, 220; on the age of the limestone of South Devon, 223; on the bone caves of Devonshire, 228; on the great fault in the forest of Dean coal-field, 229; on the subsidence of the coast near Puzzuoli, 232; on part of Borneo Proper, *ib.*; on some geological specimens from Syria, 233; geological features of Ionia, Caria and Rhodes, 311; remains of a bird, tortoise, and Lacertian saurian from the chalk, 316; on the organic composition of the chalk and chalk marls, 383, 443; classification and distribution of the older rocks of North Germany, 398; living polythalamia on the African and Asiatic coasts of the Mediterranean, 443; on M. Alcide d'Orbigny's view of the white chalk of the Paris basin, 456; list of foraminifers found in the Paris basin, 461; on a mass of trap in the mountain limestone in the Bleadon hill, 522; on the coloured sections of the cuttings on the Birmingham and Gloucester railway, 523; on a coral reef in the island of the Mauritius, 526; on a portion of the lower jaw of an iguanodon and other saurian remains found in Tilgate forest, 551; geological remarks on Kerguelen's Land, 558; on the mineral veins of the Sierra Almagrera, 561; on the Sierra de Gador and its lead mines, 563; on the polished surfaces of the rocks forming the glaciers in the Alps, 565; on a bed of lignite near Messina, 566; on the chalk and the subjacent formations to the Purbeck stone inclusive in the north of Germany, *ib.*; on a large saurian discovered near Hythe, 568; evidence of glaciers having existed in Scotland, Ireland, and England, 569; in Scotland and the north of England, 574, 587; in Forfarshire, 579; dispersion of Shap Fell granite by ice, 589.

Gerhardt and Cahours on æthereal oils, 282, 415.

Gilbert (D.), notice of the late, 62, 142.

Glaciers:—on supposed moraines of, in Scotland, 337; polished surfaces of the rocks forming the beds of, in the Alps, 565; evidence of their having existed in Scotland, Ireland, and England, 569; former existence of, in Forfarshire, 572, 579; in Scotland and the north of England, 574; evidences of, on Schiehallion, 576; in and near Strath Earn, *ib.*; near Comrie, 577; near Loch Earn, *ib.*; proofs of glacial action at Stirling and Edinburgh, 578; in the mountains of Cumberland and Westmoreland, 588.

Glashier (Mr.) on the correction of the elements of the orbit of Venus, 601.

Gmelin (L.) on Prussian blue, 284; on the croconate of copper, 292.

Graham (Prof.) on the preparation of chlorate of potash, 518.

Griffin's (J. J.) System of Crystallography, reviewed, 302.

Ground-gru, or ice formed at the bottom of running water, 555.

Grove (W. R.) on a powerful voltaic combination, 234; on some electro-nitroguerets, 548.

Halliwell (J. O.) on the Boetian system of numerical contractions, and the Alsbaldine notation, 13.

Hamilton (W. J.) on the geology of Ionia, Caria, and Rhodes, 311.

Hare's (Dr. R.) second letter to Prof. Faraday, 461.

Hargreave (C. J.) on the calculation of attractions, and the figure of the earth, 550.

Harris (W. S.) on lightning conductors for ships, 172.

Harting (M. P.) on the limit to the action of certain chemical reagents, 604.

Heat, on the change of crystalline form by, 255; on the production of, by voltaic electricity, 308.

Henry (Prof. J.), contributions to electricity and magnetism, No. IV. on electro-dynamic induction, 482; theoretical considerations relating to the phænomena described in this and the preceding communications, 498.

Herschel (Sir J. F. W.), anniversary address at the Astronomical Society, 150.

Hils conglomerate, 567; clay, *ib.*

Howard (Luke) on a cycle of eighteen years in the mean annual height of the barometer in the climate of London, 552; on a remarkable depression of the barometer in November 1840, 553; on the prevailing winds, mean temperature, and depth of rain in the climate of London, 559.

Hullmandel (C.) on the subsidence of the coast near Puzzuoli, 232.

Hume's process, on testing for arsenic and antimony by, 442.

Hunt (R.) on a remarkable solar bow, 158.

Hydrotelluric æther, formation and analysis of, 78.

Hylæosaurus, 551.

Hyposulphite of soda, new method of preparing the, 211.

Ice, dispersion of Shap Fell granite by, 589.

Iguanodon, portion of the lower jaw of an, 551.

Indigo, action of chlorine and bromine on, 207; action of potash and soda on, 280.

Indopten, chloride of, 207.

Induction, electro-dynamic, 482; produced at the moment of the beginning of a galvanic current, &c., 483; on apparently two kinds of, 492.

Infusoria, siliceous-shelled, list of, 453.

Innes (George), elements of the annular eclipse of the sun that will happen on October 8, 1847, 603.

Inorganic compounds, action of, when introduced into the blood, 547.

Iodine, sensibility of starch as a reagent for, 604.

Iron, notice of an undescribed subsulphate of, 397; sensibility of a reagent for protoxide of, 606; for peroxide of, *ib.*

Iron ores, new method of analysing, 90.

Jervin, on the composition of, 35.

Joule (J. P.) on the production of heat by voltaic electricity, 308.

Kane (R.) on the true constitution of Gros's platina salts, 293; Elements of Chemistry, reviewed, 304.

Kemp (W.) on supposed moraines of ancient glaciers in Scotland, 337.

Kerguelen's Land, on the birds of, 558; geological remarks on, *ib.*

Kersten (M.) on a new method of analysing metallic sulphurets, 211.

Kopp (H.) on the atomic volume and crystalline condition of bodies, and on the change of crystalline form by means of heat, 255.

Kühn (M.), analysis of berzeliit, 157; of a native phosphate of copper, 236.

Lambert (J.) on the mineral veins of the Sierra Almagrera, 561; on the Sierra de Gador and its lead mines, 563.

Latham (R. G.) on the science of phonetics, 124.

Laurent (M.) on the oil of esdragon, 438.

Lay (G. T.) on part of Borneo Proper, 232.

Lay (—), the scholar's lute among the Chinese, 557.

Lead, sensibility of a reagent for, 607.

Lepidomelane, analysis of, 77.

Levol (M.) on minium, 237.

Liebig (Prof.) on the preparation and formation of yellow prussiate of potash, 515.

Light, apparent new polarity in, 139.

Lightning, experiments relating to the defence of shipping from, 172.

Lignite near Messina, on a bed of, 566.

Lime, sensibility of a reagent for, 606.

Lipinic acid, 422.

Lloyd's (Capt.) letter to Mr. Murchison on a coral reef in the island of Mauritius, 526.

Logan (W. E.) on the beds of clay lying beneath the coal seams of S. Wales, 217.

London Electrical Society, 409, 520.

London Institution, 234.

Lonsdale (W.) on the age of the limestone of S. Devon, 223.

Lubbock (Sir J. W.) on a theorem of Fermat, 552; on an irregularity in the height of the barometer, 555.

Lyell (Chas.) on the geological evidence of the former existence of glaciers in Forfarshire, 579.

M. (W. A.), remarks on Messrs. Francis and Croft's abstracts, 206; note on, 300.

Mackeson (H. B.) on a large saurian discovered near Hythe, 568.

Maclear (T.) on the difference of longitude between the observations of Madras and the Cape of Good Hope, deduced from moon-culminating stars, 604; observations made at the Cape of Good Hope, in the year 1838, with Bradley's zenith sector, for the verification of the Abbé de Lacaille's arc of the meridian, 593; on some experiments made with an invariable pendulum, 602.

Madras, longitude of, computed from moon-culminating observations, 596.
Magnesia, sensibility of a reagent for, 606.
Magnetic and meteorological observations:—at Constantinople, 553; at Prague, 553, 554; at Kerguelen's Land, 553; on H.M.S. *Erebus*, *ib.*; H.M.S. *Terror*, *ib.*; at Milan, 555, 558; at St. Helena, 555; at Toronto, *ib.*; at Hobart Town, 557; at Van Diemen's Land, 557, 558.
Magnetism and electricity, Prof. Henry's contributions to, 482.
Magnetism, terrestrial, contributions to, 549.
Main (Rev. R.) on the present state of our knowledge of the parallax of the fixed stars, 597.
Manipulation, electrotype, 520.
Mantell (G.A.) on a portion of the lower jaw of an iguanodon, and other saurian remains in Tilgate forest, 551.
Marchand (Dr.) on the dioxide of copper, 212; on aconitic and itaconic æthers, 286; on the milk of the cow-tree, 291.
Marianini (Prof.) on an experiment adduced by Prof. Faraday in support of Prof. De la Rive's theory of electricity, 193, 529.
Marsh (J.) on testing for arsenic and antimony by Hume's process, 442.
Mauritius, coral reef in the island of, 526.
McCormick (R.) geological remarks on Kerguelen's Land, 558; on the birds of Kerguelen's Land, *ib.*
Megalosaurus, 552.
Mellon, formation of, 518.
Messina, bed of lignite near, 566.
Meteorological observations and table, 79, 159, 240, 320, 416, 527.
Miller (Prof.) on supernumerary rainbows, 520.
Millon (M.) on the bleaching salts of chlorine, 422.
Mineralogy:—lepidomelane, 77; antigorite, 120; pennine, 121; chlorospinelle, *ib.*; xanthophyllite, 122; pikrophylle, *ib.*; Berzeliit, 157; phosphate of copper, 236; fibroferrite, 397; phosphate of yttria, 519.
Minium, constitution of, 237.
Mitscherlich (Prof.) on cinnamonitric and benzoenitric acids, 367.
Molecules, on the symmetrical arrangement of, 161.
Moore (J.C.) on the rocks which form the west shore of the bay of Loch Ryan, 219.
Moraines of ancient glaciers in Scotland, 337; near Dumfries, 575; in Forfarshire, *ib.*; in Aberdeenshire, *ib.*; at Taymouth, 575; in Glen Cofield, *ib.*; near Callender, 577; in Northumberland, 587.
Morgan (Prof. A. de) on a method of computing life contingencies, 268.
Mulder (M.) on the action of chlorine on animal substances, 278; on cinnamonitric and benzoenitric acids, 367.
Murchison (R. I.) on the classification and distribution of the older rocks of the north of Germany, 398; letter to, by Capt. Lloyd, on a coral reef in the island of Mauritius, 526.
Muscles, voluntary, contraction of, in the living body, 560.
Mylias (M.) on the occurrence of uric acid in snails, 210.
Nautilus Orbiculus, 446.
Newton (Sir Isaac) presentation of the portrait of, to the Royal Society, by C. Vignolles, 556.
Newtonian reflecting telescope, seven feet, presentation of, to the Astronomical Society, 597.
Nitric acid, products of the action of, on castor oil, 417.
Nitric oxide, combination of, with sulphuric acid, 81.
Northampton, Marquis of, anniversary address by the, at the Royal Society, 57.
Northumberland, moraines in, 587.
Nutmegs, on a new fat acid in the butter of, 102.
Œnanthic acid, 421.
Œnanthylate of silver, 419; of baryta, 420; of potash, 421; of strontian, *ib.*
Œnanthylic acid, 418; æther, *ib.*
Oils:—elemi and olibanum, 184; palm, 186; from *Ruta graveolens*, 207; cascarilla and carraway, 281; of potatoe spirit, 281, 439; of anise and of fennel, 282; of bergamot, 283; products of the action of nitric acid on castor, 417; of esdragon, 438; of sabine, *ib.*
Olbers (Dr.), notice of the late, 72.
Optics, physical, on the application of Huyghens's principle in, 11.
Orbigny's (M. Alcide d') view of the white chalk of the Paris basin, 456; on the foraminifera of the white chalk of the Paris basin, *ib.*
Owen (R.) on the fossil remains of a bird, tortoise, and saurian from the chalk, 318.
Oxalic æther, action of chlorine on, 544; compounds derived from, 545.
Oxamethane, isomorphism of, with chloroxamethane, 372.
Parallax of the fixed stars, present state of our knowledge of the, 597.
Paris basin, Mr. Weaver on M. Alcide

d'Orbigny's view of the white chalk of the, 456; on the foraminifers of the white chalk of the, *ib.*; list of foraminifers found in the, 461.
Peltier (M.) on the phænomena of the electricity of steam, 100.
Pendulums, invariable, experiments made with, 602.
Peneroplis planatus, 444.
Pennine, analysis of, 121.
Phonetics, facts and observations relating to the science of, 124, 363.
Phosphoric acid, anhydrous, action of, on camphoric acid, 238.
Pikrophylle, analysis of, 122.
Plana (M.), award of the astronomical medal to, 153.
Platinum, on Magnus and Gros's salts of, 284, 293; manufacture of, 442.
Playfair (L.) on a new fat acid from the butter of nutmegs, 102.
Plesiosaurus, 552.
Poggendorff (Prof.) on the surprising intensity of currents of the zinc iron circuit, 42.
Poisson (D.), notice of the late, 74.
Polythalamia, living, geological distribution of, on the African and Asiatic coasts of the Mediterranean, 443; remarks on, 444; calcareous-shelled, 453.
Potash, œnanthylate of, 421; preparation and formation of yellow prussiate of, 515; preparation of chlorate of, 518; sensibility of a reagent for, 606.
Potatoe spirit, oil of, 439.
Potter (R.) on the application of Huyghens's principle in physical optics, 11; on conical refraction in biaxal crystals, 343.
Powell (Rev. B.) on certain points in the theory of undulations, 161, 270.
Prideaux (J.) on an undescribed subsulphate of iron, 397.
Prinsep (J.), notice of the late, 64.
Protochloride of cacodyl, 436.
Provostaye (M. de la) on the isomorphism of oxamethane and chloroxamethane, 372.
Pterodactylus, 552.
Rainbows, supernumerary, 520.
Redfield (W. C.) on the direction of the wind as observed in the storm of Dec. 15, 1839, 17; on the tornado which visited New Brunswick, June 19, 1835, 20.
Rees (G. O.) on chyle and lymph, 156.
Refraction, conical, in biaxal crystals, 343.
Resins from the milk of the cow-tree, 291.
Riddle (E.), on the longitude of Madras, computed from moon-culminating observations, 596.
Rigaud (Prof.), notice of the late, 144.
Roberts (M. J.) on the cause of the production of daguerreotype figures, 301.
Roemer (M.) on the chalk and the subjacent formations to the Purbeck stone inclusive in the north of Germany, 566.
Rose (A.) on the combination of sulphuric acid with nitric oxide, 81.
Rose (H.), analysis of chlorospinelle, 122.
Rotalia Beccarii, 444.
Rothman (R. W.) on an Arabic globe belonging to the Astronomical Society, 593.
Royal Society, proceedings of the, 57, 139, 307, 547.
Rumker (Mr.) ephemeris and elements of the third comet discovered by Galle, 597.
Ruta graveolens, oil obtained from, 207.
Rutherford (W.) on a new and simple series, by which the ratio of the diameter of a circle to its circumference may easily be computed to any required degree of accuracy, 561.
Sabine, oil of, 438.
Sabine (Major Edward) on terrestrial magnetism, No. 2., 549.
Salts, haloid, on the state in which they are dissolved by water and alcohol, 357; bleaching salts, 422.
Schafhaeutl (Dr. C.) on the circumstances under which steam developes positive electricity, 95; on steam considered as a conductor of electricity, 14, 265.
Schweizer (M.), analysis of antigorite and pennine, 120.
Science, letters on the progress of, in England, reviewed, 412.
Sea, on the mean level of the, 183.
Sedgwick (Prof.) on the classification and distribution of the older rocks of north Germany, 398.
Sierra Almagrera, mineral veins of the, 561.
Sierra de Gador, and its lead mines, 563.
Siliceous-shelled infusoria, list of, 453.
Silver, œnanthylate of, 419; sensibility of a reagent for, 607.
Simms's (Mr.) comparison of the Neapolitan standard yard with that of the Astronomical Society, 603.
Smith (A.) on Irish tin ore, 134.
Snails, occurrence of uric acid in, 210.
Soda, hyposulphite of, mode of preparing the, 211.
Sorites Orbiculus, 444.
Soubeiran (M.) on the oil of bergamot, 383.
Sounds, on the natural arrangement of consonantal, 363.
Starch, sensibility of, as a reagent for iodine, 604.

Stars, fixed, present state of our knowledge of the parallax of the, 597.
Steam, considered as a conductor of electricity, 14; on the electricity of effluent, 50, 93, 95, 100, 328.
Steneosaurus, 552.
Stenhouse (J.) on a new compound of chlorine and cyanogen, 29; on artificial oil of ants, 122; on the oils of elemi and olibanum, 184; on palm oil, and cacao butter, 186.
Storms, papers relating to the law of, 17, 20; correction of an error in Prof. Dove's letter on the law of, 514; notice by Prof. Dove respecting the error, 608.
Strickland's (H. E.) series of coloured sections of the cuttings on the Birmingham and Gloucester railway, 523.
Strontian, œnanthylate of, 421.
Suberic acid, 116, 422.
Sulphurets, metallic, new mode of analysis of, 211.
Sulphuric acid, on the combination of, with nitric oxide, 81; action of, on alcohol, 276.
Sylvester (Prof.) on a new and more general theory of multiple roots, 137, 249; on a linear method of eliminating between double, treble, and other systems of algebraic equations, 423.
Ternary systems of algebraic equations, 427.
Thomas's (R.) remarks on Prof. Whewell's paper on the mean level of the sea, 183.
Tilley (T. G.) on some of the products of the action of nitric acid on castor oil, 417.
Tin ore, occurrence of, in Ireland, 134.
Torpedo, on the, 552.
Tovey (Mr.) on the application of Huyghens's principle in physical optics, Mr. R. Potter's reply to, 11.
Trees, fossil, characters of some, 212.
Undulations, theory of, on certain points in the, 161, 270.
Urea, state of, in urine, 545.
Urine, state of urea in, 545.
Vegetables, living, power of attraction of, 520.
Venus, orbit of, investigation for the correction of the elements of the, 601.
Veratric æther, 441.
Vigors (N. A.), notice of the late, 66.
Vliet (M. van der) on the resins of benzoin, 439.
Vœlckel (M.) on the oils of cascarilla and caraway, 281.
Walker (C. V.) on electrotype manipulation, and on the construction of a constant acid battery, 520.
Walter (M. P.) on the action of anhydrous phosphoric acid on camphoric acid, 238; on the action of anhydrous sulphuric acid on anhydrous camphoric acid, 441.
Walton (Rev. W.), meteorological journal for 1840, at Allenheads, 556.
Wax, fossil, from Gallicia, analysis of, 235.
Weaver (T.) on the composition of chalk rocks and chalk marls by invisible organic bodies, 375, 443; on M. Alcide d'Orbigny's view of the white chalk of the Paris basin, 456.
Weddle (T.) on a new method of solving numerical equations, 559.
Wedgewood (H.) on the natural arrangement of consonantal sounds, 363.
Wheatstone (Prof.) on the electro-magnetic clock, 139.
Will (Dr. H.) on the composition of chelidonin and Jervin, 32; on the oil obtained from *Ruta graveolens*, 297; on veratric æther, 441.
Williams (Rev. D.) on a mass of trap in the mountain limestone on the western extremity of Bleadon hill, 522.
Williams (J.) on the electricity of steam, 93.
Williamson (W. C.) on some geological specimens from Syria, 233.
Wind, on the direction of the, in the storm of Dec. 15th, 1839, 17.
Woehler (Prof.) on hydrotelluric æther, 78; on the preparation of formic æther, 209; on telluret of æthyl, 219.
Xanthophyllite, analysis of, 122.
Yttria, phosphate of, 519.
Zantedeschi (Prof.) on electric currents in warm-blooded animals, 271.

END OF THE EIGHTEENTH VOLUME.

Printed by Richard and John E. Taylor, Red Lion Court, Fleet Street.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

CONDUCTED BY

SIR DAVID BREWSTER, K.H. LL.D. F.R.S.L.&E. &c.
RICHARD TAYLOR, F.L.S. G.S. Astr.S. Nat.H.Mosc. &c.
RICHARD PHILLIPS, F.R.S.L.&E. F.G.S. &c.
ROBERT KANE, M.D. M.R.I.A.
EDWARD WILLIAM BRAYLEY, F.L.S. F.G.S.

"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." Just. Lips. *Monit. Polit.* lib. i. cap. 1.

VOL. XIX.
NEW AND UNITED SERIES OF THE PHILOSOPHICAL MAGAZINE,
ANNALS OF PHILOSOPHY, AND JOURNAL OF SCIENCE.
JULY—DECEMBER, 1841.

LONDON:

RICHARD AND JOHN E. TAYLOR, RED LION COURT, FLEET STREET,
Printers and Publishers to the University of London;
SOLD BY LONGMAN, BROWN, GREEN, AND LONGMANS; CADELL; SIMPKIN,
MARSHALL AND CO.; S. HIGHLEY; WHITTAKER AND CO.; AND
SHERWOOD, GILBERT, AND PIPER, LONDON: — BY ADAM AND
CHARLES BLACK, AND THOMAS CLARK, EDINBURGH; SMITH
AND SON, GLASGOW; HODGES AND SMITH, DUBLIN:
AND G. W. M. REYNOLDS, PARIS.

CONTENTS OF VOL. XIX.

NUMBER CXXI.—JULY, 1841.

Page
Prof. Daniell on the Spontaneous Evolution of Sulphuretted Hydrogen in the Waters of the Western Coast of Africa, and of other Localities 1
Abstract of recent Researches on the Quantity of Heat evolved in Chemical Combination, particularly those of MM. Dulong and Hess.. 19
Mr. W. G. Armstrong's Further Experiments on the Electricity of Effluent Steam 25
Mr. Weaver on Irish Tin Ore 27
Mr. Martyn J. Roberts on the Analogy between the Phænomena of the Electric and Nervous Influences 31
Mr. J. Stenhouse on the Action of Peroxide of Lead on Cinnamic Acid and on Salicine 38
Messrs. W. Francis and H. Croft's Notices of the Results of the Labours of Continental Chemists (*continued*) 41
Mr. W. R. Birt on a Paraselene witnessed on May 6, 1841 .. 50
Dr. J. Sutherland on the Polarization of the Chemical Rays of Light 52
The Rev. J. Park on an Improvement in the Air-pump 59
Mr. W. J. Henwood on Mr. J. Scott Russell's Remarks on the Temperature of most effective Condensation (of Steam) .. 60
Mr. L. F. Menabrea on Cavendish's Experiment............ 62
The Rev. J. Challis's Reply to Mr. Airy's additional Remarks on Professor Challis's Investigation of the Resistance of the Air to an Oscillating Sphere.......................... 63
Prof. Forbes's Researches on Heat.—Fourth Series. On the Effect of the Mechanical Texture of Screens on the immediate transmission of Radiant Heat 69
New Books :—Lubbock on the Theory of the Moon, and on the Perturbations of the Planets. Part IV.—Woodhouse's Essay on Single Vision 81
Proceedings of the London Electrical Society.............. 87
——————— London Institution.................. 88
Two Letters on Calotype Photogenic Drawing, from H. F. Talbot, Esq., F.R.S., to the Editor of the Literary Gazette 88
Process for obtaining the Hydrobromic and Hydriodic Acids, by Dr. R. M. Glover 92
New Alum, by Dr. Mohr 93
On the supposed Hydrate of Phosphorus, by M. Marchand .. 93
Nitro-Saccharic Acid.................................. 94
Geology and Mineralogy of Engineering.................. 95
Meteorological Observations for May 1841................ 95

Page
Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; by Mr. Thompson at the Garden of the Horticultural Society at Chiswick, near London; by Mr. Veall at Boston; and by Mr. Dunbar at Applegarth Manse, Dumfries-shire 96

NUMBER CXXII.—AUGUST.

Mr. Grove on some Electro-Nitrogurets 97
Dr. Faraday on some supposed forms of Lightning.......... 104
Mr. Martyn J. Roberts's Remarks on Professor Poggendorff's Paper on the Intensity of Current of the Zinc-iron Circuit 106
Prof. Forbes's Researches on Heat.—Fourth Series. On the Effect of the Mechanical Texture of Screens on the immediate transmssion of Radiant Heat 109
Prof. Booth on a new Theorem in the Calculus of Finite Differences, with its Application to the Development of the Cosine of a Multiple Arc in descending Powers of the Cosine of the Simple Arc 125
Mr. T. Hopkins on the Formation of the Cumulus Cloud 135
Mr. Airy's Reply to Prof. Challis on the Investigation of the Resistance of the Air to an Oscillating Sphere 143
Mr. Potter on the Phænomena of Diffraction in the Centre of the Shadow of a Circular Disc, placed before a luminous Point, as exhibited by Experiment 151
Proceedings of the Royal Society 156
——————— Geological Society 168
Meteorological Observations for June 1841 175
——————— Table 176

NUMBER CXXIII.—SEPTEMBER.

Prof. Miller on the Form and Optical Constants of Anhydrite.. 177
Abstract of recent Researches on the quantity of Heat evolved in Chemical Combination, particularly those of MM. Dulong, Hess and Andrews (*continued*)......................... 178
Messrs. W. Francis and H. Croft's Notices of the Results of the Labours of Continental Chemists (*continued*) 184
Prof. Draper on some Analogies between the Phænomena of the Chemical Rays, and those of Radiant Heat 195
Prof. Redtenbacher of Prague, and Liebig of Giessen on the Atomic Weight of Carbon 210
The Rev. J. Challis on a new Method of Investigating the Resistance of the Air to an Oscillating Sphere.............. 229

Page
Mr. R. Mallet's Note on the Production of Sulphuretted Hydrogen by the Action of decomposing Animal Matter upon Sulphates 235
New Books :—Dr. Kane's Elements of Chemistry 237
Proceedings of the Royal Society 240
———— Royal Astronomical Society 241
———— London Electrical Society 245
———— Royal Irish Academy 247
Action of Sulphuric Acid on Nitrate of Ammonia, by M. Pelouze 251
Preparation of Azotic Gas, by M. Pelouze 252
Concentration of Nitric Acid by means of Sulphuric Acid, by M. Pelouze 252
Composition of Sugar of Gelatine 253
Lactate of Urea in Urine 253
Meteorological Observations for July 1841 255
———— Table 256

NUMBER CXXIV.—OCTOBER.

Mr. Darwin on a remarkable Bar of Sandstone off Pernambuco, on the Coast of Brazil 257
Mr. J. P. Joule on the Heat evolved by Metallic Conductors of Electricity, and in the Cells of a Battery during Electrolysis 260
Messrs. W. Francis and H. Croft's Notices of the Results of the Labours of Continental Chemists (*continued*) 278
Mr. J. S. McCord's Meteorological Summary of the Weather at Montreal, Province of Canada, in Lat. 45° 30′ N., Long. 73° 22′ W. 285
Prof. Booth's Remarks on a Statement in the *Traité de Mécanique* of Poisson 287
Sir W. R. Hamilton on the Focal Lengths and Aberrations of a thin Lens of Uniaxal Crystal, bounded by Surfaces which are of Revolution about its Axis 289
Dr. Brett and Mr. J. D. Smith's Experiments on the alleged Conversion of Carbon into Silicon 295
Solution of a Geometrical Problem on the Form of the Black Cross in Biaxal Crystals 305
Mr. R. Hagen on Malic Acid, and the Changes undergone by its Salts at High Temperatures 306
Proceedings of the Geological Society 315
———— Royal Astronomical Society 325
———— London Electrical Society 327
———— Chemical Society of London 328
Preparation of Lactic Acid and Lactates 333
Preparation of Formic Æther 334

Page
Preparation of Iodate of Potash, by M. Mellon 334
Professorship of Geology in University College, London 335
Meteorological Observations for August 1841.............. 335
———— Table.............................. 336

NUMBER CXXV.—NOVEMBER.

M. Dumas on the Chemical Statics of Organized Beings...... 337
Miss Zornlin on the Periodical Shooting Stars, and on Shooting Stars in general.................................. 347
Mr. M. P. Moyle on an Analysis of the Atmosphere of some of the Cornish Mines.............................. 357
The Rev. R. Murphy's Remarks on Primitive Radices........ 369
Messrs. C. Hare and M. H. Boye on the Perchlorate of the Oxide of Æthule, or Perchloric Æther 370
Prof. Powell's Note on the Theory of Light 372
Mr. W. Petrie's Results of some Experiments in Electricity and Magnetism...................................... 374
On a Theorem given in the Philosophical Magazine for August 380
Sir W. R. Hamilton on a Mode of deducing the Equation of Fresnel's Wave 381
Remarks on M. Mossotti's Theory of Molecular Action...... 384
Dr. S. M. Brown's Observations on Dr. Brett and Mr. Smith's Experiments on the alleged Conversion of Carbon into Silicon 388
Mr. E. Solly's Description of an Electric Thermometer...... 391
Proceedings of the Geological Society.................... 394
———— Zoological Society.................... 403
———— London Electrical Society 404
Notice of Beard's Patent for Improvements in Apparatus for obtaining Likenesses and Representations of Nature, &c. .. 405
On the Polished Rocks of Fontainbleau, by M. Durocher 409
On the Composition of the Brain of Man, by M. Fremy...... 411
Action of Peroxide of Iron on Potash, by M. Fremy 412
Preparation of Guaiaic Acid, by M. Thierry................ 413
London Institution 414
Scientific Books.. 414
Meteorological Observations for September 1841 415
———— Table.............................. 416

NUMBER CXXVI.—DECEMBER.

Mr. Murchison's First Sketch of some of the principal Results of a Second Geological Survey of Russia............... 417

Page
Dr. Hare on the Theory of Storms, with reference to the Views of Mr. Redfield 423
Prof. Booth on the Rotation of a rigid Body round a fixed Point 432
Mr. R. Hunt on the Conversion of the Bisulphuret of Copper (Yellow Copper Ore) into the Sulphuret (Vitreous Copper) by Electricity 442
Prof. De Morgan on the History of Fernel's Measure of a Degree 445
Messrs. W. Francis and H. Croft's Notices of the Results of the Labours of Continental Chemists (*continued*).......... 447
Mr. T. B. Jordan's Remarks on Electro-Metallurgy 452
M. Dumas on the Chemical Statics of Organized Beings (*concluded*) 456
Mr. J. E. Bowman on the question, Whether there are any evidences of the former existence of Glaciers in North Wales? 469
The Rev. R. Murphy's Calculation of Logarithms by means of Algebraic Fractions 479
Mr. H. Reinsch on the Action of Metallic Copper on Solutions of certain Metals, particularly with reference to the detection of Arsenic 480
Proceedings of the Royal Society.......... 483
——————— Geological Society.......... 489
Mr. Brayley's Lectures on Igneous Meteors and Meteorites .. 500
Action of Sulphuric Acid on Protosulphate of Iron.......... 502
Books.......... 503
Meteorological Observations for October 1841 503
——————— Table.......... 504

NUMBER CXXVII.—SUPPLEMENT TO VOL. XIX.

Mr. Airy on an extraordinary Magnetic Disturbance recently observed at Greenwich 505
Dr. Ure's Table of the successive Strengths of Pyroxylic Spirit, corresponding to its successive Specific Gravities, with some Introductory Observations 511
The late R. C. Campbell on the Ferrocyanides 513
Proceedings of the Royal Society 517
——————— Geological Society 522
——————— Royal Astronomical Society.......... 547
——————— Royal Irish Academy 584
Dr. Martin Barry on the Corpuscles of the Blood 597
Composition of Mellitic Acid 598
Salts of Lead containing Acids of Oxygen and Azote.......... 598
Mr. Halliwell's Inquiry respecting a MS. at Oxford, referred to by Mr. Rutherford in his Paper on the Ratio of the Diameter of a Circle to its Circumference 600
Index 601

PLATE.

I. Illustrative of Dr. DRAPER's Paper on some Analogies between the Phænomena of the Chemical Rays, and those of Radiant Heat.

ERRATA.

Page 261, line 20, *for* an atomic element *read* a chemical equivalent.
Page 265, line 10, *for* $= q\ V$ *read* $= q\ v$.
Page 271, line 9, *for* twenty-four inch, *read* 20 four-inch.
Page 273, line 2, *dele* Fig. 6.
Page 320, line 2 from the bottom, for *antè* read vol. iii.
Page 327, line 13, *for* 24″ *read* 54″.

The notices of papers read before the Royal Society referred to in vol. xviii. p. 547 note, will be found in the present volume, p. 240.

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

JULY 1841.

I. *On the Spontaneous Evolution of Sulphuretted Hydrogen in the Waters of the Western Coast of Africa, and of other Localities. By* J. FREDERIC DANIELL, *For. Sec. R.S., Prof. Chem. in King's Coll., London, &c.**

MY attention was first directed to the subject, which I shall have the honour of submitting to your notice this evening, by the Lords of the Admiralty, in April 1840, who sent me eight specimens, and afterwards two additional specimens, of water from the mouths of the rivers on the western coast of Africa, with directions to analyze them, for the purpose of discovering, if possible, the cause of the rapid decay of the copper-sheathing of ships employed upon those stations.

Of the comparative duration of the metal in the vessels of the Royal Navy I have not been informed; but the evil complained of is well known also in the merchant service; and upon inquiry of one of the largest copper-smelters in South Wales, he assures me, that "the experience of between thirty and forty years has led his mind to the conclusion that sheathing copper will be as much or more injured on a nine months' voyage to and along the coast of Africa, as by the wear of from three to four years on any other trade."

The first water which I examined was from the river at Sierra Leone, taken at three miles from the mouth. Upon drawing the cork of the bottle it was found to smell very strongly of sulphuretted hydrogen.

---

* Communicated by the Author; being the substance of a Lecture delivered to the Members of the Royal Institution on the 21st May, 1841.

The first idea which occurred to me was that which seems generally to have prevailed upon similar occasions, viz. that this gas was generated from some change which had taken place in the water after it had been bottled, from the decomposition of some animal or vegetable substance; but a little consideration showed that this explanation was quite inadmissible, inasmuch as the sediment from the whole bottle did not exceed half a grain, the water being perfectly bright, and the salts upon evaporation *snow-white*; and the water became perfectly sweet a very short time after it had been exposed to the air.

Indeed the common prejudice regarding the unlimited quantity of sulphuretted hydrogen generated by putrescence is perfectly untenable, and is founded solely upon its disagreeable odour. The fact is, that the quantity of sulphur in animal matter is very small, and the nauseous smell is by no means an infallible criterion of the existence of the gas.

As a natural product, sulphuretted hydrogen has hitherto been known chiefly as an ingredient in certain mineral waters, such as those of Harrowgate and Aix la Chapelle; the former of which contains, per gallon, 18·4 cub. in., the latter 44·0. The comparatively small springs which yield these waters are most carefully preserved by their proprietors, on account of their medicinal virtues, and the profits which are derived from their use.

The generation of the gas with which such waters are impregnated, has been usually attributed to some unknown action upon pyrites and other sulphurets of the metals in the interior of the earth, and it is pretty generally ascribed to volcanic action. It has also been generally known that sulphuretted hydrogen is produced by processes of decay or fermentation, in which large quantities of animal matters are concerned.

To the natural sources of this gas must now be added the estuaries of many large rivers and immense tracts of the Ocean in their immediate vicinity.

The results of the analysis of the African waters, sent to me from the Admiralty, embracing an extent of fifteen or sixteen degrees of latitude, are as follows:—

Each bottle was properly corked and sealed, and contained about three imperial pints; the water in all was perfectly bright, and had deposited very little sediment.

The first which I examined was labelled, "Water from the river at Sierra Leone, taken at three miles from the mouth, by Her Majesty's brigantine *Dolphin*, at low water, spring tides, on the 24th day of September, 1839, during the rainy season. (Signed) "Edward Holland, *Lieut.-Com.*"

Upon drawing the cork of this bottle, it was found to smell very strongly of sulphuretted hydrogen. The sediment in the bottle only amounted to 0·5 grain of vegetable matter. Specific gravity to 1018·5. The results of the analysis, calculated for the imperial gallon:

Sulphuretted hydrogen .	6·18 cub. in.	
Chlorine	943·14 grs.	
Sulphuric acid . . .	82·70	
Lime	19·14	
Magnesia	27·68	
Magnesium	32·71	
Sodium	563·33	
		1668·70

There was also a trace of potassa in this water.

The actual amount of dry salts obtained by evaporation, was 1696·0 grains. The difference between this and the results of the analysis is not more than usual in similar cases, and arises from the impossibility of determining the exact mode in which the several acids and bases are combined in the water, and from the difficulty of drying the salts without the decomposition of a portion, at least, of some of them.

2. "Water from the river Volta, taken [at sea] twenty-eight miles from the mouth, bearing W.N.W., by Her Majesty's schooner *Fair Rosamond*, latitude 5° 37′ north, longitude 1° 10′ east, on the 4th of September, 1839; season not rainy."

This water also smelt very strongly of sulphuretted hydrogen; the sediment in the bottle did not exceed 0·3 grain of vegetable matter. It contained, per gallon,

Sulphuretted hydrogen	6·99 cub. in.
Chlorine	1411·68 grs.
Sulphuric acid	92·47
Lime	14·75
Magnesia	35·70
Magnesium	12·46
Sodium	916·20
Potassium	a trace
	2483·26

Specific gravity, 1025·4

Amount of salts from evaporation . 2480·0

3. "Water from the river Bonny, taken at anchor off the

town in the river, by Her Majesty's schooner *Fair Rosamond*, on the 9th of October, 1839, about the conclusion of the rainy season."

This water smelt slightly of sulphuretted hydrogen, and the sediment in the bottle weighed only 0·4 grains, and consisted of vegetable matter. The results of the analysis were, per gallon,

Sulphuretted hydrogen	1·21 cub.in.
Chlorine	970·92 grs.
Sulphuric acid	92·10
Lime	17·36
Magnesia	33·65
Magnesium	47·11
Sodium	553·06
Potassium	a trace
	1714·20
Amount of salts from evaporation	1718

Specific gravity, 1019·0.

4. "Water from the river Mooney, which empties itself in the north-east part of Corisco Bay, taken from about a mile inside the mouth, by Her Majesty's brig *Nautilus*, September 4th, 1839. Rain had fallen, but the rainy season cannot be considered to have set in."

This water did not smell of sulphuretted hydrogen, nor did it afford any trace of that gas upon analysis; the total amount of sediment in the bottle did not exceed 0·1 grain. It contained, per gallon,

Chlorine	1184·11 grs.
Sulphuric acid	109·80
Lime	14·17
Magnesia	44·78
Magnesium	28·54
Sodium	732·32
Potassium	a trace
	2113·72
Amount of salts from evaporation	2104

Specific gravity, 1022·5.

5. "Water from the river Gaboon, taken at four miles above Parrot and Konicky Island [eight miles up the river], by Her Majesty's brig *Nautilus*, September 10th, 1839. Rain

had fallen, but the rainy season was not considered to have set in. Latitude 0° 15′ north, longitude 9° 33′ east."

This water afforded no traces of sulphuretted hydrogen. The sediment in the bottle weighed 0·2 grain.

Chlorine	1130·75 grs.
Sulphuric acid	120·08
Lime	23·05
Magnesia	43·58
Magnesium	35·41
Sodium	683·00
Potassium	a trace
	2035·87
Amount of salts from evaporation	2060·0

Specific gravity, 1023.

6. "Water from Cape Lopez Bay, taken by Her Majesty's brig *Nautilus*, September 28th, 1839, when the Cape bore W. by N. about ten miles. The rainy season had commenced."

This water smelt very strongly of sulphuretted hydrogen. The sediment in the bottle weighed only 0·1 grain, and consisted of vegetable matter. It contained, per gallon,

Sulphuretted hydrogen	11·69 cub. in.
Chlorine	1467·37 grs.
Sulphuric acid	115·20
Lime	23·21
Magnesia	41·02
Magnesium	28·44
Sodium	921·60
Potassium	a trace
Iodine	a trace
	2596·84
Amount of salts from evaporation	2576·00

Specific gravity, 1026.

7. "River Congo,—water taken off Shark's Point, at the entrance of the river, by Her Majesty's sloop *Wolverine*, on the 11th of November, 1839, four days before the customary rains, but light rains having already taken place."

This water smelt very slightly of sulphuretted hydrogen. The sediment in the bottle weighed 0·4 grain, and consisted of vegetable matter. It contained, per gallon,

Sulphuretted hydrogen	0·67 cub. in.
Chlorine	106·11 grs.
Sulphuric acid	2·30
Sodium	70·00
	178·41

And small quantities of other bases.

Amount of salts from evaporation . 188

Specific gravity, 1002·0.

This is the only case in which the salts were discoloured by vegetable extractive matter.

8. "River Congo,—water taken about thirty-five miles up that river, by Her Majesty's sloop *Wolverine*, on the 11th of November, 1839, four days before the customary rains, but light rains having already taken place."

This water contained no sulphuretted hydrogen, and the sediment in the bottle was only 0·1 grain.

The amount of saline matter was only eight grains per gallon, and consisted of the chlorides of sodium, and magnesium, and sulphate of soda, chiefly.

Specific gravity 1000·3.

"Water from the river Bango, taken [at sea] at forty miles distance from the mouth, by Her Majesty's schooner *Fair Rosamond*, on the 26th of December, 1839, in latitude 8° 33′ south, and longitude 12° 41′ east." The water emitted a very strong smell of sulphuretted hydrogen. It was tolerably clear, but contained a little gelatinous matter which resembled spawn of fish. The sediment of the whole bottle, however, when dried, only weighed fifteen hundredths of a grain.

The results of the analysis, calculated for the imperial gallon, were as follows:—

Specific gravity, 1026·4.
Sulphuretted hydrogen, 4·35 cub. in.
Dry salts, 2736 grs.

Consisting of chlorine 1513, sulphuric acid 128, neutralized by bases, which have not yet been quantitatively determined, but consisting of sodium, magnesium, calcium, &c.

"Water taken by Her Majesty's schooner *Fair Rosamond*, off the Bango and Dande rivers, latitude 8° 29′ south, longitude 12° 33′ east, on the 29th of December, 1839."

Results of analysis,—specific gravity, 1026·7.

There was no odour of sulphuretted hydrogen in this water, neither was any detected by tests. The quantity of dry saline matter, per gallon, 2624 grains, consisting of chlorine 1430, sulphuric acid 125·4, neutralized by the same bases.

It is difficult to conceive how such a striking and important fact as the impregnation of the waters of the Ocean, upon such a long line of coast, with this deleterious gas, should so long have escaped observation.

Upon searching for evidence of a similar phænomenon having been observed before, I have found in the *Phil. Trans.* for 1819, a memoir by the late Dr. Marcet, "On the Specific Gravity and Temperature of Sea Waters in different parts of the Ocean, and in particular Seas, with some account of their saline contents." Out of sixteen specimens which he examined he found one which was brought by Capt. Basil Hall from the Yellow Sea in the Chinese Ocean, which, from the account which he has given, must probably have been as highly charged with sulphuretted hydrogen as those from the coast of Africa; and he observes, "there is something in the development of sulphur in sea water which is by no means well understood."

He also noticed that a specimen obtained by Mr. Schmidtmeyer, going to South America, from lat. 10° 50′ N., long. 24° 26′ W., had an hepatic smell, and had blackened the bottle in which it was contained.

Since the report, which I had the honour to make to the Admiralty, Sir William Burnett has been kind enough to furnish me with the analysis, by Mr. Garden, of some bottles of water, collected by Dr. Mc William, of Her Majesty's ship *Scout*, in March 1839, in the river Bonny, of which the following are the particulars:—

1. Water from the river Bonny, taken half a mile inside the mouth, on the 12th of March, 1839, just before the commencement of the rainy season.

Each imperial pint of this water contained 245 grains of saline matter, consisting of the following ingredients:—

Sulphate of Magnesia
Do. of Lime
Muriate of Magnesia
Do. of Soda

Sulphuretted hydrogen, ·680 cub. in. (5·44 cub. in. per gall.)

2. Water from the mouth of the river Lagos, lat. 6° 20′ N., long. 3° 30′ E. nearly.

Each pint yielded 240 grains of saline matter, consisting of the same ingredients as those above mentioned; also Sulphuretted hydrogen, 1·844 cub. in. (14·752 cub. in. per gall.)

3. Water from the river Bonny, taken off Ju-ju Point, on the 12th of March, 1839, about one mile and a half within the mouth.

One pint yielded 200 grains of saline substances, as above, and Sulphuretted hydrogen, ·505 cub. in. (4·04 cub. in. per gall.)

4. Water from off Grand Bonny, about three miles inside the mouth of the river.

One pint yielded 208 grains of the same salts as above, and Sulphuretted hydrogen, 3·500 cub. in. (28·0 cub. in. per gall.)

These results appear to have attracted no particular attention at the time when they were obtained. The old hypothesis of the corruption of the specimens probably sufficed for their explanation upon this as upon other occasions.

Thus there can be no doubt of the important fact of the impregnation of the waters, upon the western coast of Africa, with sulphuretted hydrogen, to an amount, in some places, exceeding that of some of the most celebrated sulphur springs in the world; and of the injurious effect of such impregnation upon the copper sheathing of ships, you will be convinced by the experiments upon the table.

Were any further evidence wanting, it would be found in the state of the copper of the *Bonetta*, which lately returned from the coast of Africa, and three sheets of which were sent to me from the Admiralty for examination.

Nos. 1 and 2 were pretty uniformly covered on the outside with a green crust; and on the inside, as evenly, with a black crust of equal thickness. They were very thin in parts, and here and there eaten into holes.

No. 3 was in a much worse state, very thin, and eaten into large holes. In most parts it was easily broken by the fingers; one of the holes, of an irregular shape, measured eighteen inches in length by four inches and a half in width. This sheet was covered with green crust chiefly, on both sides; but there were evident traces of the black crust on the inner side.

Upon analysis the black crust was found to consist of sulphuret of copper, and the green of oxy-chloride of copper.

There can be no doubt that the injury to the copper arose, primarily, from the sulphuretted hydrogen. The gas appears to have penetrated to the inner side of the copper, where in Nos. 1 and 2 it has been protected from the further action of the sea water; by which, on the outside, the sulphuret was converted into chloride of copper. This conversion appears to have taken place on both sides in No. 3, from the sea water having penetrated to the under side in consequence of its greater corrosion.

That the establishment of this fact is of some importance in a mercantile point of view, I think I shall be able to convince you by two anecdotes which I will now narrate.

Not many years ago a new copper company set up a smelting establishment and brought their copper to market: some merchants purchased sheathing of them, coppered their ship, and sent her to the coast of Africa. Not many months after she returned to this country with the copper in the same state as that of the *Bonetta*. The merchants said—the copper-smelters were inexperienced hands—they did not know their business—the sheathing was improperly made; and they brought an action against the Company, who defended it.

Upon the trial some of the most eminent scientific men of the day gave evidence that there was nothing in sea water which could produce such rapid decay of the copper, and the jury, in consequence, brought in a verdict for the plaintiffs.

Now contrast this with what has happened to me within the last two months. An eminent copper manufacturer of South Wales, who had heard nothing of the investigations in which I had been engaged, came to me with two samples of copper which he wished me to analyze. The one was of new metal, and the other part of the sheathing of a ship which had just returned from Africa, after a voyage of a few months, the copper being in a state of utter decay. He stated that the merchants to whom the vessel belonged had brought an action against him on the plea that the copper was imperfect, and he wished for my evidence upon the subject, as he well knew that the copper was perfectly good. Instead of entering upon the analysis I gave him a copy of my report upon the waters of the western coast of Africa, which he sent to the merchants, and nothing further has been heard of the action.

But it may perhaps be said that little good will arise from pointing out the evil, unless we are prepared to propose some remedy for it; not that I agree to this, for the existence of the sulphuretted hydrogen is so readily tested, even by the roughest hands, that nothing can be easier than to ascertain and avoid the localities in which it prevails; motives for which course I shall presently mention, of greater weight even than the preservation of the copper.

But I think that the remedy is certainly within our command. The principle of protection proposed by Sir H. Davy is quite applicable to it, with some additional precautions suggested by this newly-discovered destructive agent, which had escaped his notice.

It is well known that his experiments were conducted chiefly with zinc and iron, as the active elements of protection; and he was led ultimately to the adoption of cast iron, "as the substance which is cheapest, most easily procured, and likewise most fitted for the protection of the copper*."

But this is not the case with regard to sulphuretted hydrogen; for as you will see by reference to the experiments upon the table, copper is much more acted upon by this substance than iron, the latter being protected by the former; and the fact is, that a piece of iron attached to copper increases the corrosion of the latter.

---

* Phil. Trans., June 1824, p. 243; [or Phil. Mag. First Series, vol. lxv., p. 204.]

Zinc, on the contrary, protects the copper not only from the action of the chlorides in sea water, but also from the sulphuretted hydrogen. This I have ascertained by the experiments before you, and you will find that the results are in perfect accordance with the electric order of these two metals in solutions of the hydrosulphurets, as given by Dr. Faraday in his last beautiful number of Experimental Researches in Electricity.

In the table which he gives, iron stands far above copper in electro-negative order, and zinc below it; lead is also above zinc; while in the usual acid solutions both zinc and iron stand below that metal*.

Now I have long been of opinion that the experiment of voltaic protection in the Navy was much too lightly abandoned upon the first appearance of an unforeseen difficulty, and that under circumstances otherwise the most encouraging.

This abandonment, you are aware, arose from what might be called over-protection, by which the attachment of weeds and zoophytes to the ships' bottoms was found to be encouraged. Earthy deposits were formed, and to these the weeds and shell-fish attached themselves.

The remedy for this appears to me to be obvious: instead of keeping the protectors always in contact with the copper, let them be insulated, and let them only be brought into metallic contact when occasion may require. This might readily be done by means of a bolt or bar, forming in one position a continuous conductor between the two metals, and in another breaking the connexion: this might always be at the command of the proper officer of the ship. Nothing could then be easier than to throw off the protection when the ship is in harbour, or in situations particularly liable to deposits; or to restore it upon going to sea, or arriving in latitudes where sulphuretted hydrogen might be found to exist.

But the protectors should invariably be of zinc, which would preserve the copper not only from the effects of sea water generally, but from the more destructive agency of sulphuretted hydrogen, which I shall presently give you my reasons for concluding not only prevails upon the western coast of Africa, but in other situations where it has never yet been suspected. Indeed I incline to believe that it would only be found necessary to use protection in sulphuretted waters, and that the action of the chlorides alone might not be more than sufficient to preserve the copper from deposits.

---

* Phil. Trans., 1840, p. 113.

Another motive for this change may be found in an observation of Sir H. Davy, viz. that a "common cause of the adhesion of weeds or shell-fish is the oxide of iron formed and deposited round the protector. In the only experiment in which zinc has been employed for this purpose in actual service, the ship returned, after two voyages to the West Indies and one to Quebec, perfectly clean. The rudder, which was not protected, was corroded in the usual way*."

But it is impossible not to speculate upon the origin of the deleterious gas which has thus been found to contaminate the sea upon the western coast of Africa, in such enormous quantities through an extent of more than sixteen degrees of latitude, and reaching in places forty miles seawards, making altogether an area of 40,000 square miles in extent.

Volcanic action seems naturally to suggest itself, but is negatived by the absence of any other indications of such action along this line of coast; and I think that I shall be able to convince you, by the evidence of experiment, that the real cause may be found in the mutual reaction of the immense quantities of vegetable matters, which must be brought down by the intertropical rivers, and the sulphates of the sea water.

The idea was suggested to me by a memoir of my friend Dr. Malcolmson, in the Geological Transactions, who speculates upon the origin of sulphuretted hydrogen in the saline lakes of different parts of the world, being "the decomposition of the sulphates in the water by the carbonaceous matter of vegetables." I tested this hypothesis by experiments in the following way:—

On the 2nd of November, 1840, I placed a quantity of newly-fallen leaves in three glass jars capable of holding about a gallon and a half of water.

No. 1. Upon the first I poured about a gallon of New River water.

No. 2. Upon the second I poured about the same quantity of the same water, in which three ounces of common salt had been dissolved.

No. 3. Upon the third, the same quantity of water, in which three ounces of crystallized sulphate of soda had been dissolved.

The three jars were then placed in a warm chamber, the temperature of which varied from about 70° to 110°, and the water was filled up from time to time, as it evaporated, and the mixture well stirred.

---

* Phil. Trans. 1826, p. 420; [or Phil. Mag. Second Series, vol. i., p. 198.]

Upon examining them on the 5th of February, 1841 (three months), the following was found to be the state of the jars:—

No. 1 had a very disagreeable odour, but produced no change whatever upon paper soaked in acetate of lead.

No. 2 was perfectly sweet, and possessed, indeed, a rather agreeable odour. It produced no effect, of course, upon the test paper.

No. 3 had a most insupportable sickening odour, much worse than that of pure sulphuretted hydrogen, and instantly blackened paper soaked in acetate of lead, throwing down sulphuret of lead with a metallic lustre.

You will have an opportunity of observing, by the specimen upon the table, that the evolution of the gas is at this moment proceeding with increased energy.

Now, the analysis of sea waters generally, and these analyses in particular, show that a large proportion of sulphates is always present in them, and there is no doubt that extensive mud-banks must be formed at the mouths of the African rivers, within the tropics, consisting chiefly of vegetable detritus, in the exact state which is most favourable to this action.

Since my report to the Admiralty upon this subject, I have seen a paper in the *Annales de Chimie* for July 1840, by Dr. Amédée Fontan, upon the Mineral Waters of Germany, Belgium, Switzerland, and Savoy, in which he suggests that the presence of sulphuretted hydrogen in those waters may be owing to the decomposition of the sulphates which they contain by vegetable matters, remarking that many of them which contain little of that gas at their sources, acquire more of it by their flow through the soil. There can be little doubt of the correctness of this opinion.

A curious fact has also been brought to my recollection by my friend Mr. Fownes, with regard to a spontaneous change which a solution of litmus undergoes when excluded from the air. It becomes of a brown colour, but still it is not spoiled, for the colour is restored by exposure to air.

M. Vogel (Ed. Journ. 31, 157), who inquired into this curious fact, found that the solution always contains sulphate of potassa, which becomes gradually decomposed with the generation of sulphuretted hydrogen, to the deoxidating power of which the effect is owing. A few drops of sulphuretted hydrogen solution produces the same effect in a few days; the solution becomes brown, but speedily recovers its colour upon contact with air. This case is the more interesting, inasmuch as the gas never exists in sufficient proportion to be discoverable by the usual tests; nevertheless it acts in these minute quantities with great energy.

But now a much more important and interesting question than that of the preservation of the copper sheathing of ships forces itself upon our attention, and that is, whether the existence of this deleterious gas in the atmosphere, which must necessarily accompany its solution in the waters, may not be connected with that awful miasma which has hitherto proved so fatal to the explorers and settlers of the deadly shores of Africa; and whether, if so, science may not suggest something to palliate an evil which is so dreadfully opposed to the progress of civilization in those parts.

When this matter was first brought under my consideration, I was surprised that the nauseous smell which must necessarily be evolved from water impregnated with this gas, at so high a temperature as that of the equinoctial regions, had not been noticed. I have in consequence turned to some of the accounts of the late travels in Africa, to seek for evidence upon the subject; and in the narrative of an expedition into the interior of Africa, by the river Niger, by Macgregor Laird and R. A. B. Oldfield, I found the following important observations:—

"The principal predisposing causes of the awful mortality, were in my opinion the sudden change from the open sea to a narrow and winding river, the want of the sea breeze, and the prevalence of the deadly miasma, to which we were nightly exposed from the surrounding swamps. *The horrid sickening stench* of this miasma must be experienced to be conceived: no description of it can convey to the mind the wretched sensation that is felt for some time before and after daybreak. In those accursed swamps, one is oppressed not only bodily but mentally with an indescribable feeling of heaviness, languor, nausea, and disgust, which requires a considerable effort to shake off."

Now, these observations were made in the very locality from which some of the first waters which I examined were taken, and nothing more is wanting to identify the cause of the rapid decay of the ship's copper with that of the mortality of the climate.

It has been experimentally found, that so small a mixture as a fifteen hundredth part of sulphuretted hydrogen in the atmosphere, acts as a direct poison upon small animals, and the sensations of languor and nausea, described by Mr. Laird, are exactly those which have been experienced by persons who have been exposed to the deleterious influence in small quantities.

The symptoms, in cases where this gas is breathed in a state of concentration, well known to medical men, are sudden weakness, and all the signs of asphyxia: the individual becomes suddenly weak and insensible; falls down, and almost immediately expires. When the exposure has been too slight to cause serious mischief, the individual is affected with sickness, colic, imperfectly defined pains in the chest, and lethargy.

Now, can it be deemed at all improbable, that an agent which is capable of acting with this severity as a direct poison, when mixed in no very high proportion with the atmosphere, should in still less quantities greatly aggravate symptoms of morbific action, which may possibly have their origin in other causes?

In the very expedition, from the account of which I have already quoted an extract, a circumstance occurred which is almost an experimental confirmation of these views. The first sickness and death in that expedition began at Cape Coast Castle; three died before entering the river, and the great mortality took place before they reached Damuggoo at the extreme upper end of the Delta, where they only arrived after a voyage of thirty-six days, from the 11th of October to the 16th of November, or twenty-seven from their entrance of the river Nún.

Now it is worthy of remark, that just before entering the river, in "breaking out" the hold to lighten the vessel, it was discovered that the cause of a "disagreeable vapour, from which they had long suffered, was, that the bags containing the cocoa had rotted, and the cocoa had fallen into the salt bilge-water and there become putrid." Here, then, were the very ingredients for generating sulphuretted hydrogen to a great extent; the lamentable consequence has been before alluded to, namely, three deaths before reaching the river. There can indeed be no doubt that the disagreeable effluvium of bilge-water, which has been carelessly left undisturbed for a long time, is owing to similar decomposition.

It is doubtless the same circumstance which renders Mangrove swamp so notoriously pestilential in all parts of the torrid zone. The tree only thrives in salt water, and its decayed foliage is admirably adapted to act upon the sulphates; and it accounts for the observation, that malarious fevers diminish as we recede from the coast, although swamp and rank vegetation may still prevail.

The close investigation which I have since given to the subject, more and more convince me that the worst cases of *malaria* are generally connected with the presence of sulphuretted hydrogen.

There is a paper in the twenty-ninth volume of the *Annales de Chimie*, p. 225, by Signor Gaetano Giorgini, which offers the strongest possible confirmation of my opinion.

"The observation of Signor Giorgini has been drawn to the state of the atmosphere in the neighbourhood of certain marshes on the borders of the Mediterranean; and by reference to historical data, and various documents, he has proved the great importance which attaches to the circumstance of their being, at times, in communication with the sea, so as to have a mixture formed between their waters and that of the sea. Both ancient and modern authors have announced the fatal effects produced in the neighbourhood of marshes by such mixture, and a local belief of the same thing is very common and strong.

"On the south of the Ligurian Apennines is a marshy shore, bounded on the west for twelve miles by the Mediterranean, on the south by the river Serchio, and on the north by the river Frigido, a torrent commencing at the foot of the Apennines, in the state of Massa di Carrara, running three or four miles over the land, and then falling into the sea. The plain is from two to four miles wide, and is traversed by a few short torrents or streams; among these are the rivers Camajore and Pietra Santa, which divide the plain into three separate basins. The rain and spring waters which flow into the three basins mentioned, are slowly discharged into the sea by natural or artificial canals, penetrating the sand-bank, which exist on the sea-side.

"The level of these stagnant waters is between that of high and low water in the neighbouring sea; there being but little difference between these two points in this part of the Mediterranean. In this state of things, formerly, when the waters of the sea arose from any circumstance (unless the waters of the marshes were very high), they used to return up the ditches, fill the basins, and inundate the country to the foot of the mountains; and with a north-west wind, the waves used to penetrate with force to the interior. The mixture of fresh and salt water thus formed, and which, in summer, was rarely changed, became corrupt, and spread infection over the neighbourhood of the most destructive kind.

"In this way the effects of the malaria were reproduced annually in the neighbouring country, with all their peculiar horrors: the population, though small, presented feeble infants and diseased men, old age being unknown there. All attempts to avoid the scourge, by living on the hills, or in the interior, and frequenting the plain when the business of cultivation essentially required it, were vain; they fell victims to the extensive influence, and such being the effects upon the inhabitants of the country, much more rapidly did a stranger suffer from the deleterious atmosphere; one single night, in the months of August and September, causing inevitable death to the incautious traveller who should stay so long in this infested country.

"Such was the state of things until 1741. Previous to that time, Gemignano Rondelli, Eustachio Manfredi, and Bernardino Zendroni had successively insisted upon the necessity of excluding the sea from these marshes; and, in 1740–41, a sluice, with folding doors, competent to give emission to the waters of the marsh, but prevent the sea from entering, was constructed at the mouth of the Burlamacca. The most complete and unexpected success immediately followed upon, and has continued with, this work. The year after its completion there were no appearances of the terrible maladies which previously appeared every year. The inhabitants soon recovered health, and the land being very fertile, the population rapidly increased, and is increasing at this moment. Viareggio has become a considerable town, and so completely has all suspicion of its insalubrity disappeared, that the first families of the city of Lucca have for years built their summer seats there. Notwithstanding the success of the precautions taken at this part of the coast, the neighbouring parts were long left a prey to the destroying influence of the mixed marsh-waters; and the inhabitants around the basins of Motrone and Perotto were not considered until the year 1804. In the years 1809, 1810, 1811, similar means were taken, with the best effects to the inhabitants of Montignosso and the vicinity; and, in 1812, a sluice was constructed on the Cinquale, which perfected the arrangements in this part, and made a large portion of country equally healthy with Viareggio. To complete the arrangement, it was now only required to guard the ditches of Motrone and Tonfalo with sluices; the former was finished in 1819, and the latter in 1821. Since that time the diseases of malaria have ceased so entirely at all points, that no other dangers are now incurred regarding the insalubrity of the atmosphere than such as may arise from neglect of these sluices, which the inhabitants of the country should regard as their palladium."

I should weary you were I to multiply examples of the existence of the worst forms of malaria in places which we now know combine all the necessary circumstances for the generation of sulphuretted hydrogen.

In Mr. Darwin's interesting Journal of the Voyage of the Adventure and Beagle, he speaks of several such places, especially in Peru, in connexion with the well-known effects of miasma, but not with any suspicion of the real origin of the evil. He mentions repeatedly the efflorescence of the sulphates of magnesia and soda upon the soil, and that the mud of the saline lakes is "black, and had a fœtid odour." The inhabitants suffer in such situations from the worst attacks of ague. He remarks, p. 447, "The attacks of illness which arise from miasma never fail to appear most mysterious. The miasma is not always produced by a luxuriant vegetation with an ardent climate; for many parts of Brazil, even where there are marshes and a rank vegetation, are much more healthy than this sterile coast of Peru."

"So difficult is it to judge from the aspect of a country whether or not it is healthy, that if a person had been told to choose within the tropics a situation appearing favourable for health, very probably he would have named this coast."

Its peculiarity consists in the saline efflorescence upon its soil, of which the sulphates of magnesia and soda constitute a very great proportion.

May not the jungle fever in India depend for much of its malignity upon the same cause? The soil in many parts abounds not only with the nitrates of potassa and soda, but the sulphates of soda and magnesia; these become washed down by the periodical rains, and mingling with the decaying leaves, the mutual reaction takes place.

Is it not worthy again of the most exact inquiry, whether the fevers which periodically afflict the cities of New York and Charlestown, in America, may not be connected with the mixture of animal and vegetable substances with the sea water in their lower districts, where they usually originate; and whether an attentive examination will not prove, that the same impregnation of sulphuretted hydrogen, which we have established upon the African coast, exists at the mouth of the vast rivers of the American continent? Indeed I have been informed by an officer high in the naval service, that during the war instances of the rapid decay of ships' copper, similar to that upon the African, were noticed upon the West-Indian station.

To bring the matter nearer home. Is it impossible that the minor insalubrity of parts of our own coast, such as that of Essex, may have some reference to the same causes acting in minor degrees. The mud of the river Thames may surely produce the same decompositions of the sulphates in the sea water as that which we have traced in other places. I find that there is a prevalent idea, which deserves investigation, that the ships in the Medway are liable to greater waste of their copper than those at Woolwich, or other places on the Thames.

Within the past week I have seen in the number of Liebig's *Annalen* for January last, that Dr. Clemm examined some water taken up in December 1839, in the open sea off Barmouth, North Wales; also from Aberystwyth and Tenby in South Wales; and he observes, "In some of the bottles from the English coast the large proportion of sulphuretted hydrogen was very striking: both in smell and taste it was converted into a strong sulphurous water." He seems to think that the accidental presence of some of the sealing-wax with which the bottles were closed, may have had something to do with the generation of the gas.

By the kindness of Sir Isambard Brunel, I have been favoured with the particulars of a very remarkable phænomenon of this kind, which attended the progress of the Thames Tunnel, and which proved of great annoyance, and was the cause of great suffering to some of the workmen. This was the evolution of great quantities of carburetted hydrogen mixed with sulphuretted hydrogen gases. In 1837 the smell was extremely fœtid. "It frequently rushed out from between the small boards of the shield, used to scour the ground with great force, bursting into a flame, which sometimes extended over some feet, and was generally accompanied by a loud roaring or hissing noise."

In 1838, the sulphuretted hydrogen was unaccompanied by the great quantity of carburetted hydrogen with which it had been previously mixed, but its effects upon the men were very severe, producing sickness, giddiness and fainting, pains in the head, and in one case delirium.

The source of this enormous production of gas may probably have been the deposits of animal and vegetable matters in the mud of the river, acting however by the sulphates, which are contained in the former, in a manner perfectly similar to that which we have traced to the sulphates in the sea water.

And here again it may be asked, as with regard to the injurious effects of sulphuretted hydrogen upon the copper of ships, can science indicate a remedy, as well as point to the cause of the disease? And again I would reply, that by furnishing an easy method of detecting the evil, she furnishes you with timely warning to fly from the infected regions. No vessel should be allowed to cast anchor or linger in sulphuretted waters. But if paramount duty should oppose itself to such a course, we have a certain remedy to propose. You have seen how instantly chlorine destroys the gas. Chlorine and sulphuretted hydrogen cannot coexist together. Plentiful fumigations of chlorine would therefore infallibly prevent the deleterious effects; and the antidote is at once cheap, and incapable, under proper management, of producing any injurious effects to counterbalance its advantages.

The Lords of the Admiralty have received these suggestions with indulgence, and have given instructions to their cruisers upon the African coast to test the waters at regular intervals. They have also abundantly supplied the African expedition with the means of chlorine fumigation; and I have the gratification of knowing that the views which I have now had the honour of submitting to you, have tended to give confidence not only to the gallant band who have devoted themselves to one of the most disinterested enterprises which ever emanated from pure Christian charity, but to the numerous friends who wait the result with anxiety.

II. *Abstract of recent Researches on the quantity of Heat evolved in Chemical Combination, particularly those of* MM. Dulong *and* Hess.

FARADAY having established the law of definite electrolytic action, and thus connected together the two principles under which scientific chemistry at the present day is organized, the abstract and the numerical, the electro-chemical theory and the law of equivalents, it became an interesting point to examine whether affinity possessed the same definiteness of character in relation to other physical agents, and especially to heat, the influence of which on affinity is even more remarkable than that of electricity, and with whose operations indeed chemistry itself, until a comparatively recent date, was identified.

Investigations tending to this end had been long since entered on. The measurement of the quantity of heat evolved in combustion was, independent of all relations to scientific theory, a problem of such technical importance, that it has occupied from time to time all those philosophers who devoted themselves to that branch of science. Their results, frequently discordant, but still often useful, have been considered, especially those of Despretz, as indicating that the heat evolved was proportional to the quantity of oxygen which entered into combination.

For some time before his death, M. Dulong had been occupied with researches of this kind, and the attention of the Academy of Sciences having been directed thereto by M. Hess, who had had conversations with Dulong on the subject, the manuscripts which he left behind were examined by Arago, and the following results obtained. Dulong had made an extensive series of experiments on the quantity of heat evolved in combustion, but did not write out any full description of his apparatus or methods; M. Cabart, however, who had been working in Dulong's laboratory, was able to describe the form of calorimeter which had been used. A rectangular box in red copper, of 25 centimetres in depth, 7·5 in breadth, and 10 in length, is the space in which the combustion is carried on; the oxygen or other gas supporting the combustion is introduced by a tube, and the products of combustion carried off by another. For the precise arrangement of these tubes, which are complicated in their form, for the purpose of receiving the thermometers by which the temperatures are read off, we must refer to the original. This box is immersed in a vessel holding eleven quarts of water. In this box gases were burned by means of jets, of various sizes, according to the combustibility of the gases. The liquids were burned by means of a few cotton threads in a glass tube closed at one end: it is not known how they were set on fire. The solid bodies (except iron, which was used in coils of fine wire) were in the state of powder, and were set on fire by a bit of amadou.

The following tables give the most important results obtained by Dulong; the unity of calorific effect employed being the quantity of heat necessary to raise the temperature of a gramme of water one centigrade degree at ordinary temperatures.

Substances and quantities employed.	Quantity of heat evolved in combustion.		
	Max.	Min.	Mean.
One litre of hydrogen, five experiments	3120	3075	3102
One litre of pond gas, four experiments	9948	9317	9587
One litre of olefiant gas, five experiments	15576	15264	15338
One litre of carbonic oxide, three experiments	3202	3069	3130
Carbon, equivalent to one litre in gaseous state, four experiments	8040	7540	7858
Alcohol, one litre in vapour, two experiments.	14441	14310	14375

Substances and quantities employed.	Quantity of heat evolved in combustion.		
	Max.	Min.	Mean.
Oil of turpentine, one litre in vapour, one experiment	70607		
Æther, one litre in vapour, two experiments	33968	32738	33254
One litre of cyanogen, three experiments	12602	12080	12269
One gramme of sulphur, three experiments	2719	2452	2602

The following simple bodies evolved by combustion in oxygen for every litre of oxygen used, the following proportional quantities of heat:—

Bodies.	No. of Exps.	Max.	Min.	Mean.
Iron	2	6281	6152	6216
Tin	3	6790	6325	6503
Protoxide of tin	3	6611	6262	6405
Copper	3	3742	3503	3578
Suboxide of copper.	1	3130		
Antimony	5	5875	5348	5552
Zinc	3	7753	7378	7577
Cobalt	1	5721		
Nickel	1	5333		

These results want probably the great accuracy which Dulong would have given to them had he been longer spared to science; but they probably fluctuate round the true numbers. We cannot judge of what general conclusions he would have deduced from them; but one important consequence appears to follow naturally from the numbers now given, since it appears that the same quantity of oxygen does not evolve the same quantity of heat in combining with various bodies. Thus if we deduce from the first table the quantity of heat evolved by each substance in perfect combustion with a litre of oxygen, we obtain the following numbers:—

Hydrogen	6204
Carbon	7858
Pond gas	4793
Olefiant gas	5113
Alcohol	4792
Carbonic oxide	6260
Oil of turpentine	5043
Æther	5542
Cyanogen	6135
Sulphur	3744

Amongst the metals similar deviations occur.

Hess states, in his letter to Arago, that he had taken notes immediately after a conversation with Dulong, in which the latter said he had arrived at the following general laws:—

1st. That the quantities of heat evolved are (nearly) the same for the same substance, no matter at what temperature it burns.

2nd. Equal volumes of all gases, in combining with oxygen, evolve the same quantity of heat.

3rd. The same quantity of heat is disengaged with the same quantity of oxygen, whether it forms a compound R + O, or R + 2 O.

4th. The quantity of heat disengaged by different substances is very different.

These conclusions, however, are by no means completely borne out by the experimental results, so far as they have been recovered, except the third, which is certainly supported by the results with tin, protoxide of tin, carbon and carbonic oxide.

In commenting on these numbers, Berzelius has pointed out a remarkable consequence deducible from them; that is, that the heat evolved in the combustion of a compound gas is the same as that evolved in the combustion of its constituents. Thus,

One volume of pond gas consists of two volumes of hydrogen and half a volume of carbon (vapour). Mean.

Two volumes of hydrogen produce heat = 6204
Half a volume of carbon (vapour) . . . . . = 3929
——10133
The highest experimental number is . . . = 9948

One volume of olefiant gas consists of two volumes of hydrogen and one of carbon (vapour). Mean.

Two volumes of hydrogen evolve heat . . = 6204
One volume of carbon (vapour). . . . . . . = 7858
——14062
The mean experimental result is . . . . . . = 15338

One volume of oil of turpentine vapour consists of eight volumes of hydrogen and five of carbon (vapour).

Mean.
Eight volumes of hydrogen evolve heat . = 24816
Five volumes of carbon (vapour). . . . . . = 39290
——64106
The experimental number is . . . . = 70607

The differences here found are not probably greater than the limits of experimental error; their direction also is not constant.

The laws of phænomena are very frequently more easily examined and established when attention is directed to those cases in which they act with but moderate activity; the results, being less sudden and turbulent, become more easily subjected to accurate measurement and observation. It has been remarkably so with electricity; and this it is which gives peculiar value to the thermo-chemical researches recently entered upon by Hess and Andrews. In Poggendorff's *Annalen* in 1839, Hess communicated some results on the admixture of oil of vitriol and water, and announced that *when two substances unite in many proportions, the quantities of heat evolved in these acts of combination are multiple proportions of each other.* To the further development of this law he has since devoted himself, and obtained a great number of important results, which we will now proceed briefly to describe.

The instances which first conducted M. Hess to the detection of this law, were found in the quantity of heat evolved by the combination of oil of vitriol with water. The method first pursued was as follows:—The strength of the dilute sulphuric acid being determined, which did not evolve heat by any further addition of water, and quantities of oil of vitriol being diluted with water so as to bring them to various atomic constitutions, these were added to the quantities of water necessary to bring them to the standard first described, and the rise of temperature consequent on the mixture accurately determined. The two liquids (the water and atomically diluted oil of vitriol) had accurately the same temperature before mixture, and the weights were accurately known.

In order to compare the results given by a series of experiments, some arbitrary unit should be taken, and for this was selected one part of sulphuric acid considered as anhydrous; for instance, if the experiment was made with $SO_3 + 3HO$. Since the quantity of this last was known, the quantity of dry sulphuric acid therein contained was also known, and let it be marked 4. Then the heat evolved in the experiment, divided by 4, gives the quantity of water which should have been raised one degree of temperature by one part of anhydrous acid. In this way the results which follow, and which were announced by M. Hess in 1839, were obtained.

Acids.	Units of Heat.	Multiples.	Corrected Units of Heat.
$SO_3 + 6HO$	43·8	2	39·4
$SO_3 + 4HO$	67·2	3	60·4
$SO_3 + 3HO$	93·5	4	84·1
$SO_3 + 2HO$	132·6	6	119·34
$SO_3 + HO$	227·5	10	204·75

The fourth column contains the number of units of heat evolved after the results had been corrected for the specific heat of each acid used.

For this simple method of mixtures M. Hess subsequently substituted the use of a calorimeter, devised by him for the purpose. This instrument consists, 1st, of a box for holding water; inside of this is, 2nd, a cylinder, fitted, as well as its cover, with a broad edge; these edges are ground and fitted tight to each other by eight screws. The outer surface of the cylinder is fitted with projecting copper wings, which serve to agitate the water in the external box, leaving free, however, a space, which admits of the entrance of the thermometer. The cylinder is supported horizontally on its axis, and is fitted on one side outside of the box with a winch handle, by which it may be made to revolve rapidly.

The mode of using this instrument is as follows: the cylinder being taken out is placed steadily in a wooden support, and the cover screwed off. Whichever of the two liquids to be mixed occupies least volume, is to be then introduced into a glass or copper vessel and laid in the cylinder; the other liquid is to be then poured in, the cover fastened, and the cylinder laid in its place, surrounded by the water of the cylinder. On then turning the cylinder rapidly round by the handle, the inside vessel overturns, the liquids mix, and the heat evolved is imparted to the water, outside of which the temperature is accurately determined by a Collardeau thermometer. The change of method when one substance is solid can easily be appreciated.

By means of experiments conducted with this apparatus, it was found, after applying all necessary corrections, that

				Units of Heat.
$H O + S O_3$	combining with	$H O$	evolved	77·17
$H O + S O_3$	,,	$2 H O$	,,	116·7
$2 H O + S O_3$	,,	$H O$	,,	38·56
$S O_3$	,,	$x, H O$	,,	510·1

From these results, combined with those already given, the following final results may be deduced for the quantities of heat evolved in the formation of diluted sulphuric acid of definite constitution:—

Acid.	Heat evolved.	Multiples.
$S O_3 + H O$	310·4	8
$H O. S O_3 + H O$	77·86	2
$(2 H O + S O_3) + H O$	38·9	1
$(3 H O + S O_3) + 3 H O$	38·9	1
$(6 H O + S O_3) + x H O$	38·9	1
Total for $S O_3 + x H O$ =	504·96	13

And from hence the heat produced by mixing sulphuric acid of any strength, with an excess of water, may be computed.

Acid.	Theory.	Experiment.
SO_3	504·96	510·1
$SO_3 + HO$	194·5	204·75
$SO_3 + 2HO$	116·7	119·34
$SO_3 + 3HO$	77·8	84·1
$SO_3 + 6HO$	38·9	39·4

In extending these researches to the combinations of other bodies than water with sulphuric acid, M. Hess arrived at the remarkable result, that *when a combination takes place the quantity of heat evolved is constant, whether the combination be direct or indirect; whether it takes place at once or by successive stages.* This principle is supported upon evidence derived from an extensive series of experiments, to the description of which we shall now pass.

[To be continued.]

III. *Further Experiments on the Electricity of Effluent Steam.*

By W. G. ARMSTRONG, *Esq.*[*]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IN pursuing my experiments on this subject, I observed that the tendency of the steam cloud to evolve *negative*, instead of *positive* electricity, gradually increased as I continued to use the apparatus, until at length positive electricity rarely appeared in the jet, even when circumstances were most favourable to its development.

Conceiving that this gradual predominance of negative electricity in the steam might possibly be owing to the progressive oxidation of the metal in contact with the water, I examined the interior of the boiler; but no change appeared to have taken place in the surface of the metal, which was in the rough state in which castings come from the mould. I then washed out the boiler with water, but upon using it again the electricity of the steam remained negative as before. After this the boiler was washed out with a solution of potash, by which means I was agreeably surprised to find the positive electricity of the jet was completely restored. This remarkable result led me to try the effect of dissolving a little potash in the water from which the steam was generated, and by so doing the quantity of electricity was amazingly increased; so much so, that I was enabled to obtain from the apparatus upwards of thirty sparks, half an inch long, in a minute.

[*] See our preceding volume, p. 328.

Having thus discovered an instance in which the electricity was so strongly affected by the contents of the water, I proceeded to try the effects of other ingredients, and the following are the results I obtained:—

Soda had much the same effect as potash, rendering the electricity of the steam positive, but not occasioning quite so strong a development. A minute quantity of nitric acid produced the contrary effect, causing the steam to be *negatively* electrified. Muriatic acid had no perceptible influence; neither had sulphuric acid, not even when iron filings were contained in the boiler. Lime tended to produce positive electricity, but not powerfully. Common salt appeared to have no effect. Nitrate of copper rendered the steam negative, in nearly the same degree as nitric acid.

Potash, soda or lime used in excess, always occasioned what engineers call *priming*; which is the ejection of water from the boiler in conjunction with the steam. Whenever this took place the electricity either vanished entirely, or became exceedingly feeble. It is therefore unintelligible to me how it happens that Dr. Schafhaeutl obtains most electricity when the steam is combined with water.

From the powerful effects I obtained, when water containing a little potash was used, I have little doubt that a properly-constructed electro-steam apparatus would answer all the purposes of an electrifying machine. My own apparatus, though small and imperfect, is no mean substitute for an electrifying machine; and if sharp edges, points, and asperities had been avoided, its efficacy would undoubtedly have been much greater than it is. A gallon of water is the utmost the boiler will hold, so as to leave sufficient room for steam; but a boiler capable of holding *ten* gallons would not be at all inconveniently large; and, with a duly proportioned extent of heating surface, would afford *ten* times as much steam, and consequently *ten* times as much electricity, as mine. There is no occasion to adhere to the form I have adopted for my boiler, which form is only eligible on account of its strength. Experience has shown that there is not much to be gained by increasing the pressure of the steam beyond 60 or 70 pounds on the square inch; and a boiler adapted to resist this pressure would be found sufficiently strong. The coil of copper pipe appended to my apparatus might be dispensed with, for I have always obtained the strongest effects by discharging the steam through a glass tube 6 or 8 inches long. It is important that the apparatus should be insulated, because it is much easier to collect electricity from the boiler than from the jet. The steam should not be discharged more rapidly than it is generated, because running down the pressure always diminishes the electricity in a much greater degree than is due to the mere decrease of density, and frequently changes the electricity of the jet from positive to negative. I find the most convenient way of using my apparatus is to place it out of doors, and to convey the electricity into the house by means of a wire. If, however, the steam were discharged horizontally it might be projected into a chimney, or out of a window, and the objection to using the apparatus within doors would be in a great measure removed.

Dr. Ure, who has witnessed a repetition of most of the experiments I have described, suggested a method of replenishing the boiler without exhausting the steam. He proposed to attach to the boiler a cylindrical vessel, of convenient dimensions, having a stop-cock at the top to receive the water, and another at the bottom to communicate with the boiler. By closing the upper cock, after filling the vessel with water, and then opening the lower one, the water would pass into the boiler without permitting any escape of steam to take place. This arrangement I would certainly recommend to any one who may feel inclined to construct an electro-steam apparatus.

The production of electricity by steam has several important advantages over the common method of obtaining it. An electro-steam apparatus is self-acting, which leaves the operator at perfect liberty to attend to results. Its high temperature renders its action independent of dampness in the atmosphere, which so greatly impairs the energy of an electrifying machine; and finally, its extreme simplicity secures it from injury or derangement.

WM. GEO. ARMSTRONG.

Newcastle-upon-Tyne, 9th May, 1841.

IV. *On Irish Tin Ore. By* THOMAS WEAVER, *Esq., F.R.S., F.G.S., M.R.I.A., &c. &c.*[*]

IN a paper drawn up by Aquila Smith, Esq., M.D., *on Irish Tin Ore*, read before the Geological Society of Dublin, on the 9th Dec. 1840, and published in the London, Edinburgh and Dublin Philosophical Magazine and Journal of Science for February 1841, it is stated, "The question has been often asked, is tin ore found in Ireland? and I believe the *only* reply which could be given is, that it was said to have been found in the county of Wicklow, about the year 1796, when the gold mines were worked on account of the Government."

It is also stated, that the existence of tin ore in Ireland was announced for the *first* time in the report of Messrs. Mills,

---

[*] Communicated by the Author.

King and Weaver, the directors under Government of the gold workings in that county, dated 1st August 1801, published in the second volume of the Transactions of the Dublin Society; but that in the catalogues of the minerals in Trinity College Museum, drawn up in 1807 and 1818, no specimen of Irish tinstone is mentioned; and hence it is said, "we might almost be induced to suspect that some other substance must have been mistaken for tinstone by the directors of the works." It is then added, "The *next* notice of this metal being found in Ireland is in the catalogue of Irish minerals in the museum of the Royal Dublin Society, published in 1832 by the late Sir Charles Giesecke." And the author concludes with a notice of his examination of some washed sand lately obtained from the gold works now carrying on by a company, under tenure from the Crown, at Croghan Kinshela mountain, accompanied by an analysis of "fragments of an earthy-looking mineral, some of them presenting crystalline planes, abraded by friction," which were contained in the sand; from which analysis Dr. Smith affirms, "in corroboration of the assertion of MM. Mills, King and Weaver, that 'native oxide of tin' exists in the county of Wicklow."

This subject is also adverted to in the able address delivered by James Apjohn, Esq., M.D., as President to the Geological Society of Dublin, on the 10th February 1841, in which it is said, "Dr. Smith, therefore, must be considered as entitled to the credit of removing all doubt as to a point long disputed, and equally interesting to the Irish capitalist and the man of science."

It is a curious subject for reflection, how well-ascertained facts, widely known and never questioned at the time, and afterwards placed repeatedly upon record, should in the lapse of years not only be lost sight of, but that their validity should even become a matter of dispute.

To refresh the memories of the older members of the Dublin Society (among whom my name stands enrolled since the commencement of this century), and to convey information to such persons as may not be cognizant of all the circumstances, it may be useful to enter into some details, and to repeat others, which I certainly never anticipated could have been called for at this time of day.

After the discovery of native gold at Ballinvalley, in Croghan Kinshela mountain, in 1796, and the establishment of the government stream-works there, all the metallic substances disseminated through the diluvial deposit, being concentrated in a mass by the successive operations of washing (well known to professed miners), were always brought under my eye for examination, and it was thus that at Ballinvalley I found that the gold was constantly accompanied by magnetic ironstone (sometimes in masses of half a hundred weight), by magnetic ironsand, by cubical and dodecahedral iron pyrites; and in small pieces and grains, by specular iron ore, brown and red ironstone, iron ochre, fragments of *tinstone* crystals, wolfram, gray ore of manganese, pieces of quartz and chlorite, and sometimes fragments of quartz crystals. I observed and collected also some specimens, which show that the gold, magnetic ironstone, and wolfram, were each of them frequently intermingled with quartz; and I have also a few specimens which exhibit the gold not only incorporated with iron ochre, but ramifying in slight threads through wolfram.

To satisfy and amuse some of my county of Wicklow friends at the time, I made an assay of the fragmentary crystals of tinstone, showing to them as good metallic tin as was ever produced in Cornwall.

The operations were interrupted by the Irish Rebellion of 1798; but were resumed in 1801, founded on our report made to Government that year. Subsequent researches showed that the gold and the tin ore were not confined to Croghan Kinshela, both being found also in a mountain named Croghan Moira, about seven miles distant from the former mountain. Of the three stream-works established there, one was placed on Ballycreen, in the stream at the eastern foot of the mountain, and minute particles of gold were found accompanied by magnetic ironstone, magnetic ironsand, compact brown ironstone, cubical iron pyrites, and numerous small garnets. In a second, placed in Ballinacapogue brook, on the western side of the mountain, small particles of gold were obtained with magnetic ironstone, magnetic ironsand, and fragments of *tinstone* crystals. A third trial, in Faunanerin stream, on the north-eastern flank of the mountain, did not produce any gold.

The existence of tin ore in the county of Wicklow having been thus well established, the Dublin Society, at the suggestion of their Committee of Mineralogy, offered, among its other annual premiums, in the early part of this century, a premium of *fifty pounds* to the discoverer of a mine of tin ore in Ireland; and a reference to the printed proceedings of that Society will, I think, show that my memory is correct in stating that that premium was offered for several successive years.

In 1818 I drew up my memoir on the Geological Relations of the East of Ireland, which was published in 1819, in the first part of the fifth volume of the Geological Transactions of London. In that memoir I have given a succinct and faithful account of the whole of the proceedings under Government in respect of the gold workings conducted in the county of Wicklow*, from which I have drawn most of the preceding details. In that memoir I also showed that tin ore was not confined to the county of Wicklow, but that it had been found in the county of Dublin also, though under different circumstances, for which discovery we were indebted to Dr. Thomas Taylor, then living in Dublin, but latterly residing on his property at Dunkerrin Castle, in the county of Kerry. The information on this subject, derived from that gentleman, I have embodied in the 18th section of my memoir (at p. 135 of vol. v. Geol. Trans.), in which, in speaking of the granite region, I have said, "The minerals which I have incidentally observed disseminated in the granite, or imbedded in the contemporaneous veins of granite and quartz that traverse the rock (which more particularly form their native seat), are schorl, tourmaline, garnet, beryl, rock-crystal, epidote, heavy spar, magnetic iron ore, galena, copper pyrites, and iron pyrites. But in addition to these, I am indebted to Dr. Taylor for the information that he has discovered in the granite, malachite, arsenical pyrites, *tinstone*, spodumene, and a new mineral, to which he has given the name of killinite†." And in a note upon the tinstone thus found, I have added, "It is an interesting fact that this metal should at length have been discovered in rock in this country, although found only in a contemporaneous vein traversing a loose block of granite; for its existence in the county of Wicklow, in the form of stream tinstone, had been ascertained several years ago by the operations of the Directors of the works at Croghan Kinshela."

We have it thus upon record that tin ore has been found in *three* distinct localities in Ireland, two in the county of Wicklow, and one in the county of Dublin; the latter being at the south-western foot of Rochetown-hill, adjoining Killiney Bay.

I may here add, that the "tinstone of a hair-brown colour, which accompanies frequently in small grains the native gold found in the streams at Croghan mountain, county of Wicklow," being No. 213 of the late Sir Charles Giesecke's Catalogue of Irish Minerals in the Museum of the Royal Dublin Society, published in 1832, and adverted to by Dr. Smith, was presented by myself before the year 1818.

So lately also as in the year 1835‡, having had occasion to notice a strange misapprehension of facts respecting the gold workings formerly conducted in the county of Wicklow, as inserted in the address delivered at the first annual meeting of the Geological Society of Dublin by their President, I then extracted from my memoir on the East of Ireland certain details, in which the occurrence of tin ore in connexion with native gold in both Croghan Kinshela and Croghan Moira mountains is again mentioned.

---

* See §§ 105 to 107 inclusive of the Memoir in pp. 208 to 213 of vol. v. of the Geological Transactions, First Series.

† See the description and analysis of this mineral by my friends Dr. Thomas Taylor and Dr. Francis Barker, Prof. Chem. Trin. Coll. Dublin, in the 13th volume of the Transactions of the Royal Irish Academy, 1818.

‡ See the London and Edinburgh Philosophical Magazine for July 1835.

We have it thus four times in evidence in print, dating from the year 1801 to 1835, that tin ore does occur in Ireland, to which Dr. Smith's testimony in 1840 now adds a fifth; and this independently of what may be found in the printed proceedings of the Dublin Society in the early part of this century. But it follows from the preceding, that Dr. Smith, in his examination of the washed sand lately procured from Croghan Kinshela mountain, has not elicited a single fact that was not established and known more than forty years since.

V. *On the Analogy between the Phænomena of the Electric and Nervous Influences. By* MARTYN ROBERTS, *Esq., F.R.S.Ed.*[*]

1. ALTHOUGH much has been said by eminent physiologists in endeavouring to prove that an essential difference exists between the nervous and the electric power, yet there are so many points of analogy untouched by these writers, and the facts they have adduced are so susceptible of explanation by electrical laws, that I feel convinced there is the greatest similarity, if not identity, between the nervous and electric influence, both in their mode of action and in the phænomena produced.

2. In the first place, I may call attention to a beautiful analogy which has, I believe, hitherto escaped observation, viz. the influence of electricity on the circulation of fluids; and there is just cause for believing that a like influence governs the circulation of the blood in the living body.

3. If we calculate the force of the heart, we shall find it wholly inadequate to propel the blood through the fine capillary vessels of the system, unless it be assisted by some other power. There are two sources of resistance to the motion of fluids in tubes, at least two that especially apply to the motion of the blood through the vascular system, viz. the tortuosities of the tubes, and *the adhesion of blood to the sides of the channel through which it is impelled.*

4. The resistance occasioned by the deviation of a channel from a straight line is easily overcome when the motion of the fluid in it is slow; for this resistance is the destruction of the momentum, or tendency of the fluid to move in straight lines,

---

[*] Communicated by the Author.

and its amount is the weight of the fluid multiplied by its velocity. It will be found that the blood has but little momentum, for its motion is slow, and that the heart possesses more than sufficient power to overcome all the resistance offered by the tortuosities of the vascular system.

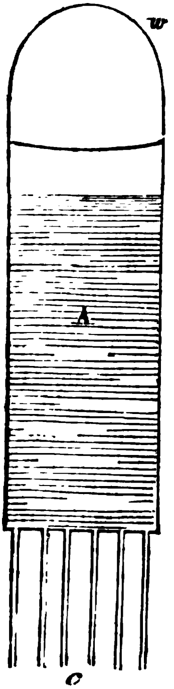
5. The adhesion of a fluid to the sides of the tube through which it passes is a source of resistance that has been much overlooked, but the retardation it would produce in the circulation of the blood would be very considerable were it not compensated or destroyed, especially in the capillary vessels, which are of so small a diameter that their parietes touch every corpuscle of blood that passes them; though some few of the capillaries are large enough to admit two or three globules abreast, many can contain but one red particle of blood in their diameter, while others are so minute as to exclude the red corpuscles, admitting only the fine lymph.

6. The adhesion of fluids to solids is so great, that if every particle of a fluid be in contact with the sides of its channel, a very considerable force will be necessary to overcome the resistance offered to its passage; and this adhesion can be proved in the following manner:—Suspend the solid we wish tried (say a bit of wood) to one end of a fine balance; to the other end add its exact counterbalance in air, then place beneath the wood a vessel containing the liquid, and let the solid rest upon the fluid surface; add weights to the counterbalance until the wood is raised from the liquid, and these weights will give the amount of adhesion. Fig. 1 shows the arrangement. The following is a list of the force of adhesion between a few solids and liquids:—A square inch of wood required 50 grains to detach it from the surface of water; a square inch of gold 446 grains from a surface of mercury; silver, 429 grains; tin, 418 grains; lead, 397 grains; glass, 194 grains.

7. Finding this adhesion to be so great, I concluded it to be the main cause of the resistance offered by capillary tubes to the flow of liquid through them, and that if the adhesion could be destroyed, it would run through such tubes in direct proportion to their diameters; for it is well known that in ordinary cases (where, as I conclude, the adhesion exists) a liquid will not flow through fine tubes in the ratio of their diameter, but in a much less quantity than would be proportionate to larger tubes; indeed, when the channel is capillary, water is either completely arrested or runs in drops at long intervals.

8. Electricity has the power of destroying this adhesion between liquids and solids, for bodies in a like state of electri-

city repel each other; and I found that when a solid resting on a fluid surface was highly electrified, no adhesion took place between them; and the following experiment will show how this also applies to the passage of liquids through tubes.



9. In the bottom of a tin tube (*a*), about an inch in diameter and three or four inches long (fig. 2), fix several metallic capillary tubes (*c*) two or three inches long, opening into the large tube. Now fill the tube (*a*) with water, and suspend it by the wire (*w*) to the prime conductor of an electrical machine. As long as the machine is not in action and the tube and water remain unelectrified, the water will not run through the capillary pipes, but will be completely arrested by the most minute, while through those of a larger diameter it will fall in slowly succeeding drops. But on turning the machine, and thus electrifying the apparatus, the water will instantly run through all the pipes with a rapidity proportionate to the force of the electricity, because the particles of the water and the sides of the pipe being in like states of electricity, repel each other, or at least the electricity overcomes the natural attraction that exists between solids and fluids, and the water is as free to flow as it would be in a tube of such larger diameter that the bulk of liquid contained therein is beyond the sensible attraction or force of adhesion of the pipe.

10. This curious experiment is but a counterpart of what occurs in the living frame; and here, the diameter of the capillary vessels being but little greater than that of the blood corpuscles, the adhesion would act upon every particle of this fluid were it not counteracted by neuro-electric influence, if I may be allowed the term.

11. The force of the heart was calculated from experiment by M. Poiseulle, who ascertained the blood to be propelled with a force that would sustain a column of mercury six inches in height, which is equal to a pressure of three pounds on the square inch, and it is easily proved such a pressure would be insufficient to circulate the blood through a body possessing no living neuro-electric power to overcome adhesion; for let us suppose a proportionate pressure applied to a syringe inserted into the aorta, for the purpose of injecting all the arte-

ries, capillaries, and veins of a dead body; this pressure, on a syringe of one and a half inch in diameter, would be about four pounds; and surely every one will allow that a force of four pounds thus applied would be insufficient to inject the whole body, even with a liquid much less viscid and adherent than blood, and if not sufficient to inject it, how much less to impel such a circulation throughout the system as is maintained in the living being!

12. That the heart derives no assistance from the arteries in propelling the blood, may be proved from the structure of these vessels. To possess a propulsive power they must have muscular contractility; mere elasticity of tissue endows them with no force; it simply causes them to act as regulators to the flow of blood impelled in waves by the rhythmic action of the heart; the air chamber in the fire engine is a similar regulator to the jet of water, compensating for the periodic injections of the pumps; the elasticity of the arteries causes a continuous flow of blood through the capillaries, but cannot force it onwards. It has been fully proved by Müller, Berzelius, Hodgkin and others, that the coats of the arteries are not muscular; and I need not enter into arguments deduced from experiment on their non-muscularity, for I believe this is now the generally acknowledged structure of the arterial coats, and that they possess no more muscular action than so many India-rubber pipes, having but elasticity, which remains long after death, indeed, until decomposition commences.

13. We may then rest satisfied that the arteries in no measure assist the heart to propel the blood, neither can the veins to any considerable amount; and we are therefore driven to the necessity of acknowledging that as the heart's force is insufficient to overcome the adhesion between the blood and the parietes of the capillaries, there must be some power destroying this adhesion, and thus allowing the heart to maintain a free circulation throughout the whole system.

14. This power destroying the adhesion, I hold to be the neuro-electric action. I have already shown that electricity suffices for a like purpose in inorganic bodies, and physiological facts prove the circulation in the capillaries to be greatly, if not altogether influenced by the nerves, which are undoubtedly the sources of the neuro-electric power.

15. The first of these facts is, that the smaller the diameter of the capillaries, the greater is the proportion of nerves distributed upon them, this being necessary to give the larger quantity of *neuro-electric* power requisite to overcome the increased adhesion in these smaller capillaries, caused by the contact of

their parietes with every particle of blood that passes through them; whereas, in those capillaries, where two or three globules can pass simultaneously, a smaller amount of *neuro-electric* influence is required, for the globules are further removed from the parietes of the tube, and therefore less under the influence of the attraction of adhesion.

16. Secondly. Inflammation may be considered as an increased *neuro-electric* action on the capillaries, which electrifying these vessels and the blood to a higher degree than in their healthy state, the adhesion is still further diminished; in those capillaries where red blood before circulated, it now flows in increased quantity; and in those which only admitted lymph in the healthy state, red globules now enter, as may be seen in inflammation of the conjunctiva, &c. The second stage of inflammation is reaction, that is, a diminished amount of neuro-electric influence; the force of adhesion is now uncompensated; the red particles attach themselves to the sides of the vessels, producing the state called congestion.

17. These interesting and successive phænomena can be curiously and beautifully imitated by the apparatus described in section 9, especially if blood (retaining its fluidity) be used instead of water; first the healthy state—a gentle electric action and moderate flow of liquid; then by an increased electric action, imitate the first stage of inflammation with excessive flow of the liquid; then in the state of reaction, which is a deficiency of electricity, we have a cessation of the jet of liquid, with the passage of a few drops at intervals; this imitates congestion; and it is but to transfer these successive actions to the living body, and the analogy is complete.

18. Again, what is turgescence but the result of an increased neuro-electric action, which, destroying the adhesion of the blood to the minute capillaries of the part, renders the force of the circulation sufficient to propel the blood into the before empty cavities? It can be nothing else but this, for the power of the heart is constant, while turgescence is but an occasional phænomenon; neither could the heart direct an impulse to the blood towards any particular part of the system; nor do I see that any theory of this peculiar action, except the one I have ventured to propound, will suffice to explain it; whereas the destruction of the force of adhesion, which I have fully shown to be not only possible, but most probable, is quite sufficient to account for all the phænomena, and will, I trust, be found a simple and satisfactory explanation.

19. Inflammation or turgescence cannot take place without a change of nervous action, and there are many proofs of these phænomena being entirely dependent upon the nerves leading

to the parts affected; thus it has been found that no inflammation ensues on the application of irritants to the eye, if the fifth nerve expanded upon the conjunctiva be divided; the application of ammonia to an eye thus injured produced no effect, while on the other eye of the animal, the nerves of which remained untouched, a similar application produced a violent inflammation.

20. It has also been proved that division of the pudic nerve completely arrested the power of producing turgescence in the singular structure of the corpora cavernosa, discovered by Müller.

21. The phænomena of blushing can be easily explained if we allow that an affection of the mind produces an increased neuro-electric action in the face and neck, which by compensating for the resistance offered by the capillaries in these parts to the flow of blood, they are easily injected by the force of the heart, and that suffused appearance called blushing is the result.

22. The languid circulation in paralysed limbs is the effect of a deficiency of neuro-electric action; there is a greater resistance offered to the passage of blood through the paralysed capillaries, a slow and difficult circulation ensues, and when this happens to excess, those vessels that before admitted the red globules, now only receive lymph, and a paleness of the part is the consequence, with diminished nutrition.

23. These several phænomena tend to prove that a change of nervous action in the capillaries is sufficient to cause an increased or diminished circulation of blood through them, and I have shown this may be accomplished in inorganic bodies by a change in their electric condition, and I trust the analogy has been proved to be complete between this and the nervous or neuro-electric action in the living body. Were any thing wanting to confirm the present hypothesis, I may add, that microscopic observation shows us the red globules of blood passing through the capillaries in a state of extreme self-repulsion, and also removed from the sides of these tubes.

24. I anticipate, with some degree of satisfaction and confidence, that the new theory I have here ventured to propound of the forces tending to motion of the blood, will have the effect of increasing our pathological knowledge of the various changes in the circulation, and clear up many of those obscurities that have long retarded the progress of medical science.

25. The question may be asked, whence is this electricity or the neuro-electric power of the body derived? I answer, by respiration from the atmosphere; for oxygen gas, in com-

bining with either liquids or solids, always sets electricity free, and in the process of respiration a large quantity of oxygen hourly combines with the blood or its carbon; and there is necessarily an immense quantity of electricity evolved during this action, which, electrifying the blood, is sufficient to produce all the phænomena I have explained. How far this electricity may be attracted and retained by the brain and spinal chord, may form the subject of subsequent inquiry; but there can be little doubt that the electricity of the living frame is thus derived from the atmosphere, and it is extremely probable that the air expired from the lungs will be found by experiment to be highly electrified from the same cause.

26. The fact of the annihilation, by electricity, of the adhesion between solids and liquids, will be found of great value to the practical anatomist in making injections of anatomical preparations.

27. If the part to be injected is strongly electrified while the coloured liquid is forced into it, the great resistance that in general exists to the passage of the liquid through the fine capillaries will be overcome, the fluid will pervade the whole tissue, and the minutest capillaries will be filled in a manner hitherto deemed impossible.

28. The air-pump is sometimes used to assist the injection of anatomical preparations, and it may be employed in conjunction with electricity; but I believe electricity will be found sufficient without such aid, and I will endeavour to describe the simplest method of thus injecting a part.

29. Suspend a metallic plate from the ceiling, or any other distant support, by three silken strings, in the manner the pan of a balance is hung; upon this is to be placed the part to be injected with the reservoir of liquid, in order that it may be well insulated, and thus fitted to receive a strong electric charge. The liquid for injection may be contained in an elastic bottle and subjected to a constant pressure by a weight, or it may be in a vessel together with compressed air, the expansion of which will force out the liquid when the channel is open; or in a reservoir, at a height above the part to be injected, and communicating with it by a pipe. These arrangements I merely throw out as suggestions; an ingenious person will soon contrive his best method of forcing in the liquid. I only impress the necessity of insulating all the apparatus, together with the part to be injected, otherwise it cannot become charged with electricity: the best mode of insulating is to place the whole upon the metallic plate, suspended upon dry silken strings three or four feet long.

30. When the apparatus, &c. is upon the plate, make a me-

tallic communication between it and the prime conductor of an electric machine, then charge the whole powerfully with electricity, and the liquid will now flow through the minutest capillaries, and this without any risk of rupturing these fine vessels.

[To be continued.]

VI. *On the Action of Peroxide of Lead on Cinnamic Acid and on Salicine.* By JOHN STENHOUSE, *Esq.*[*]

IT is well known that when cinnamic acid is digested with nitric acid, oil of bitter almonds is formed, which is easily recognisable by its smell, and the cinnamic acid is converted into the benzoic acid. If strong nitric acid is employed, however, and the digestion is long continued, the benzoic acid is still further resolved into the nitro-benzic acid of Mulder. I was induced to try if peroxide of lead would also have the effect of converting cinnamic into benzoic acid. On boiling a solution of cinnamic acid with peroxide of lead, the smell of oil of bitter almonds was immediately emitted, and the peroxide of lead became of a light ochre colour. This change of colour was owing to the reduction of the peroxide of lead to the state of protoxide. In order to determine with what acid the lead had combined, the salt was decomposed with dilute sulphuric acid, and the organic acid thus set free was purified by repeated crystallizations. It had all the external characters of benzoic acid. The following are the results of its analysis:—

gramme.

I. 0·2879 substance gave 0·725 carbonic acid, and 0·1320 water.

II. 0·2885 substance gave 0·7295 carbonic acid, and 0·129 water.

	I.	II.	Calculated numbers.
C	69·62	69·91	69·27
H	5·07	5·00	4·84
O	25·31	25·09	25·89
	100·00	100·00	

Now these approach pretty nearly the calculated numbers for hydrated benzoic acid, the formula for which is

C 14 = 69·27 per cent.
H 6 = 4·84
O 4 = 25·89

---

[*] Communicated by the Author.

The slight excess in the carbon is probably owing to a little of the cinnamic acid having escaped decomposition.

Peroxide of lead is therefore a more elegant test for cinnamic acid than nitric acid, as there are no fumes, as is the case when nitric acid is employed to disguise the smell of oil of bitter almonds, the taste of which is easily perceptible in the liquid.

Hypochlorite of lime also converts cinnamic acid into benzoic, but the oily liquid which is then formed is not oil of bitter almonds, as its taste and smell are quite different, and much more aromatic. The extremely small quantity in which it was obtained, however, unfortunately prevented me from being able to subject it to examination. The cinnamic acid on which I operated, was obtained by boiling balsam of Peru with a solution of potash, which is the most œconomical process for cinnamic acid.

Action of Peroxide of Lead on Salicine.

When a solution of salicine was boiled with a quantity of peroxide of lead, the peroxide of lead gradually assumed a light brown colour. The whole was then thrown upon a filter, and the clear liquid which passed through had a bitter, and at the same time a sweetish taste, which reminded one at once of that of salicine and of the soluble salts of lead. A portion of the liquid was introduced into a retort, and a little sulphuric acid was added. This occasioned a copious precipitate of sulphate of lead, and the red coloured substance called rutiline, which is always produced when sulphuric acid acts upon salicine, also made its appearance. On applying a gentle heat the liquid which passed into the receiver was strongly acid, and when heated with a few drops of nitrate of silver, the oxide of silver was reduced to the metallic state, clearly indicating the presence of formic acid. The other portion of the liquid, on standing some time, became muddy, and deposited a small quantity of a flocculent precipitate. On adding a few drops of ammonia, however, a very bulky precipitate fell, which was soluble in acetic acid and in solution of potash. It consisted of the combination of lead and salicine discovered and analysed by Piria. On treating it with a current of sulphuretted hydrogen till all the lead was precipitated, and purifying the salicine thus set free by two or three crystallizations, I found that the salicine had undergone no alteration. On being subjected to analysis,

gramme.

I. 0·3247 substance gave 0·65 carbonic acid, and 0·1865 water.

The formula for salicine is $C_{42}\ H_{29}\ O_{22}$ =

		Calculated numbers.
C	55·35 per cent.	C_{42} = 55·69
H	6·38	H_{29} = 6·26
O	38·27	O_{22} = 38·05
	100·00	100·00

It appears then that when salicine is treated with peroxide of lead, formate of lead and the combination of lead and salicine are the results.

Salicone.—When salicine is intimately mixed with half its weight of quicklime and subjected to destructive distillation, a quantity of a heavy reddish coloured oil and some water pass into the receiver, while carbonate of lime, mixed with charcoal, remains in the retort. The oil has a very pungent taste, and an empyreumatic smell, somewhat resembling that of creasote. It was rendered colourless by one or two rectifications which deprived it of most of its empyreumatic odour. Alkalies in the cold had no effect upon it, but when heat was applied the oil was changed into a dark resinous mass. Nitric acid attacked it violently, even in the cold, with the evolution of deutoxide of azote. Sulphuric acid reddened it in the cold, and when assisted by heat blackened it, and gave off sulphurous acid. It dissolved iodine very readily, but without explosion, even when heat was applied. When rectified the first and last portions of the oil were rejected. The middle portion had a specific gravity of 1·0212. It began to boil at 260° Fahrenheit, but the boiling point gradually rose to 272° Fahrenheit. It was rendered anhydrous by being allowed to stand for some days over fused chloride of calcium. When subjected to analysis,

gramme.

I. 0·308 substance gave 0·842 carbonic acid, and 0·214 water.

II. 0·3085 substance gave 0·849 carbonic acid, and 0·206 water.

III. 0·326 substance gave 0·886 carbonic acid, and 0·228 water.

	I.	II.	III.
C	75·59	76·12	75·45
H	7·72	7·41	7·77
O	16·69	16·47	16·78
	100·00	100·00	100·00

It is somewhat remarkable that this is identical with the composition which Dr. Ettling found for creasote.

Analysis of creasote by Ettling.

Carbon	75·56
Hydrogen...............	7·78
Oxygen.................	16·66
	——— 100·00

As, however, I have not been able to determine the atomic weight of salicone, I do not think it worth while deducing any formula from these numbers. When salicine is destructively distilled without any lime, it also yields an empyreumatic oil.

Glasgow, April 10, 1841.

VII. *Notices of the Results of the Labours of Continental Chemists.* *By Messrs.* W. Francis *and* H. Croft.

[Continued from vol. xviii., p. 546.]

Action of High Temperature on Mellitate of Ammonia. By F. Wöhler.

IT might with some probability be supposed that the honeystone or mellite is contained in the mass of the brown coal of Artern in such small particles as to escape observation. According to Wöhler's experiments, however, this is not the case. The substance which remains after the mineral has been treated with carbonate of ammonia is alumina; but it always contains a little mellitic acid, probably as basic salt, for on dissolving in nitric acid and evaporation, small crystals are obtained, which are regenerated mellite, $\underline{\dddot{\mathrm{Al}}}\ \overline{\mathrm{Me}}^{3} + 18\ \mathrm{aq}$. Mellitate of ammonia loses ammonia by boiling, and is converted into an acid salt; if, therefore, the mineral has been boiled with the carbonate, it may easily happen that an acid salt is formed, and thereby alumina dissolved. This alumina then passes into the lead or silver salts, from which the acid is then obtained in an impure state, viz. containing alumina. It is therefore better, on decomposing the mellite with carbonate of ammonia, to add a few drops of caustic ammonia from time to time. The acid ammonia salt is much more soluble than the neutral one. The acid crystallizes from a concentrated solution in fine silky needles; does not lose water when heated to 200° C. Formula, $C^4 O^3 + H^2 O$. Wöhler formerly stated that nitric acid precipitated a somewhat insoluble salt from the neutral mellitate of potassa; this is, however, not the acid salt, but a compound of the acid mellitate with nitrate of potassa, $\dot{K}\,\overset{\cdot\cdot\cdot\cdot\cdot}{\underline{N}} + 4\,\dot{K}\,\overline{Me}^2 + 10$ aq. By heating it loses 7 per cent. water, or 6 atoms; the other 4 atoms are not driven off before the salt decomposes; its rational formula is, therefore, $\dot{K}\,\overset{\cdot\cdot\cdot\cdot\cdot}{\underline{N}} + 4\,(\dot{K}\,\overline{Me} + \underline{\dot{H}}\,\overline{Me}) + 6$ aq. The true acid mellitate of potassa forms large transparent crystals, easily soluble $(\dot{K}\,\overline{Me} + \underline{\dot{H}}\,\overline{Me}) + 4$ aq. $\frac{4}{5}$ths of the water is driven off by heat. Mellitate of ammonia $(N^2\,H^8\,O + \overline{Me})$, when heated to 150° C, loses ammonia and water, and is converted into two new substances, both of which contain nitrogen. One of them is an acid, and this is probably the first case known of the production of an organic acid containing nitrogen from the ammonia salt of one which contains no nitrogen. The metamorphosis may be easily produced by exposing a thin layer of the mellitate for several hours to a temperature of 150° to 160° in an oil-bath. The operation must be continued until no more ammonia is evolved. The temperature must not rise above 160°. (A concentrated solution of mellitate of ammonia, if contained in a sealed tube, may be exposed for hours to a temperature of 200° without any change being effected.) The pale yellow powder which remains is treated with *cold* water, which separates it into two substances; it is washed with cold water as long as the filtered solution reacts acid. On evaporating an acid salt is obtained as a scarcely crystalline mass. The insoluble body is called paramid, the acid in the soluble ammonia salt, euchronic acid (εὔχροος, beautiful colour). Paramid is white, and forms hard conglomerated masses; becomes yellow in the air; perfectly tasteless and inodorous; when moistened looks and smells like wet clay; insoluble in water, alcohol, nitric, and even nitro-muriatic acid; is dissolved by hot sulphuric acid, but is separated unchanged by water. Heated to 200° undergoes no change; does not lose water. When heated more strongly is decomposed, gives off cyanide of ammonium, and forms a sublimate which is partly bluish-green and half fused, and consists partly of sulphur yellow crystals of a very bitter taste. Paramid is dissolved by long (several days) boiling with water, and on evaporating an ammonia salt is obtained. At a temperature of 200° this change is soon effected. The salt produced is *acid mellitate of ammonia.* Hence this body belongs to the class of the amides. The composition of paramid is $C^8\,H^2\,N^2\,O^4$. Two atoms of mellitate of ammonia lose 1 atom of ammonia and 4 atoms of water, $C^8\,H^{16}\,N^4O^8$, or 2 $(N^2$

$H^8 O, C^4 O^3) - (N^2 H^6 + H^8 O^4) = C^8 H^2 N^2 O^4$. However, the process is not simple, on account of the simultaneous formation of euchronic acid. By boiling with water paramid takes up 2 atoms and forms 1 atom ammonia and 2 atoms of mellitic acid, $C^8 H^2 N^2 O^4 + H^4 O^2 = 2 C^4 O^3 + N^2 H^6$.

The metamorphosis takes place best at 200°; at 100° euchronate of ammonia is formed, and at this temperature remains unchanged. Paramid is soluble in alkalies, and when heated with them becomes voluminous, and is dissolved on addition of water. If hydrochloric acid be instantly added, unchanged paramid is precipitated; if, however, it be allowed to stand, ammonia is evolved, and the solution then contains at first euchronic acid, and at a later period mellitic acid. This change takes place instantly on the application of heat. By dissolving paramid in ammonia and instantly precipitating by nitrate of silver, a compound of paramid with oxide of silver was obtained, which did not lose water at 150°; this shows that paramid does not contain water, and cannot therefore be $C^8 N^2 O^3 + H^2 O$.

Euchronic Acid.—That part of the product obtained by heating mellitate of ammonia, which is soluble in water, is acid euchronate of ammonia; if the decomposition be not completed it contains acid mellitate; if the temperature is too high it is yellow, and contains the yellow bitter matter. It is deposited in white scarcely crystalline crusts, reacts strongly acid, is not very soluble in cold water, and the paramid must therefore be edulcorated for a long time. To obtain the acid, the salt is dissolved in the smallest possible quantity of hot water, and the hot solution treated with nitric or hydrochloric acid; on cooling the acid separates as a white crystalline powder. It may be purified by re-solution in boiling water, and may be obtained in small crystals. It is very strongly acid, tastes like bitartrate of potassa. The crystals lose water when heated, and become opake. If crystallized from a solution containing sal-ammoniac, it often contains a trace of ammonia, and is then of a yellowish colour. It loses 2 atoms of water at 200°. Can be heated to 280° without change; further heated is decomposed, and gives cyanide of ammonium and a deep green bitter sublimate. Heated with but very little water to 200° in a closed tube it is dissolved, and in the solution is found acid mellitate of ammonia. If a piece of metallic zinc be put into a solution of euchronic acid, it becomes instantly covered with a layer of a splendid blue colour; if the plate of metal be dipped into dilute hydrochloric acid, the blue deposit is separated, and may be washed and dried.

It then forms a black mass, which does not contain zinc. On heating very slightly, even on paper, it becomes instantly white, and is changed into euchronic acid. This is exactly the same as with the deoxidized indigo, only *vice versâ* with regard to the colours. Wöhler therefore proposes for this blue body the name of Euchron. It is soluble in ammonia and the alkalies, yielding splendid purple colour; but the solution decolorated with great rapidity. If a solution of protochloride of iron be mixed with a solution of euchronic acid no change is produced, but on addition of an alkali a voluminous violet-coloured precipitate is produced, which oxidizes in the air with the greatest rapidity. By the action of zinc on a boiling solution of euchronic acid an almost imperceptible quantity of gas (hydrogen or ammonia) is evolved, but this is doubtless a secondary product; a white powder is formed in small quantity at the same time; it appears to be mellitate of zinc. A hot solution of euchronic acid forms, in a dilute solution of nitrate of silver, a yellow precipitate, which is dissolved again on shaking, but on cooling is deposited. It is not dissolved by ammonia, but becomes changed and passes milky through the filter, &c. The lead salt is obtained by adding a boiling solution of euchronic acid to a dilute solution of neutral acetate of lead. The salt is deposited on cooling as a yellow powder, or in small yellow crystals. The filtered fluid, if boiled, deposits white mellitate of lead. Heated to 160° loses 11·36 per cent. water. Both this salt (dried at 160°) and the silver salt, however, still contain water. The composition of the euchronic acid is $C^{12} N^2 O^6$. Atomic weight 1694·29. The anhydrous silver salt is $2\,\dot{A}g + C^{12} N^2 O^6$, the hydrated $2\,\dot{A}g + C^{12} N^2 O^6 + H^2 O$. The lead salt, dried at 150°, is $\dot{P}b + C^{12} N^2 O^6 + H^2 O$. The crystallized salt is $\dot{P}b + C^{12} N^2 O^6 + 5\,H^2 O$. Euchronic acid heated to 200° is $C^{12} N^2 O^6 + 2\,H^2 O$; the crystallized contains 2 atoms of water more. The acid euchronate of ammonia, obtained by heating the mellitate, is $N^2 H^8 O + H^2 O . C^{12} N^2 O^6$; it is not changed at 200°. The decomposition of the mellitate of ammonia is then as follows:—Seven atoms are decomposed and form 2 atoms of paramid, 1 atom of euchronic acid, 4 atoms of ammonia, and 14 atoms of water.

$$\begin{array}{lcllll}
\text{2 atoms Paramid} \;.\;.\;.\;.\;. & = & C^{16} & H^{4} & N^{4} & O^{8} \\
\text{1 atom Euchronic acid} \;.\;. & = & C^{12} & & N^{2} & O^{6} \\
\text{4 atoms Ammonia} \;.\;.\;.\;.\;. & = & & H^{24} & N^{8} & \\
\text{14 atoms Water} \;.\;.\;.\;.\;.\;. & = & & H^{28} & & O^{14} \\
\hline
\text{7 atoms Mellitate of ammonia} & = & C^{28} & H^{56} & N^{14} & O^{28}
\end{array}$$

By the metamorphosis of euchronic acid by water at 200°, into mellitic acid and ammonia, 1 atom of acid and 3 atoms of water form 3 atoms of mellitic acid and 1 atom of ammonia.

$$\begin{array}{lll} 3 \text{ atoms } \overline{Me} = & C^{12} \quad\quad O^{9} & \\ 1 \text{ atom } \ Am = & \quad H^{6} N^{2} & \\ \hline & C^{12} H^{6} N^{2} O^{9} & = C^{12} N^{2} O^{6} + 3 H^{2} O. \end{array}$$

By the conversion of paramid into mellitate of ammonia, only two atoms of mellitic acid are formed. Moreover, 3 atoms of paramid = 2 atoms euchronic acid + 1 atom ammonia. Paramid is bieuchronate of ammonia minus 2 atoms of water. It is probable that paramid and euchronic acid form in high temperatures other interesting products. A deep green sublimate and yellow needles are always formed, &c. &c. &c.—(*Annalen der Pharmacie*, xxxvii. 63.)

Products of the Action of Nitric Acid on Naphthalin. By C. de Marignac, *of Geneva.*

Laurent has shown that by the action of nitric acid on naphthalin two products are obtained; when the action takes place in the cold a substance is formed having the composition $C^{20} H^{14} N^{2} O^{4}$. Nitronaphthalase (Berzelius's nitrite of the oxide of Ikodekatesseryl), by the aid of heat, $C^{20} H^{12} N^{4} O^{3} = 2 (C^{10} H^{6} . N^{2} O^{4})$. Nitronaphthalese (nitrite of the oxide of Dekahexyl) is produced. The action of nitric acid does not, however, cease here; if it be continued two other substances are formed. When nitronaphthalese is once formed, the acid acts but very slightly, and no more red vapours are evolved until the greater part of the nitric acid has passed over, when the temperature rises and red vapours are formed. In order to obtain the products thus formed, De Marignac added the nitric acid to the naphthalin in the retort in small portions, and waited each time until all action had ceased; the operation was continued several days, and the residue in the retort every evening washed with water, in order to extract soluble matters. Three products were thus obtained:—1st, An acid, nitronaphthalic acid, soluble in water. 2ndly, An insoluble residue, nitronaphthalise. 3rdly, A substance in fine flexible needles, which sublimed in the neck of the retort and passed over dissolved in the nitric acid; this substance proved to be nitronaphthalese.

Nitronaphthalise.—The residue in the retort must be washed with boiling water and then treated with cold æther, which dissolves a little of a yellow resinous substance. The nitronaphthalise thus obtained has a tinge of yellow, is almost insoluble in æther, even when boiling, but little soluble in boiling alcohol, out of which it falls as a crystalline powder, insoluble in cold water; boiling water dissolves merely a trace; slightly soluble in nitric acid, precipitated by water. Caustic and carbonated alkalies dissolve it with a red colour, which soon becomes black; melts a little above 100°, may be volatilized without residue, but is often rapidly decomposed with slight detonation. Formula, $C^{20}\ H^{10}\ N^{6}\ O^{12}$, or, according to Laurent's view, $C^{20}\ H^{10}\ O^{3} + 3\ N^{2}\ O^{5}$. The alkaline solution is at first red, becomes brown, and ammonia is evolved; on the addition of an acid, carbonic acid is given off, and brown flocks are precipitated. This brown body is insoluble in water, alcohol and æther, soluble in caustic and carbonated alkalies, these solutions form brown precipitates in silver, lead, baryta, and lime salts; the compounds thus obtained have no determinate composition. The probable composition of the brown body appears to be $C^{12}\ H^{6}\ N^{2}\ O^{5}$. By the formation of this brown substance, formic and hydrocyanic acids are also probably produced.

Nitronaphthalic Acid.—Is formed in very small quantities under peculiar circumstances. It is generally mixed in its solution with a small quantity of a resin, from which it may be separated by evaporating to dryness and treating with a little cold water; the little nitric acid which remains suffices to dissolve the resin, and nitronaphthalic acid is left behind as a yellow crystalline powder. May be purified by re-solution in water or æther, and crystallization. The resin thus separated still contains nitronaphthalic acid. This acid is soluble in boiling water, and more so in alcohol and æther; does not contain water. Formula, $C^{16}\ H^{10}\ N^{2}\ O^{12}$. This is the crystallized acid; in the baryta and silver salts two atoms of water are replaced by two atoms of base. The formula of the silver salt is, therefore, $C^{16}\ H^{6}\ N^{2}\ O^{10} + 2\ Ag\ O$. The ammonia salt is easily soluble in water, and tolerably so in alcohol; the silver salt is white and insoluble. The ammonia salt and acetate of lead give a basic lead salt, which is a yellowish white insoluble powder, $C^{16}\ H^{6}\ N^{2}\ O^{10} + 4\ Pb\ O$. The baryta salt is yellowish white, insoluble, contains no water. The nitronaphthalic acid is, therefore, evidently a bibasic acid. Nitronaphthalate of lead seems to form, under the action of sulphuretted hydrogen, another acid, which M. de Marignac intends at a future period to examine.

In a second paper M. de Marignac states, that by the action of nitric acid on hydrochlorate of chloronaphthalese, $C^{20}\ H^{16}\ Cl^{8}$, he obtained an acid and a volatile fluid. The acid did not agree at all with Laurent's naphthalic acid. The fluid product passes over into the receiver with the nitric acid, and separates from it on account of its greater specific gravity;

part, however, remains dissolved. It is purified by washing and distillation with water. It is perfectly colourless, transparent, sp. gr. = 1·685 at 15° R. (?); has an irritating smell like chloride of cyanogen; is scarcely soluble in water; easily so in alcohol and æther. Forms, by the action of an alcoholic solution of potassa, a crystalline potassa salt. Formula, $C^1\,Cl^3\,N^2\,O^4$, perhaps $C\,Cl^3 + N^2\,O^4$. The density of the vapour could not be determined.

M. de Marignac considers naphthalin as composed of two hydrocarbons, $C^{16}\,H^8 + C^4\,H^8$; chlorine can only extract one half of the hydrogen and form $C^{16}\,H^8 + C^4\,Cl^8$; it is evident that eight atoms must be in a different state of combination from that in which the other eight atoms are. These two bodies may be separated from each other, but they then enter into new combinations. Nitronaphthalic acid may be considered as an oxide of $C^{16}\,H^6\,N^2\,O^4$, viz. $C^{16}\,H^6\,N^2\,O^4 + O^6$. This radical has arisen from $C^{16}\,H^8$ by the substitution of one equivalent hyponitric acid for one equivalent hydrogen. Hydrochlorate of chloronaphthalese is $C^{16}\,H^8 + C^4\,H^8\,Cl^8$. From the radical $C^{16}\,H^8$ the naphthalic acid is derived, while, from the other, $C^4\,Cl^8 + N^8\,O^{16}$ is produced, &c.

Action of Potassa on Camphor. By Delalande.

When the vapours of camphor are passed over a mixture of potassa and lime, heated in a tube to between 300° and 400°, they are absorbed, and no gas is evolved; the product is extracted with hot water, and the filtered solution precipitated by an acid; a white crystalline acid is thus obtained, which, when pure, may be distilled unchanged, and for which the name of Campholic acid is proposed. Campholic acid crystallizes well out of its alcoholic solution. It has a very similar appearance to camphor; reddens slightly litmus paper, but saturates bases perfectly; melts at 80°; boils at 250°; not soluble in water, to which, however, it imparts an aromatic odour; readily soluble in alcohol and æther. The temperature used during preparation must be strictly attended to, for the operation does not always succeed. It is best performed in closed tubes; the vapours are passed backwards and forwards over the heated mixture several times. The formula is $C^{20}\,H^{36}\,O^4$. Campholate of silver is obtained by precipitating nitrate of silver with campholate of ammonia; is a white caseous substance, which easily retains nitrate of silver, and must be dried, pulverized, and repeatedly washed. Consists of $C^{20}\,H^{34}\,O^3 + Ag\,O$. Campholate of lime, when an ammoniacal solution of campholic acid is added boiling hot to chloride of calcium. It is white, crystalline; more readily soluble in cold than in hot water, $C^{20} H^{36} O^4$, $Ca O$, or $C^{20} H^{34} O^3$, $Ca O + H^2 O$. The specific gravity of the vapour of campholic acid is 6·058, according to experiment; calculated, it is 5·938. Campholic acid is, camphor + 2 Aq.

If campholic acid be distilled with anhydrous phosphoric acid, and the product rectified, a colourless oil is obtained, which boils at 135°. Its formula is $C^{18} H^{32}$.. It is named Campholen. The specific gravity of the vapour is 4·353, calculated 4·344.

By the distillation of a mixture of campholic acid with caustic lime, an oil is obtained, whose formula is $C^{19} H^{34} O$, i. e. anhydrous campholic acid, minus carbonic acid, $C^{20} H^{34} O^3 - C O^2$. (*Annales de Chimie et de Phys.* trois. ser. i. p. 120—127.)

On Bromic Acid, and its Salts.

Notwithstanding the various researches which have been made on bromine since its discovery by Balard in 1826, several points in the history of this interesting body have hitherto been but imperfectly investigated, and among others, more especially its combinations with oxygen. To fill up some of these voids has been the object of Dr. Rammelsberg, who, in a paper recently laid before the Academy of Berlin, has described a number of the bromates, and several experiments to obtain hyperbromic acid, in which, however, he has not proved successful. Balard's experiments to obtain hypobromous acid showed that the tendency of bromine to combine with oxygen was exceedingly weak; bromic acid decomposes, under all circumstances, more easily almost than chloric acid, and in this respect cannot at all be compared with iodic acid. Bromate of potash is converted, on heating below strong redness, immediately into bromide of potassium; gaseous chlorine does not produce at any temperature, even with an excess of base, any decomposition. Bromic acid itself decomposes at a temperature of 120° into bromine and oxygen. The iodates of barytes, strontian and lime are converted on heating into basic hyperiodates; such is not the case with the corresponding bromates, which are immediately reduced to the state of bromides. Among the salts examined, the bromate of ammonia is remarkable from the property, not only on heating, but even after a short time, without any external cause, of decomposing with violent detonation into bromine, nitrogen and water; while probably at the same time oxygen is set free, or forms an oxide of nitrogen. The bromate of potash, of soda, and of the oxide of silver, are anhydrous; the two first crystallize in forms of the regular system, the latter is a pulverulent body of difficult solution. The salts of barytes, strontian lime, and lead, contain one atom of water; the barytes and lime salts cannot be obtained well crystallized; but the other two are isomorphous, which is likewise the case with the zinc and magnesia salts, both of which contain six atoms of water, and crystallize in regular octahedrons. The copper salt contains five atoms of water. The bromate of the protoxide of manganese decomposes soon after its formation, bromine being set free, and peroxide of manganese deposited. Several of these bromates combine with ammonia in the moist way, which was not known before. The copper and silver salts take up each two equivalents, the zinc salt one equivalent of ammonia, and, moreover, three atoms of water. Connected with these experiments is the discovery of a double salt of the iodide and bromide of mercury, which is formed direct, and consists of an equal number of atoms of both salts. (*Bericht der Akademie zu Berlin*, Dec. 1840.)

Composition of Guano.

The following analysis of this interesting and important substance, made by M. Voelckel in the laboratory of Prof. Wöhler, confirms what Klaproth found*, viz. that the guano, among its characteristic constituents, contains, besides unchanged uric acid, a considerable quantity of two of its usual products of decomposition, viz. oxalic acid and ammonia. 100 parts of moist guano contain:—

	Voelckel.	Klaproth.
Urate of ammonia	9·0	16·0
Oxalate of ammonia	10·6	
Oxalate of lime	7·0	12·75
Phosphate of ammonia	6·0	
Phosphate of ammonia and magnesia	2·6	
Sulphate of potash	5·5	
Sulphate of soda	3·8	common salt 0·5
Chloride of ammonium	4·2	
Phosphate of lime	14·3	10·0
Clay and sand	4·7	32·0
Undetermined organic substances, of which about 12 per cent. is soluble in water. A small quantity of a soluble salt of iron. Water	32·3	28·75
	100·0	100·00

(*Ann. der Chem. und Pharm.* vol. xxxviii. part 3.)

---

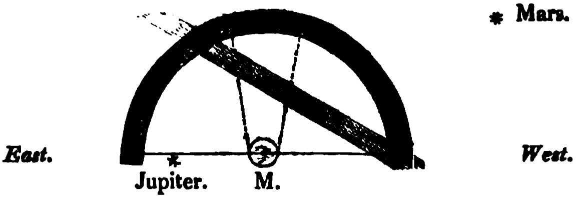
* A memoir on Guano, by Laugier, will be found in Phil. Mag., First Series, vol. xxiv. p. 126; from which it appears that Fourcroy and Vauquelin obtained similar results. Edit.

VIII. *On a Paraselene witnessed on May 6th,* 1841. *By* W. R. Birt, *Esq., Librarian and Assistant Secretary to the Metropolitan Institution.*

To E. W. Brayley, Jun., Esq.

My dear Sir,

I HAD an opportunity last night of witnessing an interesting exhibition of paraselene. The halo occurred in a modification of *cirro-stratus*, which I have termed *nebula suspensa*, in order to distinguish it from other varieties of *cirro-stratus*. I believe it is the only variety that exhibits halos; I do not recollect at the moment having observed them in others. One half only of the halo was visible, the moon being low at the time of observation; this half exhibited three portions, which were more strongly illuminated than the rest of the semicircle. The inclosed rough sketch will convey some idea of the position of these illuminated portions; the semicircle was very distinct, the base or the diameter joining the two extremities and passing through the moon, being parallel with the horizon. Both these extremities were strongly illuminated as compared with the semicircle generally, but faint as compared with the moon; these illuminated portions were not well defined; their appearance did not convey to me the idea of their being images of the moon, but merely portions of the halo more strongly illuminated than the rest. Perhaps this little figure will give the best idea of their appearance.



The darkened portions represent the illuminated portions of the semicircle.

The illuminated portion at the summit of the semicircle was very interesting; it was fainter than the portions above mentioned, and filled a small segment of the halo, in the same manner as a pencil of rays, diverging from a luminous point, would illuminate the segment of a ring situated within its cone. This illuminated portion appeared somewhat of a lozenge form, having the part immediately over the moon brightest and broadest; it became gradually fainter and narrower towards its eastern and western extremities. The difference between it and the illuminated extremities of the semicircle was very distinct.

I first saw this interesting phænomenon about a ¼ past 11, and as I continued to watch it, several appearances presented themselves which (should they have been noticed by others) may probably assist in determining some interesting particulars relative to the clouds that were passing at the times of observation, the area they extended over, etc. I therefore subjoin the observations I made until the disappearance of the paraselene;

And remain, my dear sir, yours very truly,

W. R. Birt.

Metropolitan Institution, May 7, 1841.

1841. *Mean Time estimated to the nearest minute.*

May 6. 11h 45m.—The western illuminated portion nearly disappeared, very faint, the semicircle still visible.

May 6. 11h 49m.—The western portion again visible, with a streak of cloud extending from it to the east of the summit of the semicircle.—The portion of the semicircle, where the streak of cloud intersected it, was illuminated similar to the three portions above mentioned.

This streak of cloud passed over towards the south-east, the eastern extremity of the semicircle being at the time much more strongly illuminated than the western.

May 6. 12h 2m.—A general cloudy appearance westward of the semicircle, and the western extremity very distinct, approaching a decidedly circular form.

May 6. 12h 5m.—The illumination of the eastern extremity has nearly disappeared.

May 6. 12h 8m.—Mars and Arcturus perfectly free from clouds.

May 6. 12h 12m.—A splendid meteor passed near the semicircle directly towards the horizon in the clear part of the heavens.

The clouds passing over appear to be curled cirri; those portions of them that intersect the semicircle in their passage are more illuminated than the others.

May 6. 12h 15m.—The illuminated summit of the semicircle has entirely disappeared, owing to the passage of the clouds towards the south-east; the extremities of the semicircle still faintly visible.

May 6. 12h 18m.—The semicircle obliterated, a small segment only visible to the east, with the eastern extremity very faint.

The degree of illumination was very variable, particularly towards the end of the observations; the western extremity was just discernible when I left observing.

IX. *On the Polarization of the Chemical Rays of Light.*

By JOHN SUTHERLAND, *M.D., Liverpool*[*].

IT has been long known that the invisible rays of the solar light, which manifest their presence by inducing chemical action, are possessed of some of the properties of the luminous rays. Their capability of being reflected and refracted must have been observed at the time of their discovery; and Dr. Thomas Young proved that they were capable of producing the phænomena of interference, by allowing the rays beyond the violet extremity of the spectrum to fall on paper covered with chloride of silver, after having been transmitted through glasses showing Newton's rings. The same phænomenon was also exhibited directly by M. Arago, who made use of Fresnel's experiment for the purpose of demonstrating it.

On the 21st December 1812, M. J. E. Bérard read a paper before the French Institute, "Sur les propriétés des différentes espèces de rayons qu'on peut séparer au moyen du prisme de la lumière solaire," which was published in the "Mémoires d'Arcueil," vol. iii.; and in this memoir, after investigating several properties of the chemical rays, he relates the following experiment:—"I received the chemical rays directed into the plane of the meridian, on an unsilvered glass, under an incidence of 35° 6′. The rays reflected by the first glass were received upon a second under the same incidence. I found that when this was turned towards the south, the muriate of silver exposed to the invisible rays, which it reflected, was darkened in less than half an hour; whereas, when it was turned towards the west, the muriate of silver exposed in the place where the rays ought to have been reflected, was not darkened, although it was left exposed for two hours." From this experiment he deduces that the chemical rays can be polarized like white light, when they are reflected by surfaces of glass under a certain angle, and that this angle appears to be very nearly the same for the two kinds of rays. "It is," he says, "consequently to be presumed that the chemical rays can undergo double refraction in traversing certain diaphanous bodies, and, lastly, we may say that they enjoy the same physical properties as light in general."

An experiment similar to M. Bérard's will be found detailed in the following paper, although I was not aware of there being any such on record, till informed of it by the kindness of Professor Forbes, after my paper had been read. In a communication which I have recently received from him, he also says, "In spring 1839 I tried the experiment of letting the picture formed by polarized light passing through calc spar fall upon sensitive paper, then newly discovered:—whether from the fault of the paper, I know not, but on my first trial I obtained no kind of effect, and my attention being occupied with other matters, I never repeated it: but at Birmingham, in August 1839, being requested to give some account of the Daguerreotype, which I had seen in Paris, I mentioned the experiment, and pointed out its valuable application to fix with unerring accuracy phænomena of diffraction and polarization, which different eyes have seen differently, and which, regarded as the test of theories, would thus be preserved with unimpeachable fidelity for examination at leisure by every eye."

---

[*] Communicated by the Author; having been read before the Royal Society of Edinburgh, December 21, 1840: a communication on the same subject had been previously made to the Literary and Philosophical Society of Liverpool, November 2, 1840.

With these few preliminary remarks I shall proceed to the paper itself.

Liverpool, 22d March, 1841.

In the course of last summer it occurred to me that the invisible chemical rays of light might be subject to the laws of polarization, and early in the month of July I instituted a series of experiments to determine the point. In all investigations of this nature it is of importance that the solar light should continue for a certain time of nearly the same intensity; but during the summer the sky was so frequently overcast that a very few days only could be devoted to the subject, and for a considerable part of the autumn the sun's altitude has been too low. For these reasons I have been unable to pursue the investigation so far as I could have wished, but I have nevertheless obtained a sufficient number of results to establish the principal facts, and I have thought it better to bring these forward at the present time than to allow the subject to lie over for the several months which must intervene before I can again resume it.

I have succeeded in polarizing the chemical rays:—1st, as they proceed directly from the sun; 2nd, as they exist at the extreme violet end of the spectrum; 3rd, as they fall from the sky; and by three different processes,—double refraction, reflexion, and repeated single refraction. I shall describe in succession these processes, with the apparatus used.

1. *Polarization of the Chemical Rays by double Refraction.*

The first important fact in regard to the chemical rays is, that they are susceptible of double refraction, in the same manner as the luminous rays are. To prove this, a prism of calcareous spar, one and three quarters of an inch in length and one inch in the side, and polished at each end, was employed. It was inclosed in a case having an aperture at one extremity, the other extremity being open. The extreme violet rays of the solar spectrum were allowed to pass through the aperture and to fall on a piece of photogenic paper. Two very faint images were formed, and in a minute or two these produced corresponding dark impressions on the paper. A similar result was also obtained when the direct sun's rays were employed. By this apparatus two impressions of equal intensity, each half an inch long and one-eighth of an inch broad, were obtained; but on extending the experiments I soon found that a polarized beam of greater size than it could give was necessary; I therefore substituted a plate of Iceland spar an inch square and $\frac{3}{16}$ths of an inch thick, and in order to increase the divergence of the rays, one of the planes was ground to an angle of 63° with the obtuse edge, and both planes were then polished. A plate of Iceland spar thus prepared, has the property of separating the two rays so much, that when inserted into an aperture admitting a sun-beam into a darkened room, it gives two images of polarized light, each one inch in diameter and about an inch apart, on a screen placed at the distance of eight feet from the aperture.

These images, when received on sensitive paper, both produced considerable effect; but the extraordinary more than the ordinary, and it was therefore chosen for the purpose of experiment.

An analyzing apparatus, consisting of six thin plates of mica, was placed obliquely in the course of the polarized ray, so as to form with its axis an angle of about 25°. The instrument was turned round until the plane of the mica plates coincided with the plane of polarization of the ray. When this was done the light was almost extinguished, and was allowed to fall on a piece of photogenic paper. After the lapse of five minutes no effect whatever was produced on the paper. The mica plates were then turned round 90°, until their plane was at right angles with the plane of polarization. The light was greatly increased in intensity, and in one minute the paper was tinged, in three minutes a good deal so, and in five minutes it was pretty dark. This experiment proves that the plane of polarization of the chemical rays is coincident with that of the luminous rays of the sun's light.

Instead of the mica plates employed in the last experiment, I next used the long prism of Iceland spar already mentioned. The polarized beam was transmitted along it, and the prism turned on its axis until one of the rays was extinguished: a piece of sensitive paper received a dark image from the unextinguished ray; but the extinguished ray produced no effect whatever.

A film of mica was then placed in the course of the polarized beam before it passed through the prism, and the extinguished ray immediately reappeared: the two rays were allowed to fall on sensitive paper, and both produced tints of equal intensity.

This experiment was repeated with a film of selenite instead of the mica: one of the rays was coloured of a yellowish, the other of a purple tint: on being received on photogenic paper both the images gave dark impressions, but the purple image produced more effect than the other.

The experiment was again repeated with a film of selenite, which gave a pink colour to one ray and a green colour to the other; and in this instance both images gave tints of equal depth to the paper. These experiments go to prove that the chemical rays, when polarized, are acted upon by thin crystallized plates, in a manner similar to that in which the luminous rays are influenced.

I was next desirous of ascertaining whether any phænomena resembling the coloured rings seen round the axes of crystals in polarized light were presented by the chemical rays when polarized, and for this purpose I employed an apparatus consisting of a tube two inches long and three-fourths of an inch in diameter: at one extremity of it was placed a double convex lens, having a focus of one and a quarter inch; within the tube, and at the distance of half an inch from the lens, was placed a section of a calcareous spar rhomb, such as is used for showing the coloured rings. At the other extremity of the tube was placed an oblique analyzing bundle of three mica plates, or one of Nicol's improved prisms, and the apparatus was so disposed that the polarized sunbeam was allowed to fall on the lens, and thence through the tube upon a screen placed close to it. An image of the coloured rings and black cross was thus obtained, and by turning the tube 90° upon its axis, the rings with the white cross appeared; while in this position a piece of photogenic paper was used to receive the image, and a reversed impression of the rings and cross was obtained; to wit, the place where the white cross had been was dark, the centre light, with a complete black ring round it, and segments of other rings exterior to it. The tube was next turned 90° upon its axis, so as to show an image of the rings with the black cross; sensitive paper was again employed, and another reversed impression obtained; to wit, the position of the black cross was white, the centre and interspaces dark, with segments of two or three darker circles on them.

I have also used a section of rock crystal for the purpose of obtaining impressions of its rings, and to determine whether phænomena were presented similar to those of circular polarization. The result, however, owing to the unsettled state of the weather, was not so satisfactory as I could have desired, and I have therefore left this part of the subject for a more favourable opportunity.

The next step in the investigation was to determine whether similar phænomena were presented at the violet extremity of the spectrum. For this purpose I employed a glass prism to decompose the polarized sunbeam used in the preceding experiments. A polarized spectrum was thus formed, at the extreme violet end of which most of the experiments were repeated. The extreme violet ray was allowed to pass through the prism of calcareous spar, and received on photogenic paper; one image of violet light, extremely feeble, was all that was visible. The action of the chemical ray was, however, intense, for in a minute or two a deep dark spot marked the position of the *unextinguished* ray, while no effect whatever was produced by the *extinguished* ray. A film of mica was now introduced into the course of the violet ray; two faint luminous images appeared, and two dark impressions were obtained. These experiments were again repeated beyond the extreme violet ray. That part of the spectrum which traversed the prism of calcareous spar gave no luminous image, but the result was the same; to wit, the unextinguished chemical ray gave a dark impression on sensitive paper, and the extinguished ray none; and when a film of mica was used, two dark impressions of both rays were obtained.

The experiments with the rings were also tried; but although the impressions were visible, they were by no means so distinct as those obtained from the direct sun-light, a circumstance which is partly to be attributed to the great difficulty of keeping the axis of the apparatus employed in the axis of the polarized ray. The sun's motion has to be compensated by the movement of the hand; and these experiments are on this account of difficult performance, unless a heliostate, or some similar contrivance, be used to keep the sunbeam precisely in the same direction during the required time.

2. *Polarization of the Chemical Rays by Reflexion.*

I have now stated the principal results at which I have arrived in the polarization of the chemical rays by double refraction, and shall next proceed to detail experiments which go to prove that these rays can be polarized by reflexion. The apparatus made use of consists of a mirror composed of nine plates of parallel glass, by which a beam of polarized light can be thrown upon an analyzing plate of thick flint glass, so mounted that its angle of position can be changed, and its plane of reflexion made to revolve round the polarized beam. The image of the sun, after having been reflected from the mirror at the polarizing angle, was thrown upon the analyzing plate, the plane of which had previously been turned at right angles to the plane of primitive polarization. From this plate the ray was received upon a piece of photogenic paper, and in three minutes a very faint impression was obtained. The apparatus being still in the same position, another piece of paper, cut from the same sheet, was substituted for that used in the last experiment, and the ray was depolarized by interposing a plate of mica between the mirrors; in three minutes the paper received a dark impression, thus affording another illustration of the effect of crystallized plates on the polarized chemical rays.

Another piece of paper, also cut from the same sheet, was made use of, but the analyzing plate was turned 90° upon its axis before the ray reflected from it was allowed to fall on the paper, and in three minutes an impression was obtained equal in intensity to that produced in the last experiment.

These three experiments, which occupied little more than ten minutes in their performance, were executed at a time when the sun's rays were of equal intensity, and the paper used was also of equal sensibility; and they afford an additional proof of the similarity of effect produced by polarizing forces on the chemical and luminous rays.

3. *Polarization of the Chemical Rays by repeated single Refraction.*

To exhibit this phænomenon I prepared two bundles of mica plates, nine in each bundle. These were arranged diagonally in a tube, one half of which could be turned round within the other. The tube was turned so that the planes of both bundles were at right angles, and the sun's rays were transmitted through it so as to fall on sensitive paper. In a few minutes little or no effect was produced, but on turning the planes round so as to coincide, an immediate darkening of the paper took place.

I next employed this method to polarize the chemical emanations proceeding from the sky alone; but as it was necessary in this case to have simultaneous results to obviate the effects of varying intensity, two sets of experiments were carried on at the same time. A piece of thin window glass was chosen, out of which sixteen plates, one and a half inch long and one inch broad, were cut. These were arranged diagonally in four bundles, and placed in two tubes; two of the bundles having their planes coincident in one tube, and the other two with their planes at right angles in the other tube. The tubes were placed close to each other in a perpendicular position in the open air, so that the light from the sky could pass directly through them upon two pieces of photogenic paper, cut from the same sheet, and placed so as to receive the chemical emanations. In this position they were left for two hours in a tolerably clear day; and although the chemical rays had to pass through the same number of plates in both instances, the impressions received by the paper differed much in intensity, that under the tube containing the two bundles with the planes at right angles being much less affected than the other. The explanation of this phænomenon is that all those chemical emanations which were polarized by the repeated refraction of the first bundle of glass plates did not pass through the second bundle when their planes were at right angles, and consequently produced no effect on the paper; but, on the contrary, they passed readily through the second bundle, when its plane coincided with that of the first, and produced their characteristic darkening effect. The phænomena are in fact similar to those observed with the luminous rays under the same circumstances.

Such, then, are the results at which I have as yet arrived in this interesting branch of physical research, and they appear to prove that the third great division of the solar emanations, like the luminous and calorific, are capable of being acted upon by polarizing forces, and that thus they are all subject to the same beautiful laws.

Before concluding this communication I may state, that the photogenic paper employed in the experiments was prepared in the usual way with chloride of silver, but that it would be more satisfactory to use small Daguerreotype plates, particularly in obtaining impressions of organized or crystalline structures by the solar microscope. To effect this, a pair of short polarizing prisms, made according to Mr. Nicol's improved plan, may be adapted to the microscope, one being placed so as to polarize the sun's light before it falls on the object, and the other to analyze the beam immediately after it has passed the object-glass. A sensitive surface placed so as to receive the image thus formed would take a corresponding impression of the structure.

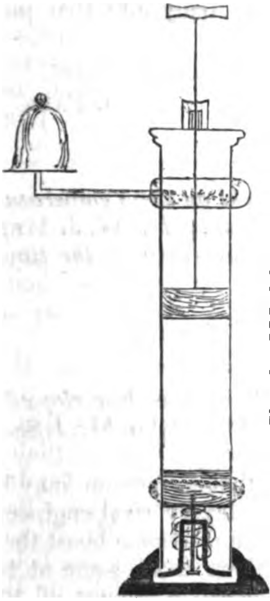
X. *On an Improvement in the Air-Pump. By the* Rev. J. Park.

To the Editors of the Philosophical Magazine and Journal.

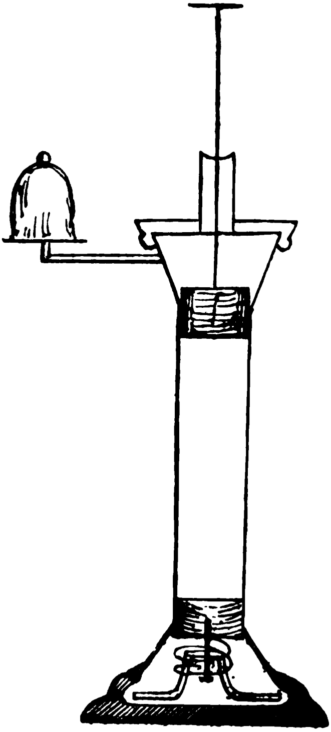
Gentlemen,

YOU will oblige me by inserting in your Journal the following brief description of an improvement in the construction of the air-pump.

A model in my possession works very satisfactorily; but, from want of opportunity, I have not been able to compare its efficiency with other instruments already in use. It consists of a barrel or cylinder of iron truly bored and polished. To this cylinder is accurately fitted a case-hardened iron piston, in the usual manner, the piston-rod working at the upper end through a collar of oiled leathers, perfectly air-tight. At a distance from each end, exceeding a little the length of the piston, are a number of very small apertures, for the admission and egress of the air. At the lower end is *another* accurately fitted metallic plug, the end of which is perfectly flat,



Sketch of Model.



Sketch of further alteration not tried in Model.

as is the end of the working piston, so that when the latter is forced down, there is no residuum of air left in the barrel at the end of the stroke, between the pistons. There is also a spring to keep the pistons in close connexion till the upper one has returned a little past the openings, by which the air is forced out, a little above which openings the lower plug is retained by a stay coming against the end of the cylinder.

It has been objected that, though the model works well at first, the metallic piston will soon wear, and then the instrument will become inefficient. This may be obviated, I conceive, by making the apertures *very small*, and by using pistons packed or covered with some elastic material, as India-rubber, which would easily pass and repass the openings without tearing or injuring the packing. But in order to dispense with the apertures, and to avoid any objection that might be taken to the use of elastic packing on account of them, it has occurred to me, that the upper and lower ends of the cylinder might be made a little wider than the rest of the barrel, for the inlet and escape of the air. In this case, however, the piston rods would require longer collars to serve as guides, that the pistons might come truly into that part where they fit tight to the cylinder.

I am, Gentlemen, your obedient servant,

J. PARK.

Ulverstone, Lancashire, March 13, 1841.

XI. *On* Mr. J. Scott Russell's *Remarks on the Temperature of most effective Condensation* (*of Steam*). *By* W. J. HENWOOD, *Esq., C.E., F.R.S., F.G.S., Secretary of the Royal Geological Society of Cornwall, &c.*

To Richard Taylor, Esq., F.L.S., &c.

SIR,

THE article "Steam Navigation" in the *Encyclopædia Britannica* (xx. p. 697), from the able hand of Mr. J. Scott Russell, contains the following remark:—

"It does not appear to be known that a vacuum may be too good. We hear it boasted every day by rival engineers that their engines have the best vacuum. Some boast their vacuum at 27 inches, others at 28, others at 29, some at 30, and at last an engineer appears who boasts a vacuum of 30½ inches. It is to be regretted that time and talent should be thus wasted. It is a fact of great importance, confirmed by experiment and by practice, that a vacuum may be too good, and become a loss instead of a gain."

Similar remarks from the same respectable authority occur in the Proceedings of the British Association at the Glasgow Meeting (p. 186). I fully concur in the statement that a vacuum may be made so good as to be a drawback on the performance of the engine in which it is obtained, but I beg respectfully to represent that this, instead of being now a novelty, has been long known and acted on.

Mr. Farey (Treatise on the Steam-engine, p. 375) says, "Mr. Watt ascertained that if the temperature of the steam and water contained in the condenser of his engines is reduced to 100 degrees, then as the steam remaining in the exhausted space will only have an elastic force equal to a pressure of not quite one pound on the square inch, it is better to leave this weak steam always in the condenser, and also in the cylinder, to oppose the descent of the piston, than to throw in any more injection water, which would be requisite to cool it more, and render it more rare and feeble."

So long ago as the year 1828, I had remarked that the *duty* of Wilson's engine at Huel Towan, was improved by raising the temperature of the hot well within certain limits: and I then calculated, for intervals of five degrees extending from 80° to 100°, the resistance which the residual vapour would oppose to the descent of the piston, the accelerating influence it would exercise on the ascent of the air-pump bucket, and also the greater or less atmospheric action to which that bucket was exposed by the period of discharging the hot water being lengthened or shortened.

The results of these computations appeared in the Edinburgh Journal of Science for 1829 (x. p. 41, O. S.), and I beg permission to quote the remarks by which they were accompanied.

"The quantity of water (to be discharged by the air-pump) should be as small as possible, not so much on account of its weight, as of the greater period during which the piston of the air-pump will be exposed to the atmospheric pressure. On the other hand, the smaller the quantity of water injected, the higher will be the temperature of the hot-well, and consequently the less perfect the vacuum. It is obvious that the smaller the quantity obtained by adding the difference between the impeding influence of the steam remaining in the hot-well on the piston, and its accelerating action on the air-pump, to the whole resistance experienced by the latter during its exposure to the atmosphere, the better will be the operation of the machine."

I have also mentioned the same subject in your valuable pages (Third Series, xiv. p. 491), and in the Transactions of the Institution of Civil Engineers (ii. p. 60). I am very glad that this important topic has engaged the attention of so able an inquirer as Mr. Russell, but I believe the foregoing extracts will show that the question has not remained unnoticed until now.

I have the honour to remain, Sir,
your faithful humble servant,
W. J. HENWOOD.

4, Clarence Street, Penzance,
May 6, 1841.

XII. *On Cavendish's Experiment.* *By* L. F. MENABREA*.

THE paper before us is a fresh mathematical investigation of the formulæ necessary in the application of Cavendish's experiment for determining the mean density of the earth; and the author seems to have been in some degree incited to the task by hearing of the intended repetition of the experiment in England. It is unfortunate, however, that as Mr. Baily's apparatus is not altogether the same in its details as that of Cavendish, the results of this paper are not wholly applicable to the new observations.

Mr. Menabrea sets out with the well-known equation

$$\frac{d^2\theta}{dt^2} = \int r^2\, d\,m = \int r\, \phi\, d\,m,$$

where $d\,m$ is an element of the system which is moved by the attraction of the large balls, and r is the distance of that element from the axis of the torsion wire. The integral $\int r\, \phi\, d\,m$ arises from attraction-forces and torsion-forces. The former are,—1st, the attraction of the large spheres upon the small ones; 2nd, the attraction of the large spheres upon the rod which connects the small ones; 3rd, that of the beam (which supports the large balls) upon the small balls; (in Mr. Baily's apparatus the large balls are not suspended from a beam, but are supported on a plank;) 4th, the attraction of the box (which contains the small balls) upon these balls. These are all investigated in the paper before us.

The next step is to calculate the Moment of inertia of the small balls, the connecting rod, and the verniers (in Mr. Baily's apparatus there are no verniers attached to the torsion pendulum). The equation is then integrated, and expressions connecting the earth's density with the data of observation are found.

Mr. Menabrea next proceeds to examine the effect of the resistance of the air upon the time of vibration. Assuming a formula for that resistance, composed of two terms varying as the velocity, and as the square of the velocity, he finds that the isochronism of the torsion pendulum is not sensibly affected for small vibrations. But he does not (as might be supposed) attempt to assign the small constants which enter the law of resistance, nor to make the resistance itself affect the final result. He examines the effect of the resistance of the air upon the results of Cavendish's method of finding the point of repose of the torsion pendulum, and shows that it is insensible when this point of repose remains in the same place.

---

* This article is a notice of a paper in the Turin Memoirs (Second Series, vol. ii. p. 305).

The author then examines the manner in which the mean density, obtained from the experiment, combined with the hypothesis that the earth is composed of spheroidal layers of variable density, is to be applied to determine the mass of the earth.

As to the question of mathematics, this paper may be useful in pointing out to future investigators a complete mode of taking into account everything that can affect the result of Cavendish's experiment. But the analysis goes somewhat beyond the practical necessities of the question; a circumstance which is almost inseparable from any attempt to investigate the conditions of a problem which contains so many unknown disturbing forces as the one of which Mr. Menabrea has treated.

XIII. *Reply to* Mr. Airy's *additional Remarks on* Professor Challis's *Investigation of the Resistance of the Air to an Oscillating Sphere. By the Rev.* James Challis, *M.A., Plumian Professor of Astronomy in the University of Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE communication you did me the favour of inserting in your Number for June, was written under the idea that some further explanation was required from me of my reasons for differing in the solution of a hydro-dynamical problem from so high an authority as Poisson, and before I was aware that Mr. Airy intended offering any additional remarks on the subject. That communication is consequently not a direct answer to the arguments contained in Mr. Airy's letter in the May Number, and I therefore ask permission to add something in reply. I shall endeavour to be as brief as possible, the discussion having, perhaps, already occupied too much of your valuable space.

Mr. Airy's reasoning consists of two parts. The object in the first is to show, that the motion of a fluid, when directed to or from a centre, must be the same at the same time at all equal distances from the centre. In the other it is argued that motion resulting from two components, one in the direction of a straight line from a centre, the other in a direction perpendicular to this, is inconsistent with the hydro-dynamical equations.

The mathematical reasoning under the first head is the same as if the problem to be solved had been thus enunciated:—To determine the motion of a fluid, assuming the whole velocity to be directed to or from a *fixed* point: in other words, the motion of a given particle is assumed to be *rectilinear*. It will be readily admitted that in this instance the motion is always the same at the same distance from the centre, because if the motions of two contiguous elements at the same distance were at any instant different, their densities would be different, and their motions would cease to be rectilinear. In the case of motion under discussion, viz. that of fluid put in movement by an oscillating sphere, the motion of a given particle of the fluid is obviously not rectilinear, and it is not therefore necessary for me to show why a solution which I propose for this case is not included in the more restricted one of rectilinear motion.

My concern is rather with the second part of the argument. The reasoning here is not brought to a conclusion. By following out the investigation I find that not only the three equations Mr. Airy adduces, but three others also, which have equal claims for consideration, are exactly verified by the kind of motion which Mr. Airy considers to be impossible. I proceed to give the mathematical reasoning.

The letter P being for shortness' sake substituted for the Napierian logarithm of ϱ, the four hydro-dynamical equations, sufficiently approximate for the proposed instance of motion, become

$$\frac{d\,\mathrm{P}}{dx} + k\,.\,\frac{du}{dt} = 0 \quad \ldots\ldots\ldots \quad \text{(a.)}$$

$$\frac{d\,\mathrm{P}}{dy} + k\,.\,\frac{dv}{dt} = 0 \quad \ldots\ldots\ldots \quad \text{(b.)}$$

$$\frac{d\,\mathrm{P}}{dz} + k\,.\,\frac{dw}{dt} = 0 \quad \ldots\ldots\ldots \quad \text{(c.)}$$

$$\frac{d\,\mathrm{P}}{dt} + \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0 \quad \text{(d.)}$$

The differential coefficients are all partial: and in any case of fluid motion to which these equations apply, the following equations must be verified:—

$$\frac{d^2 P}{dy\,dx} = \frac{d^2 P}{dx\,dy} \dots (1.) \qquad \frac{d^2 P}{dt\,dx} = \frac{d^2 P}{dx\,dt} \dots (4.)$$

$$\frac{d^2 P}{dz\,dx} = \frac{d^2 P}{dx\,dz} \dots (2.) \qquad \frac{d^2 P}{dt\,dy} = \frac{d^2 P}{dy\,dt} \dots (5.)$$

$$\frac{d^2 P}{dz\,dy} = \frac{d^2 P}{dy\,dz} \dots (3.) \qquad \frac{d^2 P}{dt\,dz} = \frac{d^2 P}{dz\,dt} \dots (6.)$$

The three equations (1.), (3.), and (6.), are the same as Mr. Airy's. Now it appears from equations (a.), (b.), (c.), that the equations (1.), (2.), (3.) are at once verified, if

$$\frac{du}{dy} = \frac{dv}{dx}; \quad \frac{du}{dz} = \frac{dw}{dx}; \quad \frac{dv}{dz} = \frac{dw}{dy};$$

that is, if $u\,dx + v\,dy + w\,dz$ be an exact differential of a function of x, y, z, which may also contain t. Let ϕ be this function: so that

$$u = \frac{d\phi}{dx}; \quad v = \frac{d\phi}{dy}; \quad w = \frac{d\phi}{dz}.$$

Then, as Poisson has shown, (*Traité de Mécanique*, tom. ii. p. 687, 2e édition) it follows, to the same degree of approximation, that $P + k \,.\, \frac{d\phi}{dt} = 0$. Hence

$$\frac{d^2 P}{dx\,dt} = -k \,.\, \frac{d^3 \phi}{dx\,dt^2} = -k \,.\, \frac{d^2 u}{dt^2} = \frac{d^2 P}{dt\,dx},$$

by equation (a.).

Thus equation (4.) is verified: and so for equations (5.) and (6.). It has been shown, therefore, that for the verification of the six equations, it is necessary and it is sufficient that $u\,dx + v\,dy + w\,dz$ be an exact differential of a function of x, y, z, and t, with respect to the three first of these variables. On the same condition the equation (d.) becomes ($k\,a^2$ being $= 1$),

$$\frac{d^2 \phi}{dt^2} = a^2 \left(\frac{d^2 \phi}{dx^2} + \frac{d^2 \phi}{dy^2} + \frac{d^2 \phi}{dz^2} \right) \dots\dots \quad (A.)$$

Hence if this equation be made applicable to a proposed instance of motion, and a value of ϕ be obtained from it by integration, it follows from the preceding general reasoning, that the same value of ϕ will satisfy the six equations above, the only condition necessary for their verification being involved in that equation. This will more clearly appear by taking a particular instance; and as Mr. Airy's argument is not more opposed to my solution of the problem of resistance to an oscillating sphere than to Poisson's, I select the latter for an example, as affording by the verification of the equations the most convincing proof of the possibility of the motion under discussion.

Poisson first transforms the equation (A.) into one of which the coordinates are polar, the centre of the sphere being pole, and takes into account the motion of the sphere. (See Additions to the *Connaissance des Tems* for 1834, p. 40. The equation under the form required for my present purpose is given in my communication to the June Number.) As the coordinates of the centre of the sphere do not appear in the transformed equation, the sphere may be regarded as stationary. The integral obtained by Poisson in art. (8.) of the memoir above cited, is,

$$\phi = \left\{ \frac{f(r-at)}{r^2} - \frac{f'(r-at)}{r} \right\} \cos\theta,$$

r being the distance of a point in the fluid from the centre of the sphere, and θ the angle which r makes with the line in which the centre moves. Hence, putting R for the quantity in brackets,

$$\frac{d\phi}{dr} = \frac{d\mathrm{R}}{dr}\cos\theta; \quad \text{and} \quad \frac{d\phi}{r\,d\theta} = -\frac{\mathrm{R}}{r}\sin\theta.$$

These are respectively the components of the velocity in the direction of r and in a direction perpendicular to r. If the centre of the sphere be the origin of rectangular coordinates, and the axis of z be taken in the direction of the sphere's motion, it may readily be shown that

$$u = \left(\frac{d\mathrm{R}}{r^2\,dr} - \frac{\mathrm{R}}{r^3}\right) z\,x$$

$$v = \left(\frac{d\mathrm{R}}{r^2\,dr} - \frac{\mathrm{R}}{r^3}\right) z\,y$$

$$w = \left(\frac{d\mathrm{R}}{r^2\,dr} - \frac{\mathrm{R}}{r^3}\right) z^2 + \frac{\mathrm{R}}{r}$$

$$\mathrm{P} = -k\,.\,\frac{d\phi}{dt} = -k\,.\,\frac{d\mathrm{R}}{dt}.\frac{z}{r}.$$

It is now an easy matter to show that the preceding values of u, v, w, and P, satisfy the equations (1.), (2.), (3.), (4.), (5.), (6.). For example, from the values of P and w,

$$\frac{d^2\mathrm{P}}{dz\,dt} = -k\left\{\frac{d^3\mathrm{R}}{dr\,dt^2}\cdot\frac{z^2}{r^2} - \frac{d^2\mathrm{R}}{dt^2}\cdot\frac{z^2}{r^3} + \frac{d^2\mathrm{R}}{dt^2}\cdot\frac{1}{r}\right\}$$

$$= -k\,.\,\frac{d^2 w}{dt^2}, \text{ and } \frac{d^2\mathrm{P}}{dt\,dz} = -k\,.\,\frac{d^2 w}{dt^2}, \text{ by equation (c.).}$$

Again, putting the letter Q for $\frac{dR}{r^2\,dr} - \frac{R}{r^3}$, we have

$$w = Q z^2 + \frac{R}{r}; \quad \frac{dw}{dx} = \frac{dQ}{dr}\cdot\frac{z^2 x}{r} + \frac{dR}{dr}\cdot\frac{x}{r^2} - \frac{Rx}{r^3}$$

$$= \frac{dQ}{dr}\cdot\frac{z^2 x}{r} + Qx$$

and $u = Qzx$; $\quad \frac{du}{dz} = \frac{dQ}{dr}\cdot\frac{z^2 x}{r} + Qx.$

Hence $\frac{d^2 P}{dx\,dz} = -k\,.\,\frac{d^2 w}{dx\,dt} = -k\,.\,\frac{d^2 u}{dz\,dt} = \frac{d^2 P}{dz\,dx}.$

It is unnecessary to go through the other cases.

This verification, it will be seen, entirely depends on the fact that $u\,dx + v\,dy + w\,dz$ is an exact differential, and is true only in the sense in which this analytical condition is true. It is at this point that the method I have adopted diverges from that of Poisson. From the foregoing mathematical reasoning, it appears that in Poisson's method that condition is not satisfied independently of the particular circumstances of the motion, and of a particular function, cos θ, introduced by considering the nature of the arbitrary disturbance. On the contrary, I argue that as it must be satisfied prior to the consideration of any instance of motion, it must be true independently of all that is arbitrary; and I have shown in the Number of the Philosophical Magazine and Journal for June, that the quantity in question is in this manner an exact differential only when the coordinates are restricted to vary from one point to another in the line of motion. The function ϕ may thus contain implicitly as a factor another function expressing the variation of velocity at a given instant in passing from one point to another in directions perpendicular to the motion, but is not differentiated with respect to the variables of this factor. Accordingly the equation for finding the value of ϕ applicable to the motion caused by a vibrating sphere is

$$\frac{d^2 . r\phi}{dt^2} = a^2 . \frac{d^2 r\phi}{dr^2}; \text{ and } \phi = \frac{f(r-at)}{r};$$

the factor, cos θ, depending on the mode of disturbance, being included in the arbitrary function. The verification of the six equations by the above expression for ϕ proceeds in the same manner as before, but by simpler operations. As ϕ does not contain explicitly the angular coordinates, the velocity perpendicular to the radius vector is nothing, and the whole velocity is directed to or from the centre of the sphere. But as the velocity is not the same in all directions from the centre, the motion of a given particle is not rectilinear, but is continually directed to or from the varying positions of the centre. When the motion is wholly in radii from a centre, it is as necessarily different in different directions from a *moving* centre, as it is necessarily the same in different directions from a *fixed* centre.

Although I agree with Mr. Airy that mixed considerations should be avoided in questions of this nature, I cannot forbear entering upon one which appears to furnish a test for deciding between Poisson's results and mine. From what has just been said, it follows that the velocity of a fluid particle at the surface of the sphere *relative* to the surface = velocity of the sphere × sin θ. Consequently if we impress on the sphere and the fluid the sphere's velocity in the opposite direction, the sphere will be reduced to rest, the fluid will impinge upon it, and the *actual* velocity of a fluid particle along the surface of the sphere will be, the velocity of the fluid × sin θ. This result, which we can hardly conceive to be in error when the motion is slow, is at variance with Poisson's conclusions.

I have now, I think, sufficiently shown that the results I long since obtained respecting the resistance of the air to an oscillating sphere are legitimately derived from a general hydro-dynamical principle. The accordance of these results with experiment is not of itself a proof of the truth of the principle, and I do not therefore see that any advantage can be gained by further consideration of this particular problem. At the same time that I say this, to avoid fruitlessly prolonging the present discussion, I willingly express the satisfaction it has given me, that the Astronomer Royal, in the midst of his many and arduous engagements, should have thought this subject worthy of so much attention; and though I do not see reason for changing my first views, I acknowledge that they have become clearer on several points by the remarks which this discussion has elicited.

I am, Gentlemen, yours, &c.

Cambridge Observatory, June 19, 1841. J. CHALLIS.

XIV. *Researches on Heat.—Fourth Series. On the Effect of the Mechanical Texture of Screens on the immediate transmission of Radiant Heat**. *By* James D. Forbes, *Esq., F.R.SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.*†

Arts. 1—12, *Laminated and Smoked Surfaces.* 13—29, *Rough Surfaces.* 30—34, *Metallic and other Gratings.* 35—53, *Powdered Surfaces.* 54—65, *Conclusions.*

1. On the 2nd September 1839, M. Arago communicated to the Academy of Sciences of Paris a letter by M. Melloni, containing some very interesting experiments on the transmission of Radiant Heat. M. Melloni finds that rock-salt (which is well known to transmit rays of heat from all sources yet tried with equal facility) acquires, by being

---

† From the Transactions of the Royal Society of Edinburgh, vol. xv. Part 1.

* The substance of the present paper was communicated to the Royal Society of Edinburgh on the 16th December 1839, in the words of the memorandum which forms part of this Note. The memorandum itself was read, with some verbal explanation and citation of additional facts, on the 6th of January. Every experiment to which reference is made in the present paper, was performed between the 12th November 1839 and the 4th March 1840. Since that time, I have not made a single experiment on the subject. Occupation of other kinds has prevented me from digesting, until now, the results of these experiments, and from stating the grounds of the conclusions which I formerly announced. The present paper, as it stands, having been submitted to the Council on the 15th May 1840, is printed by their authority. The following is the memorandum just referred to, reprinted from the Proceedings of the Royal Society of Edinburgh:—

"On the Effect of the Mechanical Texture of Screens on the immediate transmission of Radiant Heat. By Professor Forbes.—On the 2nd of September 1839, M. Arago communicated to the Academy of Sciences a letter by M. Melloni, containing some very interesting experiments on the transmission of Radiant Heat. M. Melloni finds, that rock-salt (which is well known to transmit rays from every source with equal facility) acquires, by being *smoked*, the power of transmitting most easily heat of low temperature, or that kind of heat stopped in greatest proportion by glass, alum, and (according to M. Melloni) every other substance. The experiments contained in the Third Series of my Researches on Heat, show that this is equivalent to saying, that substances in general allow only the more refrangible rays to pass; and as M. Melloni had been led by his previous experiments to the same conclusion, his statement amounts to this, that, whilst rock-salt presents the analogy of white glass, by transmitting all rays in equal proportions, every substance hitherto examined acts on the calorific rays as violet or blue glass does on light, absorbing the rays of least refrangibility, and transmitting only the others.

"M. Melloni believes, that the first exception to this rule, or the first analogue of red glass, is rock-salt previously smoked. I desire, however, first to call attention to the fact, that, in a paper published in May 1838 (Researches on Heat, Third Series), I described a substance having similar properties, namely, mica split by heat to extreme thinness, such as I employ smoked, the power of transmitting most easily heat of low temperature, or that kind of heat which is stopped in greatest proportion by glass, alum, and (according to M. Melloni) every other substance.

2. In the Third Series of these Researches, § 3, I have at-

in polarizing heat. In the month of March 1838, I had established by reiterated experiments, that the transmission of heat through glass, far from rendering it less easily absorbed by mica in this peculiar state, had a contrary effect, and also that heat of low temperature, wholly unaccompanied by light, was transmitted almost as freely as that from a lamp previously passed through glass.

"It even appears, from experiments I have since made with the same form of mica, that some specimens transmit *scarcely half* as much luminous heat previously passed through glass, as that from a body below visible incandescence.

"Mica itself, not laminated by the action of fire, possesses, as I have shown by contrasted tables in the paper referred to (Art. 23, 24), properties exactly the reverse; hence the effect is due to the peculiar mechanical condition of the body, and not to its elementary composition.

"It, therefore, at once occurred to me, on reading M. Melloni's communication, that the effect of smoking the salt might be merely owing to a mechanical change in the surface affecting the transmission.

"Roughening the surface was the most obvious experiment, and I found, as I anticipated, that heat of low temperature is very much easier transmitted by salt scratched by sand-paper in two directions at right angles, than luminous heat. Thus, a plate of salt which, when well polished, transmits 92 per cent. of heat derived from a lamp, and sifted by a glass plate, and also 92 per cent. of heat wholly unaccompanied by light, transmitted, when roughened, only 17 per cent. of the former and 45 per cent. of the latter.

"A thin plate of mica, when similarly scratched with emery-paper, so as merely to depolish it, transmitted much more nearly the same per-centage of heat from different sources than when *bright;* showing that the loss of polish affects the transmission of the more refrangible rays much more sensibly that that of the others.

"Yet this effect is not attributable to a variation in the ratio of the reflexion of heat of different kinds at the surfaces of the plate. For, in the *first* place, I have proved, and already communicated the fact to the Royal Society (see Proceedings for April 1839), that reflexion takes place at a polished surface, with almost, if not exactly, the same intensity for all kinds of heat; and, *secondly*, I have found, by direct experiment, that, at least for the higher angles of incidence, reflexion is most copious from rough surfaces for heat of low temperature, or the same kind which is most freely transmitted, proving incontestably that the *stifling* action of rough surfaces is the true cause of the inequality.

"That there is a real modification of the heat in passing through a roughened surface, as well as through laminated mica and the smoky film, appears from direct experiments which I have made on the heat sifted by these different media; which, when transmitted by any one of these, is found in a fitter state to pass through each of the others; and this modification is found to be more perceptible as the character of the heat is more removed from that which these media transmit most readily, that is, as the temperature of the source is higher. Thus, heat derived from a lamp, has 36 per cent. transmitted by a certain smoked plate of rock-salt. But if the heat transmitted by the smoked salt has previously been sifted or anatempted to demonstrate, directly and numerically, that the rays of heat which have passed through alum, glass, and indeed every substance which I tried, have a *mean refrangibility* superior to that of the rays before such transmission; and as M. Melloni had been led in a general way by his previous experiments to a similar conclusion, he inferred, and justly, that most diathermanous bodies absorb the less refrangible rays in excess, and therefore are to heat what green, blue, or violet diaphanous media are to light. Rock-salt alone (so far as we know) possesses the property of *indifferent diathermancy*, and is the single analogue of white transparent glass.

3. The generalization of this principle is a matter of much importance, and especially as it carries our knowledge a step higher in the scale of truth, by teaching us to refer to the quality of *refrangibility* certain properties of heat, which before were connected only with certain vague characters of the nature of the source whence it was derived. Amongst other things we find, what was long suspected, but what M. Melloni first conclusively proved, that the presence or absence of light is, to a great extent, immaterial; no doubt a *concomitant*, but

---

lysed by transmission through another plate of smoked salt, through laminated mica, and through roughened salt, the per-centage is raised from 36 to 41 in the two former cases, and to 40½ in the latter, proving incontestably the specific action of these transmissions in arresting the more refrangible rays.

"I next considered, that as a moderate number of scratches appeared to produce this modification, it might be practicable to obtain the effect by transmitting heat simply through fine wire gauze. I could not obtain it finer than sixty wires to the inch, and in this case, I could obtain no indications of differences in the transmitted ratios of one or other kind of heat. The proportion transmitted to the direct effect, was, in every case, almost exactly that of the area of the interstices of the gauze to its entire surface.

"When fine gratings (used for Fraunhofer's interference fringes) made of cotton-thread were used, even in this case no difference was perceived; here, however, the thread, having probably a certain degree of permeability, might mask the effect.

"When fine powders were strewed between salt plates, leaving minute interstices, the easier transmission of heat of low temperature was again apparent.

"Having procured delicate lines to be drawn with a diamond point on a polished salt surface, first dividing it into squares 1–100dth inch in the side, then into parallel stripes 1–200dth inch apart, and finally into squares of the latter dimension, in every case the effect resembled that of random scratches, and was more apparent as the surface was more furrowed.

"I have finally to observe, that the mere process of natural tarnishing by the exposure of salt to the air, produces a similar effect.

"These facts evidently point to phænomena in heat, resembling diffraction and periodic colours in light. I cannot doubt that the simple transmission through fine metallic gratings would produce effects similar to those of the striated surfaces of rock-salt.—*December* 16, 1839."

not an *indispensable* circumstance. Again, certain relations had been established at an early period in the history of the science of heat, between the *colour* of a surface and the quantity of heat which it absorbed, and this relation for any two surfaces compared (as black and white, of similar textures), was first clearly shown by Sir John Leslie, to depend upon the luminosity of the source of heat, to which conceiving it proportional, that philosopher based upon it the principle of his Photometer*. Professor Powell, of Oxford, conceived and executed an ingenious experiment, by which it is demonstrated that the interposition of a screen of glass, though it stops but little light, alters most materially the influence of colour on the transmitted heat, thus annihilating at once the principle of photometric measurement adopted by Leslie, except in a very limited class of cases†. M. Melloni has fully confirmed the experiments of Professor Powell‡, which therefore may be considered as establishing this conclusion, that the quality of *blackness* or *whiteness* of a surface affects its power of absorbing heat (*not* in proportion to the luminosity of that heat, as was formerly supposed, but) in proportion to its refrangibility.

4. It is both convenient and correct, therefore, to consider the refrangibility of heat as the cause of most of its distinctions of *kind* and *degree* of modification in our experiments, instead of making vague reference to the temperature of the source whence it is derived. Heat derived from the following scale of temperatures corresponds to heat of progressively elevated refrangibility; as, 1. Heat from ice has a less refrangibility than that from, 2. the hand, which again is below, 3. that from boiling-water: then comes, 4. that from a vessel of mercury under its boiling temperature, 5. a piece of smoked metal, heated by an alcohol lamp behind, but itself quite invisible in the dark, 6. incandescent platinum (a coil of wire in an alcohol flame), 7. an oil lamp (Locatelli's). Such is the scale of heat which has often been referred to in M. Melloni's researches and my own; but though our apprehension of the temperature of the source ceases to be so clear above this limit, and the colour and brightness of the light which accompanies the heat no longer varies distinguishably, the scale may

---

* Essay on Heat, 1804.

† Phil. Trans. 1825, p. 187.

‡ *Ann. de Chimie, Avril* 1834. M. Melloni finds, for instance, that the rays from an oil-lamp falling on black and white surfaces, affects their temperature in the proportion of 1000 : 805. And the same proportion holds if they be transmitted through a plate of rock-salt; but if a plate of alum be used, though equally transparent for light with the salt, the proportion is now 1000 : 429.

be carried upwards indefinitely by interposing screens of different materials, which either may be proved *directly* (as I have done in the Third Series of these researches) to increase the refrangibility, or we may take Professor Powell's, or any similar test, which our experiments lead us to conclude to be co-ordinate with the fact of refrangibility. Such a prolongation of the scale of heat-sources would be,

8. Oil-lamp heat transmitted by Common mica.
9. —————————————— Glass (Argand lamp).
10. —————————————— Citric acid.
11. —————————————— Alum.
12. —————————————— Ice.

A clear appreciation of the scale of refrangibility as the important test for the qualities of heat cannot be too clearly apprehended and admitted. Heat from *any* source, if it admit of transmission at all through glass, alum, or water, will ultimately have the character of glass-heat, alum-heat, or water-heat, just as light from the sun, or from a candle, becomes red, blue, or green, by transmission through glasses of these colours.

5. Now, when M. Melloni had shown (and this experiment I believe was original to him), that substances which stop every ray of even intense light (as opake glass and some kinds of dark mica), yet transmit a sensible quantity of heat, it was not unnatural to inquire whether the *invisible heat* thus obtained from a *luminous* source, might not possess the qualities of heat from a dark source, in other words, whether bodies, like black glass and mica, instead of stopping the less refrangible rays like glass, alum, &c., would not suffer these to escape, and absorb the most refrangible rays, acting upon heat as a body does upon light, which stops the yellow, blue, and violet rays, that is, as red glass does.

6. Experiment partly fulfils this expectation, and partly not. The careful and complete series of experiments made by M. Melloni upon the qualities of the invisible heat thus obtained[*], shows, that although it resembles low-temperature-heat, in so far as it is very feebly transmitted by alum or citric acid, yet low-temperature-heat (that from boiling water for instance) is but very faintly transmitted through the black glass or mica, which ought not to be the case if these bodies acted like a sieve, which arrested the more refrangible rays, and suffered the others to escape.

7. The direct test, however, of examining the refrangibility of the heat rays issuing from opake screens yet remained; and

---

[*] *Annales de Chimie, Avril* 1834.

in applying this, I proved that opake glass and mica act as clear glass and mica do in *elevating the mean refrangibility of the transmitted heat.* Hence I concluded that the effect of such media upon heat is to absorb the rays of greatest and least refrangibility, in short, to act as homogeneous yellow glass would do upon light, the *mean* refrangibility being on the whole, however, increased by transmission. I also pointed out that heat from luminous sources is probably far more compound in its nature than dark heat; that the darkness of heat is no test of its refrangibility; and that even the most refrangible rays may contain heat separable from the light which accompanies it*.

8. In all this, then, there appears nothing exactly equivalent to the action of red glass upon light,—no substance which transmits most easily heat of low refrangibility and temperature, and which separates heat of that description from the compound emanation from luminous sources. Reasoning probably upon the conclusions just stated, M. Melloni conceived the happy idea of combining an opake substance, such as smoke, with a solid, which itself should effect no specific change upon the incident heat. He therefore smoked *rock-salt*, and found that it presented a complete analogy to red glass, transmitting most easily heat of low temperature and refrangibility.

9. Whilst I give full credit to M. Melloni for the ingenuity and importance of his experiment, I must be permitted to state, that I conceive that I preceded him by eighteen months in the discovery of a substance possessing similar properties, although I very readily admit, that, having been led to that observation incidentally, I first pursued the remark into consequences which I considered important, after M. Melloni had called particular attention to the experiment with smoked surfaces. On the 27th February, 19th and 20th March 1838 (as appears by my Journal of Experiments), I proved that mica, split into very thin films by the action of heat, such as I employ for polarizing, possesses the property of transmitting in larger proportion several of the less refrangible kinds of heat, and in particular, that it transmits heat from a source perfectly obscure, in almost exactly the same proportion with the highly refrangible heat of a lamp transmitted through glass. I have no hesitation in saying, that no other substance known previously to M. Melloni's experiments with smoked salt, gave any approximation to the following results, which are taken from the Third Series of my Researches, art. 24.

---

* Researches on Heat, Third Series, art. 73, 81, &c.

Table of the proportion of Heat from different sources transmitted by the Polarizing Mica Plates I and K, contrasted with the transmissions by Mica in its usual state, and with Black Glass.

Source of Heat.	Mica split by Heat, Plates I and K.	Mica ·015 inch thick.	Opake Black Glass*.
Locatelli lamp, with glass...	100	100	100
Locatelli	116	79	70
Incandescent platinum	108	70	...
Brass at 700°...................	96	21	...
Heat at 212°.....................	62	11	...

* A contrast experiment made at the same time, March 20, 1838.

10. This singular result of the mechanical condition of the mica did not fail to strike me greatly at the time, and was not published until after careful repetition. It afforded a triumphant reply to an objection against my experiments which I was then combating, that the quantity of heat absorbed by the polarizing plates had modified and even inverted the results, and having satisfied myself of that, I did not pursue the matter further. The moment, however, that I read M. Melloni's communication on Smoked Salt, I perceived the important light which the perfectly analogous case of the split mica might throw upon the phænomenon. It was evident that the results were similar in kind, it was probable that they might be made to approximate in degree. Instead, therefore, of interposing mica piles at the great and disadvantageous obliquities which I had employed (when I wished simply to test their action as polarizing plates), I took a split mica pile (frequently referred to in former parts of these memoirs under the designation H) and placed it *perpendicularly* to the incident rays of heat. I obtained the following results:—

Transmission through Split Mica H, at a Perpendicular Incidence.

Source of Heat.	Per 100 of Incident Rays.	Relative Transmission.
Locatelli, with glass	9·2	100
Locatelli	13·7	150
Dark hot brass	17·3	188
Hot water	16·3*	178

* This observation having been made at a different time from the others, and probably not under exactly the same circumstances, I have stated it in the way least favourable to the views I entertain: the per-centage actually observed was 19.

11. It appears, then, very clearly, that this peculiar condi-

tion of mica induces, in opposition to the natural quality of the substance (9), the same peculiarity which a film of smoke possesses relatively to the incident heat. It is truly for heat what red glass is for light, it transmits most freely rays of lowest refrangibility.

12. Seeing clearly from the first that the change of character in mica was due to the splitting up into an almost infinite number of minute surfaces the natural laminæ of the mineral mica; and attributing the character of redness (so to speak) to the multiplied and irregular reflexions and interferences which must so take place, it occurred to me as very probable, that the effect of smoke was due to the superposition of a prodigious number of minute opake points upon a transparent surface, and *that* not so much from any physical peculiarity of its carbonaceous material, as from the mechanical distribution of opake dust over the diaphragm of rock-salt.

13. This induced me to try the effect of *mechanical alterations of the physical surface* of the salt, expecting to find an effect analogous to that of smoking, and, guided by no other grounds of conjecture than those which I have stated, I roughened with sand-paper both sides of a polished plate of rock-salt, furrowing each surface rectangularly until it was quite dim. I then examined its transmissive power for heat from different sources, and was gratified to find my anticipation realized. The proportion of dark heat transmitted, compared to that from a lamp sifted by glass, was no less than as 3 to 1*.

---

* I state it as a proof of the conviction which I had of the real character of split mica with respect to heat, that the reasoning stated in the text was founded upon no experiments made subsequently to those of March 1838 already qnoted. The very first entry in my journal-book of last autumn contains *simultaneous* experiments, (1.) on smoked salt, to verify M. Melloni's observations: (2.) on split mica, to extend my own of March 1838 to perpendicular incidences: (3). on scratched surfaces, on the assumption that the two former would be realized. As M. Melloni thinks that I had not a clear idea of the properties of split mica, which, indeed, if I understand him, he still doubts, I will quote *verbatim* the passage in my laboratory-book alluded to.—" 1839, Nov. 12. M. Melloni having lately stated (*Comptes Rendus*, 2nd Sept.) that smoked rock-salt is the only substance known which transmits heat of low temperature easier than luminous, this is in the first place contradicted by my experiments of 1838, Mar. 20. &c. on mica split by heat, already published,—and in the next place, I felt [feel] some doubt whether [in his experiments] it was the quality of the *material* or only the *surface* which affects the result. To try this, and to verify previous experiments, I smoked a plate of rock-salt; I *roughened* another with sand-paper, first on one, and then on both surfaces; I had also the split mica plate marked H placed *perpendicularly* to the rays of heat."

[*Here follow the experiments.*]

" It clearly appears, then, that salt simply roughened transmits most dark heat. I presume that the effect of smoking is only superficial, and that roughening stifles luminous heat faster than dark heat."

This is the *first* entry in my book after the publication of M. Melloni's letter in the *Comptes Rendus*, and it is given *entire*.

14. It thus appeared that there are at least three conditions under which a medium can be found capable of transmitting heat of low refrangibility, and that two of these had reference *solely* to mechanical constitution. It was natural to generalize and attempt to include the case of the film of smoke, as well as the striated and the laminated surface, under one category. I have already said that the mechanical distribution of the opake carbonaceous particles offered a plausible analogy, which I proceeded to attempt to carry out.

15. The numbers in art. 10, may be compared with the following:—

Source of Heat.	Transmission per 100 of Incident Rays, by		Relative Transmission by	
	Smoked Salt.	Rough Salt.	Smoked Salt.	Rough Salt.
Locatelli, with glass ...	30	49	100	100
Locatelli	...	62	...	126
Dark hot brass	58	70	192	142
Hot water	67	77	223	157

16. It occurred to me that if the action of the smoke was entirely a *superficial* one, or due to the character of a rough surface applied to the plate of rock-salt, that the effect of two such surfaces upon the transmission of heat would probably differ from that of a single film of smoke, so thick as to produce an equal absorption of heat of any particular degree of refrangibility. For this purpose I smoked three plates of polished rock-salt, so that two marked D and E absorbed *together* as much dark heat (very nearly) as the third plate A did alone.

17. I may take this opportunity of mentioning the way in which I have succeeded in smoking inflammable surfaces without burning them, or crystallized plates, like rock-salt, which crack and fly by the direct application of the flame of a candle. A coarse gas-flame, surrounded by a wide metal tube 10 or 15 inches long, against the side of which the flame partly plays, affords a stream of comparatively cool smoke, which may be applied to any given surface. With these three smoked salt-plates I obtained the following results:—

	Source of Heat.		
	Locatelli, with Glass.	Locatelli.	Dark Heat.
	Per cent.	Per cent.	Per cent.
Smoked Salt Plate A	8·3	17·2	32·9
———————— D	26	41	58
———————— E	23·5	36	53·5
———————— (D + E)	7·3	18	32·1

As most of these results are from single experiments, the first and the last line must be considered as almost identical, and certainly do not indicate any material specific difference in the absorbent qualities of one thick and two thin films of smoke, which might be expected if the action were a merely superficial one.

18. From these numbers we deduce another conclusion of some importance. Since a film of smoke transmits most easily heat of low temperature and refrangibility, we may expect that it will modify the quality of any compound beam of heat which it transmits, and that one such transmission will therefore render a second more easy. Now, we find that the plate D transmitted 26 per cent. of heat from the first of the above sources, and that of the 26 rays escaping from D, and falling upon a second smoked film E, E transmitted 7·3, or 28 per cent. of those incident upon it. But by the third line of the table E transmitted 23·5 per cent. only of the direct rays, consequently the capacity for transmission has been increased. In the same way for Locatelli heat we find the percentage for E raised from 36 to 44 by previous transmission through D; and for dark heat from 53·5 to 56.

19. Hence a useful application of smoked surfaces to which I have sometimes had recourse. It is often important to operate with more or less refrangible rays of heat under exactly the same circumstances of parallelism or divergence, and intensity. Having adjusted an oil-lamp with a salt lens, so as to afford a compound beam stronger than required, we may, by interposing a plate of smoked salt, absorb the most refrangible rays, and suffer the others alone to pass, and by then using a glass of proper thickness, the intensity of the heat may be reduced in the very same proportion, but the more refrangible (hottest) rays are alone retained*.

20. Now the results of (17), though not what I anticipated as most probable, do not altogether relieve us from some doubt as to the nature of the action of the film of smoke, although those experiments, as well as others which are to be detailed in this paper, incline me to M. Melloni's opinion, that the

---

* *Smoked glass* is evidently an excessively opake compound medium, being composed of two parts which absorb opposite ends of the heat spectrum. It is curious to reflect how little the true cause of the opacity of a film of smoke deposited upon glass was understood at the time that it was quoted as a convincing proof of the *immediate* radiation of heat through solid bodies. Far from smoke being the untransparent substance supposed (I use the word loosely in applying it to heat), it transmits a quantity of some kinds of heat really surprising, although the thickness of the smoke be considerable.

smoke acts by its own intimate constitution, and not by its mechanical arrangement. Though I have examined smoky films with a powerful microscope, I have failed in detecting the minutely divided particles of carbonaceous matter of which it must undoubtedly consist. Still the reticulation which fine powder strewed on a surface must form, if it act by the minuteness of the spaces which are left (as in diffraction-experiments on light), must act more intensely when by superposition such reticulations become more minute and complicated. And it may little matter whether the smoky screens are distinct, and deposited on separate plates mechanically placed in succession, or whether they are accumulated by continued smoking on a single surface. I do not state this with a view to maintain my own original opinion, which I am rather disposed to abandon, and to consider a smoked surface, *diathermanous*, as well as *transparent*, in the full meaning of the words; but in extending my experiments to roughened surfaces, I was rather surprised to find that the continued action of furrowing the surface by scratching it with coarse sand-paper, not only diminished the transmission of heat, but increased the *specific* action on rays of different refrangibility, whilst one would rather have imagined that the action being here due to the destruction of polish, and therefore *superficial*, any exaggeration of the roughness would not have increased the relative *diathermancy* to rays of low refrangibility.

21. Conclusive experiments, however, mark an increased sensibility to various kinds of heat by increased roughness. Two plates of salt, marked a and b, having been scored with sand-paper in rectangular directions on both sides, were placed so as to intercept similarly a parallel beam of heat. The difference of the following numbers is due to the less degree of roughness of a.

	Source of Heat.		
	Locatelli, with Glass.	Locatelli.	Dark Hot Brass.
	Per cent.	Per cent.	Per cent.
Rough Salt Plate a	30	48·5	59
———— b	16·6	28·5	45
———— $(a + b)$ ...	7·2	16	27·5
Per-centage of heat received through a transmitted by b ...	24	33	46·5
Ratio of a to b	100 : 55	100 : 58·5	100 : 76

Here, then, we find the per-centage of transmission raised in every case by a previous transmission through a rough surface. The increased facility of transmission is greater in proportion as the incident heat was more heterogeneous; dark heat undergoes very little change. It appears also by the last line of the table, that the increased roughness of *b* compared to *a*, had *enhanced* the characteristic effect (analogous to *redness* for light).

22. I have made a great many experiments to satisfy myself that the action of all the three media already specified (14) is precisely analogous, and that they actually insulate similar rays by absorption. The following table is a specimen, showing the increased facility with which rays of heat, from whatever source, are transmitted by smoked rock-salt after previous transmission through the same or other substances.

Table showing the Per-centage of Transmission by the Smoked Rock-Salt Plate E for heat from different sources, and modified by passing through the following Media.

Source of Heat.	Heat transmitted by			
	Nothing.	Split Mica H.	Smoked Salt D.	Rough Salt *a*.
Locatelli, with glass ...	23·5	...	28	29
Locatelli	36	43·5	44	40·4
Dark hot brass	53·5	56	56	55

23. It is very important to consider how this action of rough surfaces may be explained, and whether we have any analogous phænomena in the case of light. Can it be owing to the circumstance that the depolished surface reflecting differently the various kinds of heat, those kinds least copiously reflected persevere, and form the majority of the transmitted rays? To this it may be replied, that the intensity of reflexion at polished surfaces is so insignificant at a perpendicular incidence for either heat or light*, that were the *whole* specularly reflected heat, transmitted in the one case, and absorbed in the other, the difference, instead of amounting to 30 per cent. or more, of the incident heat (21), could not exceed 4 per cent.

24. Arguing from the analogous case of light, I anticipated, on the contrary, that the *reflected* as well as the *transmitted* beam, would be more intense from such a surface, as it is well known that polish becomes more specular for rays of light con-

---

* See Melloni, *Ann. de Chimie*, Dec. 1835, and my Memorandum on the Intensity of Reflected Heat and Light, Proceedings of the Royal Society of Edinburgh, p. 254.

sisting of longer undulations, the inequalities of the surface first becoming insignificant for red light.

25. In this I was not deceived. My purpose not being to investigate fully the subject of diffuse reflexion, I confined my attention to the establishment of the general fact. Employing an apparatus which I have not yet described, but which bears a great analogy to that figured in the Society's Transactions, vol. xiv. pl. xiii., and described in art. 51 of the Third Series, I observed the intensity of reflexion of heat from different sources at a *single* polished surface of flint-glass, and at a similar surface depolished with emery. I obtained at considerable incidences the following striking results as to the increased susceptibility of heat to be *regularly* reflected at a rough surface, when it is of low temperature or refrangibility.

Ratio of the Intensities of Heat reflected by a Polished and a Rough Surface of Flint-Glass.

Angle of Incidence.	Source of Heat.		
	Locatelli, with Glass.	Locatelli.	Dark Hot Brass.
60°	...	100 : 34	100 : 35·4
70	100 : 26·5	100 : 38·3	100 : 43·5

So far then the character of the action of depolished surfaces is consistent. *The stifling effect* (which diminishes both the reflected and refracted ray) *of a rough or laminated surface diminishes with the refrangibility of the incident heat.* That the same thing takes place in the reflexion of light we know; it is probable that it does so in its transmission likewise, though this has not been so distinctly observed. Most impure substances transmit a ruddy gleam, vapour of water does so whenever it is not colourless*, and every practical optician knows, that in a great majority of media the violet end of the spectrum is first absorbed.

[To be continued.]

XV. *Notices respecting New Books.*

On the Theory of the Moon, and on the Perturbations of the Planets. Part IV. [With a "*Note on the Calculation of the Distance of a Comet from the Earth.*"] By J. W. Lubbock, Esq., Treas. R.S., Vice-Chancellor of the University of London, &c.&c. Lond. 1840. 8vo. Pp. xiv., 355–417, and 1–6.

IN the former parts of this work (the first of which was noticed in our fourth volume, p. 218) the author endeavoured to explain methods of determining the inequalities of the moon, and the periodical inequalities of the planets, which appear to him preferable to any which have yet been proposed. He observes, in the Preface to the part now before us, that the expressions for the variations of the elliptic constants discovered by Lagrange, were not recommended by that illustrious mathematician to be employed for this determination, although Poisson afterwards advised that they should be used; but if his valuable life had been spared, the author observes, "it is possible that his opinions upon this point would have been modified." Mr. (now Sir John W.) Lubbock then proceeds to quote a letter addressed to him by M. de Pontécoulant, in which the methods of solving the difficult problem of the theory of the moon successively employed by Clairaut, d'Alembert, Laplace, Damoiseau and Plana are summarily discussed; the remarks of M. de Pontécoulant being introduced by the observation, "it will be seen that they coincide with the opinions I have expressed in various places during the course of this work respecting the manner in which the perturbations of the moon can be computed with the greatest facility."

---

* Edinburgh Transactions, vol. xiv. p. 371.

"The introduction of auxiliary variables," it is next remarked, "offers a wide scope to the imagination. Recently, M. Hansen has proposed to arrive at the expression for the longitude by altering the time in the elliptic value. It is evident that a celestial body will have, at any instant, the same longitude which it has at some previous or subsequent instant in the elliptic movement; and if this difference of time be calculated, the longitude can be obtained from the elliptic expression by substituting for the true time some other quantity. The radius vector cannot be calculated from the elliptic expression by the same alteration of the time; but M. Hansen gives an expression, by means of which subsidiary terms may be obtained, which, added to the radius vector calculated from the elliptic expression, may give its proper value."

M. Hansen's method being applicable to all mechanical problems, the author proceeds to illustrate it by a simple case, supposing

$$\frac{d^2 x}{d t^2} + x + \alpha x = 0,$$

and considering αx as the disturbing function, and neglected, at first,

$$\frac{d^2 x}{d t^2} + x = 0,$$

of which the solution is

$$x = a \sin (t + b),$$

a and b being constants. If $R = \frac{\alpha x^2}{2}$, $\alpha x = \frac{d R}{d x}$.

When the disturbing function is retained a further approximation may be obtained by one of the three following methods, exemplified by the author:—

1. By substituting the value of x, $a \sin (t + b)$ in αx, and proceeding by the method of indeterminate coefficients.

Or, 2. By finding values of a and b considered as variable, which, substituted in the expression

$$x = a \sin (t + b),$$

will satisfy the differential equation.

Or, 3. By finding a quantity z, such that

$$x = a \sin (t + z + b),$$

so that x is the same function of $t + z$ in the disturbed, as of t in the undisturbed motion. The two latter of these methods are more fully developed.

This case, illustrative of M. Hansen's method, is investigated at large; and the author, stating that in the more complicated question of the determination of the longitude of the planets and their satellites, the reasoning is precisely similar, proceeds to exemplify its application to that problem, in accordance with the expressions previously obtained by Laplace and Pontécoulant.

The increased labour, trouble, and difficulty of the method proposed by M. Hansen, are then pointed out, and the special discussion of that method is closed by the following observations:—

"It is impossible to estimate the policy of adopting M. Hansen's methods sufficiently without considering at the same time the actual state of the lunar theory. M. Plana, after much labour, exhibited results obtained with no less skill than honesty of purpose, and containing few numerical mistakes. His results have been examined by me, and more extensively by M. de Pontécoulant; and as we pursued an independent process, it is probable that few if any errors remain undetected in M. Plana's expressions for the longitude and latitude of the moon. If M. Hansen contemplates the exhibition of the value of z to the same degree of approximation, and not developed according to powers of m, such an expression will be incapable of verification, and useless. If the value of z be developed according to powers of m, no opinion will be possible of its accuracy until it has been verified independently by other mathematicians, for only the terms which are independent of the eccentricities will be found in M. Plana's expression for the longitude."

So far as these observations relate to the great work of Plana on the lunar theory, they may, we think, be instructively placed in apposition, by the student of analysis and of physical astronomy, with the character of M. Plana's labours given by Sir John F. W. Herschel, in his address to the Royal Astronomical Society on the subject of the award of the medal to the Italian astronomer and analyst, which has already been transferred to our pages. (Phil. Mag. Third Series, vol. xviii. p. 153.)

The most advantageous method of calculating the perturbations of the small planets, is stated by Sir John Lubbock to be that described by M. de Pontécoulant (*Théor. Anal.*, vol. iii. p. 505), which he briefly recapitulates, and next proceeds to compare with the method of M. Hansen, pointing out the far greater facility of the operations required in the former; and terminating at once the consideration of the subject and the Preface to the work, with the subjoined reflections:—

"If the form of the present planetary tables be abandoned which give the longitude, and tables be formed which give the value of z, most of the numerical calculations contained in the third volume of the *Méc. Cél.* will be thrown away. It would be better to subject those figures, and particularly the quantities b, to a careful examination.

"Public institutions, established with the view of contributing to the progress of astronomy, would confer more essential services to science if their strength were devoted to purposes of this kind rather than to the inconsiderate multiplication of unreduced or not wanted observations. The determination of the perturbations of the small planets is a matter of urgency. Moreover, the perturbations of the comet which bears the name of Encke, are of too great cosmological importance to be properly entrusted to any single hand, however skilful and conscientious, or indeed even to a single method. But such troublesome computations, to be conducted with a fair chance of success, must be undertaken with the requisite appliances, and seem therefore properly to fall under the superintendence of those astronomers who are placed in public situations. Mutual cooperation upon a uniform system would produce more important results than any incoherent computations founded upon fantastical expressions."

The applicability of the views suggested in the preceding paragraph to the future proceedings of the British Association, the Royal Astronomical Society, and other scientific bodies concerned in the reduction of astronomical observations and the formation of new astronomical tables, will immediately be perceived.

The contents of the work itself (in this fourth part), are distributed in the following order:—

Calculation of the first term in the longitude, appertaining to the arguments 0, 1, 2, 3, 4, 5, 6 and 7, in illustration of M. Hansen's equations.—Calculation of terms depending upon the second approximation, in further illustration of M. Hansen's equations.—Calculation of the first term in the reciprocal of the radius vector and of the longitude of the moon, by means of the variation of the elliptic constants.—On the variation of the arbitrary constants in mechanical problems.—On the variation of the semi-axis major of the moon's orbit.—On the divergence of the numerical coefficients of certain inequalities of longitude in the lunar theory.

In the calculation of the first term in the longitude, appertaining to the arguments 0, 1, 2, 3, 4, 5, 6 and 7, in illustration of M. Hansen's equations, Sir John Lubbock adopts an equation and expressions given by M. Encke, in his paper entitled "*Über die vom Director Hansen eingeführte Form die Störungen in unserm Sonnen-system vollstandig zu entwickeln.*" He then shows in what manner the same terms may be calculated by his own methods.

The terms depending upon the second approximation are next calculated, in further illustration of M. Hansen's equations; and then the first terms in the reciprocal of the radius vector and of the longitude of the moon, by means of the variation of the elliptic constants. In treating of the latter subject, the author shows, by an elementary example, the kind of operations which would be required in order to determine the periodical inequalities of the moon or planets by means of the variation of the elliptic constants, as proposed by Poisson; exemplifies a modification of that method arising from the introduction of a certain constant quantity; and concludes the section by comparing with its previous contents the calculation of the same terms according to his own methods.

The next subject is the variation of the arbitrary constants in mechanical problems, commencing with a view of the theory of that variation, substantially identical with the author's paper on the same subject inserted in our eleventh volume; and followed by the extension of the methods employed in that paper to a more general case, as given in our twelfth volume.

This is followed, under the head "*On the Variation of the Semiaxis Major of the Moon's Orbit,*" by the author's demonstration of the theorem that the expression for that Variation contains no argument of long period, accompanied by a multiple of m less than m^4. As this had previously appeared at length in our pages (Third Series, vol. xvii. p. 338), we need not notice it further at present.

The concluding section, on the divergence of the numerical coefficients of certain inequalities of longitude in the lunar theory, is thus introduced:—

"The divergence of the numerical coefficients in the lunar theory, made manifest by M. Plana's development of the expressions according to the powers of m, presents a difficulty in a complete numerical solution of the problem, that is, a solution intended to embrace all quantities which are sensible in practically ascertaining the moon's place with the accuracy required for comparison with the best observations. But the following questions naturally occur: Is there any method of approximation which will serve to select the more considerable terms, rejecting others? Is the divergence due chiefly to the development and expansion, according to powers of m, of the divisors introduced by integration? In the latter case the difficulty might be easily avoided; but each of these questions must be answered in the negative."

In order to illustrate this point, the author has selected indifferently two terms in the longitude amongst those in which this divergence is met with, and he examines their construction without introducing details which do not bear immediately upon the point referred to.

After this examination he concludes the section, and with it the fourth part of his work On the Theory of the Moon, with the annexed observations:—

"These are the most considerable, but it would evidently be impossible to employ with safety any rule of approximation which did not embrace other terms. In this and in other cases in the lunar theory it will be found that coefficients, when formed correctly, are made up by the addition of numerous small terms, which come from various sources: hence the danger of attending only to the leading or principal terms which may occur upon an incomplete examination: hence also the extreme practical difficulty of the problem in whatever manner it be approached.

"In the method which M. Plana has adopted, first, the mean longitude of the moon is obtained in terms of the true longitude, and the true longitude is afterwards found in terms of the mean longitude by reversion. But the divergence of the numerical coefficients exists equally in the former expression, and does not arise in the operation of reversion."

A note is appended "*On the Calculation of the Distance of a Comet from the Earth.*" Sir J. Lubbock having in his former Treatise on that subject (noticed in our fourth volume, p. 219) considered the position of the comet and of the sun to be defined by *longitude* and *latitude*, deduces, in this note, from the equations given in that treatise, those which must be employed if the positions of the comet and sun are defined by *right ascension* and *declination*; after which he successively examines the equations of Legendre and Lagrange (showing the former to be preferable), notices the equation recommended by Mr. Airy, and the solution of Laplace; and concludes as follows:—

"If observations of right ascension and declination be employed, the trouble of calculating the observed longitudes and latitudes is avoided, but additional terms are introduced explicitly into the final equations from which the distance of the comet is sought, and which may be considered as identical, whichever data are employed."

An Essay on Single Vision. By Dr. WOODHOUSE, Fellow of Caius College, Cambridge. Weale, 59, High Holborn, 1841.

This subject belongs to a class of questions intermediate between Natural Philosophy and Pathology, the research of which as a branch of science has been recommended by Bacon in his *Novum Organum*.

Suggestions on this particular question have been made by Newton, Harris, Wood, Herschel and other optical writers, but which cannot be considered as completely accounting for the phænomenon.

The author considers "that the parts immediately acted upon by any exciting power, do perceive." This position admits of dispute; it is well known that during the Taliacotian process, an injury inflicted on the new-made nose is felt on that part of the forehead from whence the skin had been removed (see Johnstone on Sensation). We have only room to notice two, out of many ingenious experiments by the author. Let C be an object looked at, and P one interposed. Two images of P will appear. If P be moved towards C, those images will mutually, and *equally* approach towards coalescing.

Exp. 2. Let a candle be placed a few feet from the observer, and let him look at a pencil between the candle and his eyes; two images of the candle will appear: but, contrary to the first experiment, when the pencil is moved, one of the images remains stationary, the other proceeding alone to coalesce with it.

From similar experiments, and some physiological considerations, the Doctor infers that one eye may be regarded as a *master eye* wholly occupied with vision, the other being nearly or wholly passive. This tract will repay perusal.

The author concludes it with the following problem, which had been treated already by Wollaston, viz. "When a picture looks at you in one part of a room it will look at you in another." The author's solution seems simple and satisfactory.

XVI. *Proceedings of Learned Societies.*

LONDON ELECTRICAL SOCIETY.

June 15th.—A Letter was read from Martyn Roberts, Esq., Mem. Elec. Soc., describing "Some Experiments which show that Radiation, a property of Heat, is not a property of Electricity." The experiments were those in which a Bennet's electrometer is exposed to the influence of a charged conductor, and the leaves, which by induction had diverged, collapse the instant the charge is removed from the conductor. This would not be the case if electricity radiated as heat does: it would then pass through the interstices of the air and produce a permanent effect upon the instrument. The author in conclusion said, that "the establishment of facts such as these would throw much light upon the nature of electricity; and its dissimilarity from, or the analogy it bears to heat, will be one great means of penetrating more deeply into these mysterious agencies."

A translation was read of a paper entitled "On the Remarks of M. Becquerel relative to the comparative measure of the action of two voltaic pairs, one Copper and Zinc, the other Platinum and Zinc, by M. Jacobi." M. Becquerel had attributed the superior power of the nitric acid battery to the reaction between the nitric acid and the acidulated water. M. Jacobi considers this reaction by no means sufficient to produce the astonishing results obtained from a Grove's battery; and he alludes to some experiments by which M. Fechner had proved that of the whole power obtained from a battery, whose two exciting liquids were nitric acid and *alkali*, only $\frac{1}{60}$th part was due to this species of reaction; and hence M. Jacobi inferred that when *acidulated water* was substituted for alkali, the amount due to this reaction would be in proportion still less. The author here gave a series of propositions deduced from our present knowledge of voltaic phænomena; and concluded with some further experiments in illustration of the actual superiority possessed by the platinum over the copper battery.

We must refer our readers to the Proceedings of the Society for the other papers which were read: viz.—

"Descriptive Memoir of an Atmospheric Electrical Apparatus, and its appendages, comprehending an insulated line of wire, extending about three hundred and sixty-five yards horizontally over the town of Sandwich," by W. H. Weekes, Esq.

A Letter addressed to J. P. Gassiot, Esq. from H. Coller, Esq., describing some experiments, not altogether unsuccessful, in copying a Daguerréotype Plate by electrotype.

The date of the experiments was March 1840. The Plates obtained were on the table.

"A Register of the electric state of the Atmosphere in relation to its Meteorological Phænomena, for the month of May 1841, from observations made with the insular apparatus already described," by W. H. Weekes, Esq.

This register, which will be supplied regularly, will be of great value on account of the mode of obtaining the data it contains.

The reading of a communication from Andrew Crosse, Esq., Mem. Elec. Soc., and another from Thomas Pine, Esq., was deferred to the next meeting.

LONDON INSTITUTION.

At a meeting of the Managers of the London Institution, on Friday, June 4th, Sir Thomas Baring in the Chair, Professor Grove showed the electric spark as produced by the condensation of effluent steam, the original experiments on which subject have already appeared in our journal. At one extremity of a brass insulated conductor was suspended a copper plate of about 7 inches diameter: the jet of steam issuing from a tube connected with the boiler of a model low-pressure steam-engine, impinged upon this plate, and several sparks of a quarter of an inch in length, were drawn from the conductor. The electricity of the conductor was shown to be positive. A Leyden phial was charged, &c. We understand that it is the intention of Professor Grove to repeat in the theatre of the Institution any novel scientific experiments which may be capable of popular illustration.

XVII. *Intelligence and Miscellaneous Articles.*

TWO LETTERS ON CALOTYPE PHOTOGENIC DRAWING, FROM H. F. TALBOT, ESQ., F.R.S., TO THE EDITOR OF THE LITERARY GAZETTE.

DEAR SIR,—IT is now two years since I first published a brief account of Photogenic Drawing. During this interval I have taken much pains, and made many experiments, with the hope of rendering the art more perfect and useful. In this way I have obtained a good many improvements, with the mention of which I shall not detain you at present.

I shall confine myself in this letter to a single subject, viz. the discovery which I made last September of a chemical process by which paper may be made far more sensitive to light than by any means hitherto known. It is not easy to estimate exactly how far this increase of sensibility extends; but certainly a much better picture can now be obtained in *a minute* than by the former process in *an hour*.

This increased rapidity is accompanied with an increased sharpness and distinctness in the outlines of the objects—an effect which is very advantageous and pleasing, and at the same time rather difficult to account for.

The shortest time in which I have yet succeeded in impressing an image in the camera obscura has been *eight seconds*; but I do not mean to assign this as the precise limit, for it can only be ascertained by more careful and multiplied experiments.

The production of the image is accompanied with some very extraordinary circumstances, to which I will advert in a subsequent letter. These phænomena are extremely curious; and I have not found in chemical writers any mention of anything similar.

The image, when obtained, must of course be fixed, otherwise the process would remain imperfect. It might be supposed, *à priori*, that this fixation would be very difficult, the paper being so sensitive. But it fortunately happens that, in this instance, what seems a reasonable inference is not borne out by fact, that new photographs are more easily and perfectly fixed than was the case with the former ones. When fixed, a great many copies may be made from them; and thus the original view can be multiplied with facility.

I think that the art has now reached a point which is likely to make it extensively useful. How many travellers are almost ignorant of drawing, and either attempt nothing, or bring home rude unintelligible sketches! They may now fill their portfolios with accurate views, without much expenditure of time or trouble; and even the accomplished artist will call in sometimes this auxiliary aid, when pressed for time in sketching a building or a landscape, or when wearied with the multiplicity of its minute details.

One of the most important applications of the new process, and most likely to prove generally interesting, is, undoubtedly, the taking of portraits. I made trial of it last October, and found that the experiment readily succeeded. Half a minute appeared to be sufficient in sunshine, and four or five minutes when a person was seated in the shade, but in the open air. After a few portraits had been made, enough to show that it could be done without difficulty, the experiments were adjourned to a more favourable season.

Several photographic processes being now known, which are materially different from each other, I consider it to be absolutely necessary to distinguish them by different names, in the same way that we distinguish different styles of painting or engraving. Photographs executed on a silver plate have received, and will no doubt retain, the name of Daguerréotype. The new kind of photographs, which are the subject of this letter, I propose to distinguish by the name of *Calotype*; a term which, I hope, when they become known, will not be found to have been misapplied.

I remember it was said by many persons, at the time when photogenic drawing was first spoken of, that it was likely to prove injurious to art, as substituting mere mechanical labour in lieu of talent and experience. Now, so far from this being the case, I find that in this, as in most other things, there is ample room for the exercise of skill and judgement. It would hardly be believed how different an effect is produced by a longer or shorter exposure to the light, and, also, by mere variations in the fixing process, by means of which almost any tint, cold or warm, may be thrown over the picture, and the effect of bright or gloomy weather may be imitated at pleasure. All this falls within the artist's province to combine and to regulate; and if, in the course of these manipulations, he, *nolens volens*, becomes a chemist and an optician, I feel confident that such an alliance of science with art will prove conducive to the improvement of both.

I remain, yours, &c.

31, Sackville Street, Feb. 5, 1841. H. F. TALBOT.

DEAR SIR,—I will now proceed to give you some further details, for which I had not room in my last letter, respecting the phænomena which occur during the very singular photographic process to which I have given the name of Calotype. And I may as well begin by relating to you the way in which I discovered the process itself. One day, last September, I had been trying pieces of sensitive paper, prepared in different ways, in the camera obscura, allowing them to remain there only a very short time, with the view of finding out which was the most sensitive. One of these papers was taken out and examined by candlelight. There was little or nothing to be seen upon it, and I left it lying on a table in a dark room. Returning some time after, I took up the paper, and was very much surprised to see upon it a distinct picture. I was certain there was nothing of the kind when I had looked at it before; and, therefore (magic apart), the only conclusion that could be drawn was, that the picture had unexpectedly *developed itself* by a spontaneous action.

Fortunately, I recollected the particular way in which this sheet of paper had been prepared, and was, therefore, enabled immediately to repeat the experiment. The paper, as before, when taken out of the camera, presented hardly anything visible; but this time, instead of leaving it, I continued to observe it by candlelight, and had soon the satisfaction of seeing a picture begin to appear, and all the details of it come out one after the other.

In this experiment, the paper was used in a moist state; but since it is much more convenient to use dry paper if possible, I tried it shortly afterwards in a dry state, and the result was still more extraordinary. The dry paper *appeared* to be much less sensitive than the moist; for when taken out of the camera after a short time, as a minute or two, the sheet of paper was absolutely blank.

But, nevertheless, I found that the picture existed there, *although invisible*; and by a chemical process analogous to the foregoing, it was made to appear in all its perfection.

After several further experiments, which were requisite in order to come to a right understanding of this unexampled natural process, I found it expedient to abandon the former method of taking views with the camera, in favour of the new one, so far excelling it in rapidity and power. The result of my experience hitherto with this calotype paper is, that if properly prepared, it will keep three or four months, ready for use at any moment, and moreover it is used in a dry state, which is a great convenience.

The time of exposure to light in the camera may be varied, according to circumstances, from a quarter of a minute upwards; and the paper, when taken out of the instrument, appears quite blank, as I said before, but it is impressed with an invisible image. It may be kept in this invisible state for a month or so, if desired, and *brought out*, or rendered visible, when wished for. But, generally, this is done shortly after, or at least on the same day, for fear of accidents (such as a casual gleam of daylight, which would at once annihilate the whole performance). Whenever it is desired to render the picture visible, this is done in a very short time, as from a minute to five or ten minutes, the strongest impressions coming out the easiest and quickest. Very faint impressions (as those obtained when the paper has been only a few seconds in the camera, or the objects have not been luminous enough) take a longer time in coming out; but they should not be despaired of too soon, as many of them exhibit difficulty at first, as if reluctant to appear, but nevertheless end by coming out very well. The operator of course remains in a darkened room, lit by candles only.

I know few things in the range of science more surprising than the gradual appearance of the picture on the blank sheet, especially the first time the experiment is witnessed. The operator ought to watch the progress of the picture, until, in its strength of colour, sharpness of outline, and general distinctness, it has reached in his judgement the most perfect state. At that moment he stops further progress by washing it over with a fixing liquid. This is washed off with water, the picture is then dried, and the process is terminated.

The picture is found to be very strongly fixed, and from it numerous copies may be taken on common photogenic drawing paper, by the method of superposition in sunshine. The original picture does not readily become altered, or wear out by this exposure to the sun; but in case it does so, as happens sometimes, I find that it may be in general readily *revived*. This *revival*, which is a most curious particularity of the calotype process, not only restores the picture to its pristine strength, but frequently causes fresh details and minutiæ to appear in the picture, which had not appeared before, at the time when it was first *brought out*, or rendered visible (owing to that process having been checked too soon). These details, therefore, had been lying in an invisible state on the paper all this time, *not destroyed* (which is the most extraordinary thing) *by so much exposure to sunshine*. They were protected by the fixing liquid. But no one could have supposed beforehand, or without ocular demonstration, that it could have exerted so complete a protecting power. This is an invaluable property of the calotype—the power of reviving the pictures—not only because it allows so many copies to be made, but because it enables the artist to correct the error of his judgement, in case he has made too faint a picture at first, by stopping it too soon while it was coming out.

Some further details on this subject, and an account of the chemical processes employed, I reserve for a paper which I intend to lay before the Royal Society.

I am, &c. &c.

Lacock Abbey, Feb. 19, 1841. H. F. Talbot.

164

PROCESS FOR OBTAINING THE HYDROBROMIC AND HYDRIODIC ACIDS. BY DR. R. M. GLOVER.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In the latest Report of the British Association, there is an account of a new process for obtaining hydrobromic and hydriodic acids, said to be communicated by me. This account is quite incorrect. I first observed this erroneous report in the Athenæum, whence it was copied into several British and Foreign journals; but as I gave a correct abstract to Dr. Playfair, one of the Secretaries to the Chemical Section, when he passed through this town shortly after the meeting, I expected to see the error corrected in the Report of the Association.

It is known that all the solid bromides and iodides are decomposed by sulphuric acid with evolution of bromine and iodine; but I find that although the solid bromide and iodide of barium obey the general rule, their solutions are decomposed by sulphuric acid without any evolution of the elements, and with the formation of pure solutions of the hydracids. This difference is doubtless owing to the agency of the water in speedily removing the sulphuric acid in the form of the insoluble sulphate. The Report of the Association incorrectly states that I had proposed the solid iodide and bromide of barium acted on by sulphuric acid as convenient sources of the hydracids. My process is the very reverse.

In this way, in particular, hydrobromic acid can be conveniently obtained; more conveniently indeed than the hydriodic acid; because it is difficult to prepare the iodide of barium unmixed with the carbonate of baryta and free iodine, while the bromide does not undergo any decomposition from the carbonic acid of the atmosphere. A saturated solution of the bromide may be decomposed by a solution of sulphuric acid diluted with an equal bulk of water, the diluted acid being allowed to cool before it is added to the saline solution, and hydrobromic acid is set free unmixed with bromine. In this way the amount of the hydracid in any solution of it we may employ can be easily known, calculating from the weight of the bromide decomposed.

Should you insert this rectification, you will oblige

Your obedient Servant,

Robert Mortimer Glover, M.D.,
Lecturer on Chemistry in the Newcastle
School of Medicine.

Literary and Philosophical Society Rooms,
Newcastle, April 21, 1841.

NEW ALUM. BY DR. MOHR.

A variety of alum has been for some time introduced into Germany, which is recommended as containing the principles necessary to dyeing and calico printing in a state of great concentration; this quality is represented as rendering its use more advantageous, and less expensive in carriage. This alum has not the slightest resemblance to common potash alum; it exists in flat quadrangular tables, which are slightly transparent, and dissolve very readily in water; the taste of this salt is sweetish, acid, and aluminous, and much more distinctly so than common alum; when heated in a crucible it swells, and eventually becomes a gummy mass, which, when more strongly heated, gives out sulphuric vapours. Nevertheless the mass which has been heated to redness is entirely and readily soluble in water. If pulverized sulphate of potash be thrown into a concentrated solution of this alum, a crust of common alum is quickly formed. The usual reagents prove that it contains sulphuric acid, alumina, and a small quantity of potash, but no ammonia. M. Mohr found it to consist of

Sulphuric acid......	36·24	
Alumina	13·91	
Potash............	1·50	
Water	49·60	——101·15

This composition shows that the alum in question is, properly speaking, merely sulphate of alumina with an excess of acid, and combined with water of crystallization. The small quantity of sulphate of potash shows the origin of this salt, it being probably prepared with pulverized and calcined pipe-clay and sulphuric acid not completely concentrated. This new alum is entirely free from iron. —*Journal de Pharmacie*, t. xxvi. p. 633.

ON THE SUPPOSED HYDRATE OF PHOSPHORUS. BY M. MARCHAND.

M. Pelouze was the first chemist who supposed that the white crust which covers phosphorus that has been long kept, is a hydrate of this substance: M. H. Rose regards it merely as a modification of the state of aggregation: M. Mulder believes that it is a compound of oxide of phosphorus and phosphuretted hydrogen. He has remarked that the white sticks of phosphorus become of a red colour in aërated distilled water, and he immediately obtained a similar white substance by subjecting red oxide of phosphorus to the action of phosphuretted hydrogen.

M. Marchand being desirous of settling the question, pressed a quantity of the white crust, strongly, between folds of filtering paper, as had been done by M. Rose; he then placed it in a capsule over sulphuric acid *in vacuo*, and allowed it to remain so for several days. On the first day a feeble light was perceptible; but it soon disappeared: and when access of air was allowed the mass inflamed as if it had been phosphorus under similar circumstances.

A fresh quantity of the white crust was again dried over sulphuric acid, but under a receiver without a vacuum. M. Marchand placed it afterwards, as M. Rose had done, in a long weighed tube of glass, the lower end of which was closed. He ascertained the quantity, and immersed the tube in hot water. There was but little moisture disengaged, and it was very probably hygrometric, and the residue, which was converted into common phosphorus, was weighed, and found scarcely diminished.

In order to verify the assertions of M. Mulder, the author put white sticks of phosphorus into aërated water; he left them in it for several weeks without perceiving any alteration of colour; he then directed upon them a current of oxygen gas, without producing any sensible effect. He obtained, as M. Mulder did, the compound of oxide of phosphorus and phosphuretted hydrogen, and found that it strongly resembled the white crust; but the manner in which it was acted upon by heat, he found to be very different.

M. Marchand concludes, therefore, from his experiments, that the supposed white crust is merely, as was first supposed by M. Rose, phosphorus in an altered state of aggregation.—*Journal de Pharmacie*, tom. xxvi. p. 764.

NITRO-SACCHARIC ACID.

M. Boussingault states that this acid is obtained by dissolving sugar of gelatin in weak nitric acid. It is to be slightly heated, and on cooling the solution crystallizes; no other reaction takes place besides the mere dissolving of the sugar in the acid. This acid has a very sour taste, and also a slightly saccharine one. M. Boussingault analysed it in three different states: crystallized, dried, and as it exists in salts.

Dried at 230° Fahr., this acid contains,

	By experiment.	By calculation.	
Hydrogen	4·2	4·0	H^{42}
Carbon	18·1	18·2	C^{32}
Oxygen	56·5	56·3	O^{37}
Azote....	21·2	21·5	N^{16}
	100·	100·	

Nitro-saccharic of silver crystallizes very readily; this salt burns without detonating. Its composition is,

Hydrogen	1·86	
Carbon................	10·08	
Oxygen	27·63	
Azote	11·83	
Silver	48·60	——100·00.

It is stated by the author that nitro-saccharic acid dried at 210° Fahr. loses four equivalents of water, which are replaced in the nitro-saccharate of silver by four equivalents of oxide of silver, and the analysis of the nitro-saccharate of potash leads to the same conclusion; it was also observed by M. Boussingault, that it will appear on slight examination that the nitric acid remains unmodified.

The nitro-saccharate may be represented as resulting from the union of nitric acid with the corresponding saccharate, or as a compound of sugar of gelatin with a nitrate; and, in fact, the nitro-saccharates may be obtained by treating the saccharates with nitric acid.

The following are the formulæ of the various compounds of sugar of gelatin which have been mentioned:—

Sugar of gelatin	$C^{32} H^{36} N^8 O^{14}$
,, ,, in salts........... ..	$C^{32} H^{30} N^8 O^{11}$
,, ,, combined with silver	$C^{32} H^{30} N^8 O^{11} (Ag\,O)^4$
,, ,, ,, ,, copper	$C^{32} H^{30} N^8 O^{11} (Cu\,O)^4$
,, ,, ,, ,, lead	$C^{32} H^{30} N^8 O^{11} (Pb\,O)^4$
Nitro-saccharic acid crystallized.........	$C^{32} H^{30} N^8 O^{11} (N^2 O^5)^4 (H^2 O)^6$
Nitro-saccharic acid dried at 230° Fahr.....	$C^{32} H^{30} N^8 O^{11} (N^2 O^5)^9 (H^2 O)^3$
,, ,, in salts	$C^{32} H^{30} N^8 O^{11} (N^2 O^5)^4 (H^2 O)^2$
Nitro-saccharate of silver	$C^{32} H^{30} N^8 O^{11} (N^2 O^5)^4 (Ag\,O)^4 (H^2 O)^2$
,, ,, of potash	$C^{32} H^{30} N^8 O^{11} (N^2 O^5) (K\,O)^4 (H^2 O)^2$.

Journal de Pharmacie, t. xxvii. p. 37.

Preparing for Publication.

The Geology and Mineralogy of Engineering: including the Principles of the sciences of Œconomic Geology and Mineralogy as applied to the Arts. By EDWARD WILLIAM BRAYLEY, Fellow of the Linnæan and Geological Societies, Associate of the Institution of Civil Engineers, Corresponding Member of the Royal Geological Society of Cornwall, &c.

METEOROLOGICAL OBSERVATIONS FOR MAY 1841.

Chiswick.—May 1. Fine: very hot: clear. 2. Very fine: cloudy. 3. Rain. 4. Foggy: cloudy and fine: very heavy fall of rain at night. 5. Rain: cloudy and fine: lightning at night. 6. Rain: cloudy. 7. Fine: rain. 8. Heavy showers. 9, 10. Very fine. 11. Overcast: slight rain at night. 12. Cloudy and fine. 13. Fine. 14—16. Very fine. 17. Cloudy and windy. 18. Fine. 19. Rain with strong wind. 20. Boisterous: showery: cold at night. 21. Dry haze: rain. 22. Showery and mild. 23. Slight haze: cloudy and fine. 24. Fine. 25. Very fine. 26. Hot and dry. 27. Sultry: much sheet-lightning at night, with occasionally some of the zigzag and forked kind, together with thunder, and abrupt showers falling in large drops. 28. Sultry: very fine. 29. Very fine: lightning at night. 30, 31. Overcast and very fine.

Boston.—May 1. Fine: therm. 66° half-past 2 P.M. 2. Cloudy: rain with thunder and lightning P.M. 3. Cloudy. 4. Cloudy; rain early A.M. 5. Rain. 6. Cloudy: rain early A.M. 7. Cloudy: rain P.M. 8. Rain. 9. Fine. 10. Cloudy. 11. Fine: therm. 72° 3 o'clock P.M.: rain P.M. 12. Cloudy. 13. Fine. 14. Cloudy. 15. Fine: therm. 69° 3 o'clock P.M. 16. Fine. 17. Cloudy: rain P.M. 18. Cloudy. 19. Rain: stormy P.M. 20. Stormy: rain A.M. and P.M. 21. Fine. 22. Cloudy: rain early A.M. 23. Cloudy. 24—27. Fine. 28. Cloudy: therm. 79° 3 o'clock P.M.: rain P.M. 29. Fine. 30. Cloudy. 31. Fine. N.B. This May month has been warmer than any preceding May month since 1834.

Applegarth Manse, Dumfries-shire.—May 1. Fair and fine: thunder. 2. Drizzling afternoon. 3. Fair: frosty: hail. 4. Rain all day. 5. Rain occasionally. 6. Fair and fine. 7. Rain for four hours. 8. Rain P.M.: thunder. 9. Fair till night: rain P.M. 10. Fair but cloudy. 11. Wet A.M.: cleared up. 12—15. Fair and fine. 16, 17. Wet nearly all day. 18, 19. Showers A.M., then fine. 20. Showers. 21. Fair and fine. 22. Wet A.M.: fine P.M. 23. Fair but gloomy. 24. Fine summer day. 25, 26. Bright and cool. 27. Parching wind and hot sun. 28. Fine: rained a little. 29. Fair and fine. 30. Soft rain from eleven to four. 31. Fine summer day.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. Roberton; *by* Mr. Thompson *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. May.	Barometer.						Thermometer.									Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	30·040	29·998	29·794	29·44	29·90	29·68	57·8	67·2	47·0	76	42	58	65	40	NE.	E.	calm	SW.	...	...	...	...	52
2.	29·720	29·674	29·640	29·00	29·56	29·60	61·7	70·4	53·2	76	45	61	55½	45½	S.	SW.	calm	SE.	...	·25	...	...	57
3.	29·772	29·780	29·716	29·18	29·66	29·66	47·7	71·5	47·2	50	43	49	51	32½	N.	NE.	calm	SE.	·375	·30	·36	...	52
4.	29·628	29·609	29·480	29·13	29·48	29·25	53·7	54·8	45·3	69	50	46	51	40	E.	S.	SE.	SE.	·250	·70	·04	...	49
○ 5.	29·464	29·571	29·399	28·85	29·15	29·25	55·4	66·0	52·0	62	46	53	59½	49½	SE.	SW.	calm	SW.	·719	·15	·25	...	55
6.	29·556	29·774	29·495	28·91	29·31	29·42	57·8	61·3	53·0	65	43	57·5	55½	45½	S.	SW.	W.	S.	·077	·02	·08	...	55
7.	29·746	29·708	29·528	29·18	29·50	29·38	59·2	64·6	49·7	66	47	53	58	45	S.	S.	calm	SW.	·019	·10	...	...	56
8.	29·548	29·581	29·491	28·92	29·38	29·55	57·5	66·6	52·5	62	44	54	59	35	S.	SW.	W.	SSW.	·116	·16	·36	0·80	54
9.	30·094	30·097	30·008	29·44	29·75	29·80	56·3	72·2	47·3	65	50	56·5	55	37	S.	SW.	W.	SW.	·044	...	·02	...	52
10.	30·180	30·153	30·117	29·50	29·73	29·80	60·2	63·3	53·0	65	46	59	56	46	S.	SW.	W.	WSW.	...	...	...	...	53
11.	30·094	30·054	30·003	29·45	29·75	29·87	62·2	63·8	51·6	77	46	64	56½	50½	S.	S.	SW.	S.	...	·01	...	...	55
12.	30·190	30·247	30·128	29·55	30·06	30·19	57·0	70·7	53·0	62	39	55·5	57	40	NNW.	NE.	calm	WSW.	·019	...	·05	...	56
☾ 13.	30·388	30·330	30·314	29·77	30·25	30·26	55·7	63·4	47·5	66	36	58	60½	39	N.	NE.	calm	SW.	...	...	...	...	52
14.	30·424	30·364	30·262	29·83	30·23	30·11	53·7	63·6	46·3	65	40	55	62½	43½	NE.	NE.	calm	SW.	...	...	...	...	49
15.	30·244	30·194	30·043	29·59	30·00	29·84	59·7	69·4	48·7	74	40	61	59	44	S.	W.	calm	S.	...	...	...	...	52
16.	29·968	29·911	29·674	29·24	29·60	29·49	53·7	66·0	49·0	74	49	60	56½	46½	SSW.	SW.	W.	WSW.	...	...	...	0·64	50
17.	29·664	29·632	29·518	28·93	29·20	29·10	58·3	70·0	52·3	68	45	55·5	56	46½	S.	SW.	SW.	SW.	...	·01	...	...	55
18.	29·610	29·553	29·531	28·90	29·15	29·26	57·8	63·0	49·4	66	46	57	56½	46	S.	SW.	W.	SW.	...	·02	·02	...	54
19.	29·406	29·674	29·280	28·77	28·98	28·85	55·3	65·7	51·3	60	47	52	58	45	S.	S.	NW.	S.	...	·15	·06	...	53
● 20.	29·328	29·578	29·253	28·65	28·83	29·20	58·2	59·6	50·7	61	39	54	56½	45	W.	SW.	SW.	W.	·075	·06	...	...	53
21.	29·702	29·647	29·526	29·19	29·51	29·64	58·7	67·7	48·4	69	54	57·5	59	43½	ENE.	E.	calm	SW.	·044	·16	·09	...	52
22.	29·672	29·811	29·607	29·10	29·70	29·80	59·7	65·8	56·5	68	43	55	61	48	N.	S.	calm	SW.	·133	·01	·02	...	54
23.	30·040	30·159	29·994	29·45	29·90	30·05	58·3	64·2	53·7	75	46	56	61	49	S.	SE.	calm	E.	...	...	...	...	54
24.	30·242	30·180	30·137	29·61	30·10	30·10	63·5	70·3	53·6	70	51	65	65½	49	NE.	E.	calm	SW.	...	...	...	...	55
25.	30·220	30·165	30·133	29·69	30·25	30·27	63·5	70·6	55·8	73	54	65·5	64	43	N.	NE.	E.	E.	...	...	...	...	61
26.	30·146	30·070	29·955	29·58	30·17	30·03	67·7	70·0	55·0	80	58	61	67	46½	NE.	NE.	E.	E.	...	...	...	...	62
☽ 27.	29·960	29·882	29·823	29·35	29·95	29·85	71·7	79·4	62·0	82	60	69	78	51	NE.	NE.	calm	NE.	...	·03	...	...	66
28.	30·000	30·038	29·893	29·20	29·80	29·90	70·3	79·0	64·8	80	51	68	72	50	SSE.	SW.	calm	SW.	·116	...	...	1·80	65
29.	30·186	30·096	30·061	29·48	30·00	30·00	65·7	78·3	57·7	72	54	67	63	52½	NW.	N.	calm	SW.	...	·03	·05	...	55
30.	30·078	30·016	29·979	29·44	30·02	29·95	61·5	71·9	58·0	74	49	65·5	63	42	NW.	NW.	N.	SW.	...	...	...	...	59
31.	30·086	30·042	29·995	29·43	29·78	29·90	66·7	78·6	57·3	78	49	66	68	42	S.	W.	W.	SE.	...	...	...		62
Mean.	29·916	29·922	29·799	29·28	29·700	29·711	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]					[illegible]	[illegible]	[illegible]	[illegible]	[illegible]

THE

AMERICAN JOURNAL

OF

SCIENCE AND ARTS,

CONDUCTED BY

PROF. B. SILLIMAN, M. D., LL. D.,

F. G. S. London and Paris, &c. &c.

AND

B. SILLIMAN, JR., A. M.,

Of the department of Chem., Min. and Geol. in Yale College.

For Jan. 1841, No. 81, or Vol. 40, No. 1.

WILEY & PUTNAM,

35 Paternoster Row, London, and 161 Broadway, New York.

THIS Journal will be delivered punctually in any part of Great Britain or Ireland, and all orders directed to W. & P. will receive prompt attention. It is the only American Journal exclusively devoted to the cause of General Physical Science, and has now continued in an unbroken series for more than twenty one years. It is no vanity on the part of the conductors to say, that its pages embrace a larger portion of original communications on American Science than any other Journal. The proceedings of the American Philosophical Society are regularly given, as also of the Boston Society of Natural History, and other similar American Societies. It is fully illustrated by engravings, lithographs, and wood cuts.

The general character of the work may be inferred from the annexed TABLE OF CONTENTS of the last number for January, 1841.

Opinions of the Work.

THE following extracts from private letters and the journals, are made from among a very great number of favorable notices of the work both at home and abroad.

"The character of the *American Journal* is strictly national, and it is the only vehicle of communication in which an inquirer may be sure to find what is most interesting in the wide range of topics which its design embraces. It has become, in short, not more identified with the sciences than the literature of the country."—*Extract from a letter to the senior Editor, from the author of the Life and Writings of Washington.*

One of the oldest and most celebrated of British geologists, in a letter to the Editors, expresses the opinion—"that the American Journal has presented in its pages a greater amount of original matter than any journal published in the English language."

"The high estimation in which we hold this work, will be best judged by the copious extracts which will be found in our present and earlier numbers."—*The Mining Review and Journal of Geology and Mineralogy, conducted by Henry English, Esq., F. G. S., London.*

"It has seldom been our lot to meet a philosophical work so interesting to the general reader."—*ib.*

"This excellent periodical continues to be supported not only with undiminished vigor, but with an increasing amount of original and enriching communications from the gifted friends of science in our country. It has now acquired a veteran constitution, and is destined, we hope, to transmit to very distant ages, the fruits of American ingenuity, research, and discovery. It has not only gained, within twenty one years of its existence, an American and European, but, as far as the English language has been made the vehicle of knowledge, a cosmopolitan reputation. Its track of usefulness differs so materially and essentially from that of our own periodical, that we feel no other rivalship than those of good wishes and the most friendly desires for a permanent prosperity."—*Journal of the Franklin Institute, Philadelphia, Sept.* 1840.

ADVERTISING SHEET.

All advertisements of New Works, &c. which are forwarded to W. & P. will be inserted either on the cover or on an advertising sheet, at the customary rates. The American circulation of the work renders it a peculiarly fit vehicle for English publishers and authors, whose notices will thus be extensively circulated in America.

CONTENTS.

Page.
Art. I. Notices of European Herbaria, particularly those most interesting to the North American Botanist, . . . 1
II. Fragments of Natural History; by Prof. J. P. Kirtland, M. D., ... 19
III. Description of a Halo or Corona of great splendor, observed at Greensburgh, Westmoreland County, Pa; by Alfred T. King, M. D., ... 25
IV. Extracts from the Proceedings of the American Philosophical Society, ... 27
V. Additional Remarks on the Tails of Comets; by William Mitchell, ... 59
VI. Notice of a Locality of Zeolites, &c., at Bergen, Bergen County, New Jersey; by Wm. Oland Bourne, . . . 69
VII. Notice of the Geological Survey of the State of New York, presented to the Legislature, Jan. 24, 1840; by Prof. Oliver P. Hubbard, M. D., ... 73
VIII. On the Magnetic Dip in the United States; by Prof. Elias Loomis, ... 85
IX. Description of some new species of Fossil Shells, from the Eocene, at Claiborne, Alabama; by Henry C. Lea, 92
X. A Description of several New Electro-Magnetic and Magneto-Electric Instruments and Experiments; by Joseph Hale Abbot, ... 104
XI. Development of some interesting Properties of Numbers; by George R. Perkins, ... 112
XII. Remarks on the Geological Features of the Island of Owyhee or Hawaii, the largest of the group called the Sandwich Islands, with an account of the condition of the Volcano of Kirauea, situated in the southern part of the Island near the foot of Mouna Roa; by Edward G. Kelley, ... 117
XIII. The employment of Iodine as a reagent for Hydrosulphuric Acid; by M. Alphonse du Pasquier, ... 123
XIV. Notice of Geological Surveys. I. Of the State of Ohio. II. Of Indiana. III. Of Michigan; by Prof. Oliver P. Hubbard, M. D., ... 126

Page.

XV. The Daguerreotype and its Applications; by W. H. Goode, 137

XVI. Supplementary Note to the Article on the Pneumatic Paradox in the last number of this Journal; by Joseph Hale Abbot, ... 144

XVII. Miscellaneous Observations on Insects, &c.; by Dr. John T. Plummer, ... 146

XVIII. On Terrestrial Magnetism; by Prof. John Locke, M. D., 149

XIX. Electrography or the Electrotype; by the Junior Editor, 157

XX. Bibliographical notices:—Report on the Tea Plant of Upper Assam, 165.—Report of M. Guillemin, 167.—The Spiritual Life of Plants, 170.—The Journal of Botany, &c., 172.—Hooker's Flora Boreali-Americana, or the Botany of the Northern parts of British America, 173.—Endlicher's Genera Plantarum: Enumeratio Chenopodearum: Stendel's Nomenclator Botanicus: Caricography: Fossil Infusoria in England, 174.—Chemical composition of cellular and woody tissue in Plants, 176.—Organic Chemistry in its applications to Agriculture and Physiology, 177.—Report on the Geological and Agricultural Survey of the State of Rhode Island in 1839, 182.—History of Embalming and of Preparations in Anatomy, Pathology, and Natural History, including an account of a new process for Embalming, 194.—A General Outline of the Animal Kingdom: Boston Journal of Natural History, 196.—Supplement to the introduction to the Atomic Theory, 197.

Miscellanies.—Horticultural Experiments, 197.—Volcanic Ashes, 198.—African Meteorite of Cold Bokkeveld, 199.—Further account of the Shooting Stars of August, 1840, 201.—Meteors of November, 202.—Meteoric Observations in October and December, 1840, 203.—Meteorological Notes in 1741-1757, 204.—Galle's Three Comets: New Comet: Manufacture of Glass for Optical Instruments, 207.—Parasite of the eggs of the Geometra vernata, 211.—Circular of the Royal Society of Northern Antiquaries, 212.—Level of the Dead Sea: Preservation of Timber: Preservation of Timber long sunken under water, 213.—New process for making Sulphuric Acid, 214.—Oxalic Ether with Chlorine: Elaterite, or Fossil Caoutchouc, 215.—Gold in France: Artificial preparation of Sugar: Action of Alcohol upon Alkalies, 216.—Iodine in Coal: Six new species of Kangaroo, 217.—Proceedings of the Tenth Meeting of the British Association: Necrology, 218.

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

AUGUST 1841.

XVIII. *On some Electro-Nitrogurets.* *By* W. R. GROVE, *Esq. M.A. F.R.S.*[*]

THE ammoniacal amalgam, or that extraordinary substance which is formed by electrolysing solutions of ammonia or its salts with mercury at the cathode, first discovered by Dr. Seebeck, has at various periods attracted the attention of the most distinguished chemists, and from being the apparent link between the metals and permanent gases, must ever be an object of deep interest. The experiments which I here venture to lay before the Royal Society, will be found, I trust, to throw some light upon this compound, to connect the ammoniacal amalgam with the experiments performed by different philosophers upon the decomposition of ammonia during its passage over heated metals, to explain some anomalies in these effects, and to make known some new and curious combinations.

The first experiment which I made on the ammoniacal amalgam was to freeze it, which, by the kindness of Mr. Everett, I was enabled to do in the laboratory of the Middlesex Hospital, and at which experiment Professor Schönbein, of Basle, was present. A mass of the compound formed by immersing an amalgam of potassium in muriate of ammonia was subjected to solid carbonic acid liquefied in the usual way by sulphuric æther; it froze readily, but at what exact temperature we had no means of ascertaining; at the moment of solidifying it contracted slightly, but without giving out any gas; in its solid state it could be broken, and its fracture was of a very dark gray, or nearly black colour, something resembling that of cast iron which has been exposed for some time to the air; it had little or no metallic lustre: on thawing it gave out ammonia, hydrogen and nitrogen.

---

[*] Communicated by the Author; having been read before the Royal Society, on the 4th of February, 1841.

My next attempt to procure a solid ammoniacal compound of this nature, was, by forming an extremely fusible alloy of mercury, and Newton's fusible metal. I obtained one which was solid at ordinary temperatures, but which fused at 86° Fahrenheit. I made this the cathode of a voltaic battery in a solution of ammonia gently heated; it remained perfectly unaltered, giving off the gases as a solid electrode would have done. It now occurred to me that by subjecting a solution of one of the salts of ammonia to electrolysis, having at the anode an oxidable metal; this would be dissolved and revived at the cathode in conjunction with nascent nitrogen and hydrogen, and thus a permanent compound might be formed; to effect this it was necessary that the oxide or chloride of the metal should be easy of reduction, soluble in, and not precipitable by, ammoniacal salts; thus lead, iron, and platina offered little hope of success, and on trial proved ineffectual. Tin was also inefficient from the quantity of insoluble peroxide and oxychloride formed; with this metal indeed a small quantity of dark-gray spongy matter collected at the cathode, but was soon reacted upon by the solution. With zinc, copper and cadmium, however, the results were very different; and the compounds formed and methods of forming them, I will describe separately. A long glass trough was filled with distilled water, into which were placed several lumps of muriate of ammonia; a platina wire forming the cathode, and a button of distilled zinc suspended by a platina wire, the anode of six pairs of zinc and platina plates (charged, as originally proposed by me (Phil. Mag. for May 1839), with dilute sulphuric and concentrated nitric acid,) were immersed in the liquid at the two extremities of the trough, and separated by a fence of crystals of the salt. A spongy mass, which appeared to be formed of a cluster of folia or irregular facets, of the colour and lustre of plumbago, soon formed around the cathode, and as it increased in size rose up and floated; this specific levity was, however, principally due to the gas entangled in its interstices, as when slightly compressed between two strips of glass it instantly sank; having formed a sufficient quantity of this sponge it was gently removed, placed on a filter, and well washed with distilled water, then dried over a sand-bath at a gentle heat, during which it gave out no ammoniacal odour; then pulverized by rubbing between the folds of filtering paper; when thus dried and reduced to powder its colour was unaltered; it had no metallic lustre: the specific gravity of a portion of it which I tried, proved to be 4·6; it conducted electricity, and was acted on by acids precisely as metallic zinc, but the hydrogen evolved from it by dilute sulphuric acid was mixed with a little nitrogen; when this solution was supersaturated with caustic potass it evolved no ammoniacal odour.

Five grains of the substance well dried, were placed in a narrow tube bent so as to form a retort, and heated to redness by a spirit-lamp; they gave out a permanent gas, which was at first collected over recently boiled mercury, but as no ammonia was detected, the gas in subsequent experiments was collected over water: the following is the result of four experiments, the air in the tube having been first rarefied and a portion expelled by heating it at parts distant from the substance:—

Experiment.		Cubic inches.
1st	5 grains gave	0·7
2nd	————	0·75
3rd	————	0·76
4th	————	0·72

4)2·93(0·73

This gas was examined by detonation in an eudiometer, which differed from those of Volta and Henry in being furnished with a loop of platina wire, which could be ignited at pleasure by a feeble voltaic current; this eudiometer I have found to possess many advantages, but it would be out of place here to dilate upon them. The gas expelled from the zinc compound, when mixed with one-eighth of oxygen, detonated, and contracted to five-eighths of the whole volume; the residue suffered no further contraction by oxygen or hydrogen, and exhibited all the negative characters of pure nitrogen: upon several subsequent examinations portions of the original gas were found to contain from one-third to one-fourth hydrogen, and on examining the very last portion, or that which remained in the retort, I found it to leave a very small residuum when detonated with oxygen; the last portions of gas given out contained a greater proportion of hydrogen than the first, it thus appearing that the nitrogen was evolved at a lower temperature than the hydrogen; the residuum in the tube was unaltered as to colour, but contracted in bulk, and a little dew had collected in the bend of the tube.

Cadmium was subjected to the same process as zinc; its electrolysis differed somewhat from that of zinc; with the latter metal the solution was during the whole time perfectly transparent, and no precipitate formed; with cadmium, after a short time, the hydrated oxide was precipitated by the ammonia formed at the cathode; but the portions of this precipitate contiguous to the cathode, being redissolved by excess of ammonia, were revived by the voltaic current; the sponge formed very rapidly; its growth could be distinctly seen; it was of a very dark leaden gray approaching to black, and resembled very closely the mercurio-ammoniacal amalgam; it had not the semi-crystalline appearance of the zinc sponge, but when pressed between glass had a leaden lustre and very much the appearance of an amalgam; if left in the solution, it was observed to contract and acquire a lighter shade of colour, most probably from the reaction of dissolved chlorine; the portions to be examined were, therefore, removed instantly and plunged into distilled water. The specific gravity was 4·8, analysed in the same way with the product from zinc; four experiments gave of permanent gas,

	Cubic inches.
1	0·25
2	0·18
3	0·20
4	0·20
	4)·83
Mean	·207

This gas, however, when examined, proved to contain no hydrogen; the residue in the tube was of a yellowish green colour mixed with globules of distilled cadmium.

Copper subjected to similar electrolysis produced a bright blue transparent solution, in which, at the cathode, grew a beautiful purple or rather chocolate-coloured coagulum; this frequently, but not always, floated upon the liquid; its specific gravity was 5·9. Five grains, when heated, gave of permanent gas,

Cubic inches.
0·13
0·10
0·09
0·11
4) ·43
·107

This gas also contained no hydrogen, but was wholly nitrogen;

the residue in the tube had changed colour from chocolate to red, and had all the characters of pure copper.

I now tried nickel and silver with the nitrate of ammonia and gold with the muriate; with nickel very little spongy matter was formed; I could not collect sufficient for analysis, and the fearful nature of the ammoniacal precipitate prevented my collecting sufficient of the silver deposit. To obtain an analogous compound of gold I mixed its chloride with a solution of muriate of ammonia, and electrolysed with platina electrodes, divided by a pipe-clay diaphragm, to prevent the reaction of the chlorine; a black deposit was formed, which did not float, and was but little coherent; its specific gravity was 10·3, and five grains gave 0·05 cubic inch of gas; from the small quantity obtained I could not positively pronounce as to its nature, but it appeared to be nitrogen. I have also formed amalgams by using mercury at the cathode with zinc, &c. at the anode, but the mercury, even when alloyed, would not retain the gases, and the amalgam yielded no more than the solid metal alone would have done.

In forming each of these substances the process must be continually watched, as, if the solution of the metal be in excess with respect to that of the ammoniacal salt, the metal is reduced alone; this is easily noted in the case of copper from the difference of colour; at a certain stage of the process the chocolate coagulum becomes fringed with dendritic fibres of pure copper, and the line of demarkation is clearly visible; in the zinc and cadmium compounds this effect is not so evident, but a very little practice enables one to recognize the difference, the pure metal being of a lighter gray, and assuming a regular arborescent appearance instead of the amorphous sponge; it generally commences on the under part, from the greater specific gravity of the metallic solution; when this reduction of pure metal has once commenced it continues, and to form again the nitroguret, the solution must be changed, and the process commenced *de novo*.

In all these compounds the quantity of nitrogen is far under the equivalent proportion, and in many cases gradations of shade could be observed in the sponge, on which occasions less nitrogen was found to have entered into combination; these gradations were somewhat abrupt, the mass having a stratified appearance like clay of different shades rolled up; the averages I have given were from portions of the deepest colour; an increase of power did not deepen it beyond a certain point; but whether a very high power will cause more nitrogen to combine with the metals, I have as yet had no opportunity of proving.

From the specific gravity of the cadmium compound I expected it to have yielded more nitrogen, and from the yellow colour of the residue I consider that some of this element, with probably hydrogen, formed a new combination analogous to that of ammonia and potassium examined by Sir Humphry Davy; to enter upon this analysis will require a new series of experiments, foreign to the immediate purpose of this paper. Another question of difficult resolution is, whether the hydrogen expelled from the zinc compound was in immediate combination, or resulted from the reaction of the zinc upon combined water: I incline to the opinion that the latter was the case, but see no means of proving it, as the same heat which would expel the water would decompose the substance. Although I had no expectation of procuring a solid compound (the theoretical ammonium), yet I did not neglect the trial; I distilled in the vapour of naphtha some of the zinc compound, but obtained only gaseous products. It may be worth remarking, that the quantities of nitrogen which enter into combination with the metals in these experiments is in proportion to their affinity for oxygen, and it may, perhaps, be considered an argument, in addition to the many already advanced, to prove that nitrogen is an oxide.

Independently of their individual interest, the strong analogy in character, and the manner of formation between these compounds and that of the ammoniacal amalgam, is (to me at least) satisfactory evidence of similarity of constitution. Berzelius supposed this to be an amalgam of mercury and ammonium. Gay-Lussac and Thenard supposed it to be a combination of nitrogen, hydrogen, ammonia and mercury. Mr. Daniell has advanced an opinion, supported by some experiments, that it is not a chemical combination, but a mere heterogeneous adhesion, the mercury being puffed up as a soap-bubble. If the above experiments entitle me to advance an opinion without incurring the charge of presumption, I should say that it was certainly a chemical combination, possibly of mercury, nitrogen and hydrogen, but more probably of mercury and nitrogen swelled with hydrogen; and from the circumstance of its being always moist, containing necessarily a good deal of ammonia, that its non-permanence is due solely to the mobility of the mercury; for place this *quatuor pedibus* with the other metals, i. e. solidify it, and the compound is perfectly permanent; reduce the other analogous metallic compounds to the state of the mercurial one, i. e. fuse, or even heat them, and they cease to be permanent. With regard to the hypothesis of ammonium, have we a right to assume the existence of a metal because the nitroguret or ammoniuret of mercury bears a physical resemblance to an amalgam, might we not, upon the same principle, assume many other elements? As a theoretical question in the mind of an experimenter, it can do no harm; but as a recognized base, I cannot but think it may tend to mislead.

Another class of facts explained by the experiments above detailed, are those exhibited in the decomposition of ammonia during its passage over heated metals, first observed by, I believe, Berthollet, who remarked, that iron subjected to this process became brittle, but did not change in weight. Thenard, in similar experiments with iron, copper, silver, gold and platina, could not detect an increase in weight of above $\frac{1}{500}$th; Savart observed an increase of about $\frac{1}{500}$th. Despretz, however, although in his first experiments he could get no greater increase than Savart, yet on a repetition found that in the case of iron, and iron only, he could increase its weight 11 per cent.; he does not state the reason of his first failures, or of his subsequent success.

These somewhat discordant results appear to receive an easy explanation by my experiments, an explanation, indeed, first theoretically suggested by Ampère, viz. that in passing heated ammonia over the metals a nitroguret was formed and immediately decomposed; that thus the physical structure of the metals was changed without any notable increase of weight or chemical alteration.

Before concluding this paper, I ought not to omit a passage in one of Mr. Daniell's papers, which, upon a search made subsequent to my experiments, is the only one I have found bearing upon them. After stating some results with his constant battery charged with muriate of ammonia on the zinc side of the diaphragm, and sulphate of copper on the copper side, he makes the following remark:—

"There were no indications of free ammonia in the exterior cell; the precipitated copper, however, did not exhibit the beautiful, bright, pink hue which it ordinarily presents, but was of a dull, grayish, earthy appearance, resembling that of copper over which ammoniacal gas has been passed at a red heat, and probably contained some combined nitrogen. I had not, however, time to examine a compound which is worthy of further investigation."

All the copper nitrogurets which I have formed have had a chocolate colour, and the nearest substance to which I can liken them is the coagulum of the blood; the grayish colour remarked by Professor Daniell proceeded probably from some zinc being precipitated with the copper, or possibly the temperature at which he wrought (124°) occasioned some difference. With the exception of the above sentence I have found no anticipation of my results, and as far as my own knowledge and inquiries extend, am justified in giving them as new.

XIX. *On some supposed forms of Lightning.* *By* Michael Faraday, *D.C.L.*, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE magnificent display of lightning which we had on the evening of the 27th of last month, and its peculiar appearance to crowds of observers at London, with the consequent impressions on their minds, induces me to trouble you with a brief letter on certain supposed appearances and forms of lightning, respecting which the judgment of even good observers is often in error.

When, after a serene sky, or one that is not overcast, thunder-clouds form in the distance, the observer sees the clouds and the illumination of the lightning displayed before him as a magnificent picture; and what he often takes to be forked lightning (i. e. the actual flash, and not a reflexion of it), appears to run through the clouds in the most beautiful manner. This was the case on that evening to those who, being in London, observed the storm in the west, about nine o'clock, when the clouds were at a distance of twenty miles or more; and I have very frequently observed the same effect from our southern coasts over the sea. In many of these cases, that which is thought to be the electric discharge is only the illuminated edge of a cloud, beyond and behind which the real discharge occurs. It is in its nature like the bright enlightened edge which a dark well-defined cloud often presents when between the sun and the observer; and even the moon also frequently produces similar appearances. In the case of its production by lightning and distant clouds, the line is so bright by comparison with the previous state of the clouds and sky, so sudden and brief in its existence, so perfectly defined, and of such a form, as to lead every one at the first moment to think it is the lightning itself which appears.

But the forms which this line assumes, being dependent on the forms of the clouds, vary much, and have led to many mistakes about the shape of the lightning flash. Often, when the lightning is supposed to be seen darting from one cloud to another, it is only this illuminated edge which the observer sees. On other occasions, when he was sure he saw it ascend, it was simply this line more brilliant at its upper than at its lower part. Some writers have described curved flashes of lightning, the electric fluid having parted from the clouds, gone obliquely downwards to the sea, and then turned upwards to the clouds again: this effect I have occasionally seen, and have always found it to be merely the illuminated edge of a cloud.

I have seen cases of this kind in which the flash appeared to divide in its course, one stream separating into two; and when flashes seen at a distance are supposed to exhibit this rare condition, it is very important the observer should be aware of this very probable cause of deception.

I have also frequently seen, and others with me, a flash having an apparently sensible duration, as if it were a momentary stream, rather than that sudden, brief flash which the electric spark always presents, whose duration even Wheatstone could not appreciate. This I attribute to two or three flashes occurring very suddenly in succession at the same place, or nearly so, and illuminating the same edge of a cloud.

The effect I have described can frequently be easily traced to its cause, and when thus traced best prepares the mind to appreciate the mistakes it may lead, and has led, to in the character, shape and condition of the lightning flash. It often happens at the sea-side, that, after a fine day, clouds will towards evening collect over the sea on the horizon, and lightning will flash about and amongst them, recurring at intervals as short as two or three seconds, for an hour or more together. At such times the observer may think he sees the lightning of a flash; but if he waits till the next illumination, or some future one, takes place, he will perceive that the flash appears a second time in the same place, and with the same form; or perhaps it has travelled a little distance to the left or right, and yet has the same form as before. Sometimes an apparent flash, having the same shape, has occurred three or four times in succession; and sometimes it has happened that a certain shaped flash having appeared in a certain place, other flashes have appeared in other places, then the first has reappeared in its place, and even the others again in their places. Now in all these cases it was simply the illuminated edges of clouds that were seen, and not the real flashes of lightning. These forms frequently appear to be in the cloud, and yet are not distinguishable till the lightning occurs. It is easy, however, to understand why they are then only developed, for that which appears in the distance to be one dull mass of cloud, distinguishable in figure only at its principal outline, often consists of many subordinate and well-shaped masses, which, when the lightning occurs amongst or beyond them, present forms and lines before unperceived.

The apparent duration, which I before spoke of, is merely a case of very rapid recurrence, and may, by a careful observer, be easily connected with that which I have now proposed as the best test of the nature of the phænomenon.

There are some other circumstances which will help to distinguish the effect I have thus endeavoured to describe from the true appearance of the lightning flash, as the apparent thickness, sometimes, of the supposed flash, and its degree of illumination; but I have, I think, said enough to call attention to the point; and, considering how often the philosopher is, in respect to the character of these appearances, obliged to depend upon the report of casual observers, the tendency of whose minds is generally rather to give way to their surprise than to simplify what may seem remarkable, I hope I have not said too much.

I am, Gentlemen, your obedient servant,

June 22, 1841. M. FARADAY.

XX. *Remarks on* Professor Poggendorff's *Paper on the Intensity of Current of the Zinc-iron Circuit. By* MARTYN J. ROBERTS, *Esq., F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

CIRCUMSTANCES preventing my having access to your Magazine, it was long after the publication of your 114th Number that my attention was called to it as containing a translation of Prof. Poggendorff's Memoir, read before the Berlin Academy, upon the discovery made by me of an anomalous condition of iron in the galvanic circuit*.

In this memoir the learned Professor states that I have offered no opinion in explanation of the phænomenon, and that indeed a satisfactory one could hardly be given with the present views of electrical science in England. I must confess myself to blame in not having given the experiments in greater detail, as it might have prevented the mistake the learned Professor has evidently fallen into as to the cause of the phænomenon, and I trust the condition of electrical science in England is not so low as to prevent our giving an explanation of so simple a matter; indeed I am surprised that in repeating my experiments the learned Professor did not at once perceive the true solution of the apparent anomaly.

In his memoir he appears to attribute the superiority of the *iron-zinc* over the *copper-zinc* galvanic pair to some inherent power of resistance to the reception of electric fluid, existing in a greater degree in copper than in iron; and if I understand him rightly, he supposes this to be not a less facility of conduction in the copper, but a resistance to the entrance of the electric fluid into the substance of the metal from another conducting medium in contact with it; that is to say, it is more difficult for electricity to enter copper from an acidulated solution in which this metal is immersed, than to enter iron in like circumstances, although the copper is actually a better conductor than iron when the electric fluid has once penetrated its substance. This I grant to be the fact, but not that the cause is any such singular and contradictory property in copper as first to offer a greater and then a less resistance to the passage of electricity; but it is simply that copper, when immersed in an acidulated solution, does not retain so clean a metallic surface as does iron when exposed to a like action.

---

* Phil. Mag. Third Series, vol. xviii. p. 42.

When a *copper-zinc* pair is placed in dilute sulphuric acid an action takes place upon both the metals, and the balance of their affinities for the acid determines the direction and intensity of the electric current; but an obstacle to its free circulation arises by the resistance offered to its passage from the acid into the copper, because this metal has in a measure been acted upon by the acid and its surface partially oxidated; but as the affinity of the base for the acid under these circumstances is not sufficient to cause the solution of the oxide, it therefore remains upon the surface of the copper plate, and as oxides are worse conductors of electricity than their metallic bases, we have here a resistance presented by the oxidated surface to the entrance of the electric current into the copper plate. On the other hand, when an *iron-zinc* pair is immersed in dilute acid, we have also an action on both metals, but the balance of affinities is here not so much in favour of the zinc as when it is in combination with copper, therefore the *intensity* or *electromotive force* generated by the iron-zinc is not so great as in the copper-zinc battery; but as I have shown, the *quantity* circulated by the iron-zinc is greater, because the surface of the iron not only oxidates as did the copper, but in consequence of its greater affinity for the acid this oxide becomes dissolved in the liquid, and it is thus removed from the surface of the metal, which remains purely metallic, bright, and far more fitted to conduct electricity than would be the oxidated surface of a copper plate; it therefore offers less resistance to its entrance, and a larger *quantity* is thus circulated, although (in consequence of the balance of affinities) in less *intensity* or *electromotive force* by an iron-zinc than by a copper-zinc galvanic pair.

I think this explanation is proved to be the true one, both by Prof. Poggendorff's experiments and by my own. I found,—1st, When comparing a ten-pair iron-zinc with a ten-pair copper-zinc battery, that although in a given time of considerable duration a much greater quantity of water was decomposed by the iron than by the copper battery, yet that at the first immersion of the plates in acid a much more rapid decomposition was effected by the copper than by the iron combination, although in a very short time this superiority disappeared, and the iron became the strongest.

2ndly. When a single pair, copper-zinc, was tried against a similar pair, iron-zinc, and their powers tested by a differential galvanometer; at the first immersion the deviation was in favour of the copper, but in a short time the iron overcame it by a deviation of fifty degrees.

3rdly. When these pairs were inspected at intervals, it was seen that the iron preserved a bright metallic surface, while the copper was covered by a dark coat of oxide.

4thly. A copper-zinc pair was tested against an iron-zinc pair of equal surface, to ascertain the comparative loss of metal by oxidation and solution during the working of these little batteries, which were allowed to remain in action for about twelve hours; result as follows:—

	Original Weight.			Weight after 12 Hours' Action.			Loss.		
	drs.	s.	grs.	drs.	s.	grs.	drs.	s.	grs.
Iron . .	9	0	2	8	2	16	0	0	6
Zinc . .	6	1	10	5	1	8	1	0	2
Copper .	4	0	17	4	0	17	None.		
Zinc . .	6	0	17	4	2	17	1	1	0

showing that the copper loses none of its weight when in communication with zinc; but that iron lost six grains under similar circumstances.

From these experiments we may conclude, that as at the first immersion the copper-zinc is superior to the iron-zinc, but afterwards becomes inferior to it, some change must have taken place during this interval which reduces the electric current of the copper-zinc battery; this reduction is proved by the first and second experiments; what the change is was seen by inspection in the 3rd experiment, viz. a clean surface of iron, and an oxidated surface of copper. Lastly, that this bright iron surface is caused by the solution of the oxide of the metal, is proved by the 4th experiment, where the iron lost six grains of its weight.

The cause, then, of the anomalous electric condition of iron is not that this metal possesses a less power of resistance to the transition of electric fluid than does copper, although in reality a worse conductor than this metal, a contradiction which has been maintained by Prof. Poggendorff; but it is, that iron, when in galvanic action, retains a more bright and metallic surface than copper does in like circumstances. This is also the case with all metals possessing the apparently anomalous property of being positive to a metal A, when associated with it as a galvanic pair; but more highly negative, or at least giving a stronger electric current than this metal A would, if associated with a metal B, still more positive than either of the former. I may call the attention of experimenters to the œconomy and power of the *iron-zinc* battery, which, by a simple contrivance to ensure a regular supply of acid, can be made as constant in its action as would be desired for ordinary purposes.

Compared with the *copper-zinc* battery, the following results show its power and œconomy:—

A small ten pair *iron-zinc* tested against a similar sized *copper-zinc* battery: an equal measure of dilute acid was poured into each battery; both were employed in decomposing water until all action ceased. The iron-zinc produced, by the expenditure of this acid, four cubic inches of mixed gases in 104 minutes. The copper-zinc produced, by a like expenditure of acid, only one and a half cubic inches of mixed gases, and this required 125 minutes. The expenditure of zinc is also much less when associated with iron than with copper, and this in the proportion of twenty to twenty-seven.

Prof. Poggendorff states, that it is necessary to give a zinc-platina battery three times as large a surface as that of a zinc-iron one to produce equal effects; and as zinc-platina has hitherto been considered the best galvanic arrangement, with the exception of Daniell's, the œconomy of the zinc-iron battery is manifest.

I am, &c.,

MARTYN J. ROBERTS.

XXI. *Researches on Heat.—Fourth Series. On the Effect of the Mechanical Texture of Screens on the immediate transmission of Radiant Heat.* By JAMES D. FORBES, *Esq., F.R.SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.*

[Continued from p. 81, and concluded.]

26. A MORE minute analysis of the influence of surface upon heat is what we now propose. And three questions present themselves for immediate solution:—(1.) If deficiency

of polish produce a variation in the proportion of not less than 8 to 1 in the quantity of transmitted radiated heat from different sources, can we employ salt plates with the ordinary degree of polish, and yet consider them as equally transparent for every kind of heat, as M. Melloni's discovery has hitherto entitled us to do? (2.) Is the effect of roughness common to other substances as well as rock-salt? (3.) The operation of depolishing with sand-paper is nothing more than the making of an infinite number of distinct grooves on a polished surface; supposing these grooves to be regularly formed, and capable of numerical estimation, will the effect continue?

27. (1.) With respect to the first of these questions, it is satisfactory to be able to answer it affirmatively in a general way. I took two salt plates, of which the surfaces had not been *regularly* polished for a long time, and which, though bright and clear, were by no means particularly even and true. Of heat from Locatelli's lamp previously sifted by glass, these *four surfaces* of rock-salt transmitted 72 per cent. With dark heat from smoked brass the per-centage was 73, a difference which, in this experiment, could hardly be considered as appreciable. *The transmission of these two very different kinds of heat was therefore equal.* M. Melloni has shown that when rock-salt is pure and perfectly polished, ·92 of the incident heat is transmitted by a pair of surfaces, and therefore four surfaces should transmit $(\cdot 92)^2$ or 84·5 per cent. This estimate I have verified, and am satisfied of its accuracy. The deviation in the present case (which I think it right not to pass over) is due partly, no doubt, to the inequalities of surface, but chiefly to some imperfections in the salt itself, which, as the experiment was merely a relative one, were not adverted to. In contrast with this, I used at the same time (December 11, 1839) a piece of salt, which once had been polished on both sides, but which, by being laid aside for some years, had become completely dull and gray on its surface. This specimen, then, was simply *depolished*; it contained no furrows, and had been subjected to no mechanical action whatever. Its per-centage of transmission was,

	Locatelli, with Glass.	Dark hot Brass.
Tarnished salt	66	77

clearly establishing the general principle.

28. (2.) With respect to the question, whether roughness of surface has a similar effect in modifying the diathermancy of other substances as well as rock-salt, we are able to give a distinctly affirmative answer. Rock-salt being, so to speak, quite indifferent to the quality and source of the incident heat,

any cause of specific action becomes immediately apparent. Not so with any other substance, which, exercising already a specific action in virtue of its nature, is to have that specific action modified by a modification of surface. At least the question is, whether or not this modification will occur? An example will best illustrate how this modification may be discovered and expressed. I took a plate of mica with its natural bright surfaces, and so thin as to transmit in considerable abundance heat from different sources. The per-centages in this state were determined as follows:—

	Locatelli, with Glass.	Locatelli.	Dark Heat.
Mica with *bright* surfaces ...	83·5	74	37

Both sides of the mica were depolished with emery-paper, and the experiment repeated (27th November, 1839):

	Locatelli, with Glass.	Locatelli.	Dark Heat.
Mica with *rough* surfaces, ...	45·5	51	31·5

Denoting the original transmissions by 100, the diminished effect due to the roughness of the surface will be represented by

54 69 85

demonstrating as clearly as possible that the stoppage is proportioned to the temperature of the source of heat; thus, whilst 46 per cent. of the first kind was arrested by the roughness of the surface, only 15 per cent. of dark heat was stopped.

29. (3.) With regard to the third question, the action of a comparatively small number of scratches on a polished surface, instead of a general diminution of its polish, I proceeded thus:—I caused a series of extremely minute lines to be drawn mechanically with a diamond point, on a well-polished surface of rock-salt, so as to divide it into squares having *one-hundredth* of an inch for their side. A similar plate was scored by fine lines in the same manner, parallel to one another, and *one two-hundredth* of an inch apart. A portion of this second plate was crossed rectangularly, by lines drawn at the same distance, so as to divide the surface into squares four times smaller than in the first instance. These three media gave the following results with two very different kinds of heat (December 6–11, 1839):—

Source of Heat.	Scored in squares 100 lines to the inch.	Scored in lines 200 to the inch.	Scored in squares 200 lines to the inch.
Locatelli, with glass	76·5	61·5	45
Dark hot brass	82·3	68·5	64·5 *

* The part of the second plate which was scored *across* being more free from flaws than that which was *once* scored, explains the little difference between this number and that in the preceding column.

For heat of 212° the per-centage was still higher, as will afterwards be shown.

30. *Metallic Gratings.*—If the mere defect of transparency were the cause of the peculiar action of scratched surfaces, we might expect that any opake filaments would act in the same way. Could we dispense with the *medium* altogether, and employ a *screen*, which should have the qualities which we had artificially given to the physical surface of the medium, we should evidently have advanced a step in the interpretation of the phænomena. The action of grooved surfaces and gratings upon light suggested so forcible an analogy, that before I was able to procure the mechanically striated surfaces, described in the last article, I had employed fine metallic wire-gauze as a diffraction-screen, hoping to obtain results similar to those which I anticipated, and afterwards did obtain, by drawing fine lines upon rock-salt.

31. The fact that diffraction-phænomena in light, produced by gratings, are wholly irrespective of the nature of these gratings, as, for instance, whether they be formed of metal-wires, or mere lines drawn through a soapy film stretched on glass, gave some countenance to this experiment. I was not unaware that diffraction spectra are produced, not by a parallel beam of light, but by a picture, formed of a distant luminous point. Still, though the ground or field illuminated by parallel rays passing through a grating must evidently have a uniform tint, it does not appear absurd to suppose that that tint may be different from white. Nor does this question appear to have occurred to mathematicians or optical writers, until the problem presented itself to me in the course of this investigation.

32. With such wire-gauze as I could easily procure, I failed in obtaining any peculiarity of action as relates to heat from different sources; and further, the quantity of heat intercepted by the metallic grating appeared to be nearly, or exactly, proportional to the surface of the opake portion of the screen. Thinking that perhaps finer gauze than that I used (60 wires to the inch) might produce the desired effect, I obtained, through the kind assistance of Sir John Robison and M. Leonor Fresnel, the finest manufactured in Paris, going as high as about 160 per inch. In general my first results were confirmed, viz. (1.) that the proportion of heat stopped is irrespective of the source; (2.) that it is to the incident heat as the area of the wires is to the area of the surface. It must be observed, however, that the determination of this latter proportion with extreme accuracy by an examination of the grating, is not so easy as might at first sight appear. When the

wire is fine compared to the *interstices*; the interstices are pretty nearly rectangular and equal-sided. But this is not the case in most manufactured wire-gauze. One set of wires is nearly parallel and straight, but not so the set interlaced with the former, which do not generally make their intersections at right angles, and hence, universally, the interstices are somewhat smaller than a calculation proceeding upon the number of wires per inch, and their diameter would give. Distrusting my own observations, I put three specimens of wire-gauze into Mr. John Adie's hands, requesting him to determine the mean diameters and intervals of the wires. With a very accurate micrometer he determined 14 values for each of these quantities in both directions. From these data the proportion of the interstices to the whole surface of each grating is easily calculated, and the results are given below for three sorts of gauze of which I had *previously* determined the permeability for heat.

Micrometric Measurement of Wire-Gauze. Unit of Measure = $\frac{1}{32400}$ inch.

Wire-Gauze.	Lengthwise.		Breadthwise.		Ratio of Interstices to Surface.
	Interstice.	Wire.	Interstice.	Wire.	
No. 1. (57 per inch)	534	371	562	384	·3504
No. 2. (92 per inch)	375·6	179·4	402·6	179·6	·4680
No. 3. (129 per inch)	200	159	284	168	·3500

33. The numbers in the last column (computed on the supposition of the interstices being geometrical rectangles) are to be compared with the following experimental determinations of the proportion of incident heat transmitted by these gratings.

Proportions per 100 of Incident Rays of Heat transmitted by Wire-Gauze.

Wire-Gauze.	Locatelli, with Glass.	Locatelli.	Dark Hot Brass.	Hot Water.
No. 1. (57)	32·5	32	33·5	
No. 2. (92)	46·0*	...	44·7 †	
No. 3. (129) ...	30·5	...	30	29·7

* Two such gratings superimposed, so that the wires formed angles of 45° with one another, gave for the per-centage of transmission 20·7. The square root of this, or effect due to each grating, is 45·5, or almost the same as the number in the text.

† Two superimposed gratings gave 21·2 per cent., or 46 for each system separately.

The differences for each grating, perhaps, do not exceed the errors of experiment. In every case these numbers are *inferior* to the geometrical interstices, but what inclines me to think that this difference is due to the irregularities of figure of the gauze (including the effect of *flattening* of the wires where they overlap, making the interstices obtuse-angled) is this: that No. 2, in which the wires were finer compared to the interstices than in others (the total interstices being one-third part larger in proportion), and the gauze evidently far more regularly formed than in the other cases, the per-centage transmitted differs very little from the geometrical gauge. I own, at the same time, that a difference of 5 per cent. in No. 3 (which is evidently not due to an error of observation), seems to me barely accounted for by this remark.

34. *Thread-Gratings.*—With gratings of fine cotton-threads $\frac{1}{100}$dth inch apart, used for showing Fraunhofer's Spectra, I obtained a similar result. These threads were arranged parallel-wise on two frames, capable of being superimposed rectangularly. Thus, we can either employ a screen of parallel threads *one-hundredth* of an inch apart, or a screen of mathematically accurate squares, formed by superposition. It is difficult in this case, however, to obtain the diameter of the thread accurately enough to estimate the ratio of interstices.

Per-centage of Incident Heat transmitted by Cotton-Thread Gratings, $\frac{1}{100}$dth inch apart.

	Locatelli, with Glass.	Dark Heat.
Thread Grating, Single	29·5	30·2
——————— Double	9·0 *	8·3 †

* Corresponding *single* action = 30·0 per cent.
† ———————— = 28·8 ————

The difference here seems imperceptible, the differences, such as they are, being in opposite directions. The results in the last column are from single experiments (November 28th, 1839).

35. *Action of Powders.*—Adhering to the idea (12.) that the action of a smoked surface was due to the mechanical action of a number of minute opake points distributed over a transparent body, it occurred to me almost at the commencement of these experiments, to try the effect of powders artificially sifted on such a surface. Any ingredient, however, which could make the powders adhere to the surface, would hae vitiated the experiment, by introducing its own

proper diathermancy. I therefore included the powders between two polished plates of rock-salt, closed at the edges with wax. The preliminary experiment (27.), to show that the salt surfaces, in the state in which I commonly employed them, exercise no perceptible influence on the quality of the transmitted heat, was evidently a very important one for the conclusions I meant to draw. It was, as I have stated, quite satisfactory.

36. The first experiments which I made with powders (December 6, 1839), were with chalk and alum, finely dusted between two plates of salt. I selected the chalk on account of its absolutely uncrystalline and opake character; and alum, because its power of stopping rays of heat of low temperature was so very great, that I judged that if the influence as a *mechanical modifier of surface* should prove predominant, and allow as much, or more, heat of *low* than of *high* temperature to pass, the *mechanical* influence of a substance in fine powder would be clearly established.

37. Now, the result at which I arrived, and which was entirely conformable to my anticipation, may serve to show the caution requisite in drawing conclusions from limited data, however apparently conclusive. The surfaces powdered with chalk suffered *rather more* heat of low than of high temperature to pass (viz. 34·5 per cent. dark heat, and only 30·5 of heat from Locatelli lamp, transmitted through a thick glass-lens), whilst the salt strewed with alum appeared quite indifferent to the kind of heat incident* (transmitting only 17 per cent. of both, thus showing that the powder was in considerable quantity). I concluded, therefore, with apparent reason, that the chalk having no *specific* action, or being (most probably) opake or *athermanous*, the powder of it acting mechanically, allowed low temperature heat to pass in excess, whilst in the case of alum, the *specific* action was entirely counteracted by the *mechanical* action of the powder. I simply stated the fact amongst others detailed in the preceding pages, in a memorandum presented to the Royal Society of Edinburgh on the 16th December 1839†, and a few days after, in a slightly different form, communicated to M. Arago, and printed in the *Comptes Rendus de l'Académie des Sciences*, 6th January 1840. On the 28th of December, I obtained a similar result for charcoal powder (whose affinity with smoke

---

* Yet an alum plate of a certain thickness transmits no less than 27 per cent. of the one kind of heat, and *no sensible portion* of the other (Melloni.)

† See our July Number, p. 69. note *.

suggested its use), and yet it does not appear that the general conclusion which I intended is entirely warranted.

37. It is well known that Sir Isaac Newton overlooked the variable dispersive power of bodies for light, in consequence of having compared two, in which the dispersion *happened* to be proportional to the mean refraction. A similar haste to generalize would have led to error on the present occasion, had not a simultaneous investigation led me to re-consider the subject of powders. Whilst waiting for the arrival of fine wire-gauze from Paris, it occurred to me to try the effect of metals in a state of extreme division. It seemed, however, first desirable to ascertain whether the metals are as incapable of transmitting heat as is commonly supposed.

38. For this purpose, I stretched a piece of the thinnest gold-leaf across a wide diaphragm of pasteboard, and suffered an intense parallel beam of heat from Locatelli's lamp to fall directly upon the pile. A screen of glass was interposed, which, by experiment, was known to stop 43 per cent. of this sort of heat. The needle of the galvanometer deviated 31°·2, the glass being interposed; the equivalent direct effect would have been $31{\cdot}2 \times \frac{100}{43} = 72°$. When the glass was removed and the gold-leaf put in its place, on the brass screen being alternately introduced and removed, not the faintest motion was perceptible in the needle; had it amounted to $\frac{1}{20}$th of a degree, that is, had $\frac{1}{1400}$dth of the incident heat been transmitted by the gold-leaf, I considered that the effect would have been perceptible. Yet this gold-leaf was so thin that the features of a landscape could be distinctly seen through it, of the usual bluish-green tint. No more convincing proof certainly can be desired, that conduction plays no sensible part in these experiments, since it did not sensibly act on a film of one of the best-known conductors of heat, and perhaps not more than $\frac{1}{300000}$dth of an inch thick. I thought it worth while to repeat the experiment with dark heat, and with the same results. The analogy of the action of split mica on light to metallic reflexion led me to suspect, that if any kind of heat were transmitted by metallic leaves, it would be that of low temperature.

39. The imperviousness to heat of gold-leaf, the thinnest continuous film of metal which we can obtain, satisfied me of the importance of obtaining the metals in a condition to verify my experiments with the powder of other substances. When the hope diminished of obtaining wire-gauze of a degree of fineness (I mean fineness in the *wire*, not closeness of texture, for that was comparatively immaterial) which might

vie with the diamond scratches on the salt surface, which presented, under the microscope, an irregular furrow, probably nearly $\frac{1}{2000}$dth of an inch in mean breadth,—I recurred to the project of using the metals in *powder*. It was evident, from the experiments on depolished and scored surfaces, that the *irregularity* of these streaks had nothing whatever to do with the phænomenon of checking rays of high refrangibility and admitting others. Sand-paper scratches, than which nothing can be more irregular, produced the effect, and that more intensely as the surface became more coarsely and closely furrowed. Nay, it occurs in natural tarnish, where there can be no linear arrangement of the points affected. It seemed to me, therefore, that a surface covered with a metallic powder, presented the *limit* of a grating where the interstices were not required to have any regular form.

40. The next difficulty was to obtain impalpable powders indubitably metallic, to which I attached very considerable importance, for it was quite conceivable that the metallic sulphurets and other substances employed for the fictitious metallic powders called gold, silver, and copper bronzes, might have specific diathermancies which might injure the experiment. I at length succeeded in obtaining silver by precipitation, and copper from Daniell's battery; and with some difficulty I procured from a large manufacturer coinage silver and gold, reduced by mechanical trituration to a perfectly impalpable and beautifully metallic powder. These expensive preparations are now wholly superseded by the admirable fictitious bronzes in use in the arts. These, together with metallic copper-bronze, perfectly impalpable, furnished by the same individual, and a much coarser tin powder used by druggists, formed the material of a very careful series of experiments, which I extended over a very considerable period, and varied in a great many ways. (1840, Jan. 28, &c.)

41. The following table contains the results of my experiments on metallic powders, which (with the exception of tin) may be considered as perfectly impalpable, adhering to the dry finger, and *undoubtedly metallic.*

Per-

Per-centage of Heat from different Sources transmitted by Metallic Powders.

Powder.	Locatelli Lamp.			Dark Hot Brass.	Hot Water.	Remarks.
	Glass interposed.	Direct.	Smoked Salt interposed.			
Gold, No. 1. (adhering to a single surface of salt)	58	...	50·5			
Gold, No. 2. (between two salt plates)	7·4*	...	...	4·1*		
Silver, No. 1. (between two salt plates)	25·3	24·2	...	21·8	...	Mean of a considerable number of results.
Silver, No. 2. (adhering to a single surface of salt)						
1st Series	27·7	...	18·5	...	...	The same plate was used, but differently placed in respect of the pile, so that each series stands by itself.
2nd Series	29·5	...	22·1	...	25	
Copper, No. 1, (between two salt plates)						
1st Series......	14·8	...	16·0			
2nd Series	17·4	...	...	18·7	17	
Copper, No. 2. (between two salt plates)	5·6*	...	...	4·05*		
Tin (between two salt plates)	27·0	26·0	...	25·5	...	Mean of a considerable number of results.

42. These observations are confessedly very imperfect. I am persuaded, however, that the apparent anomalies are not errors of observation; other instances will presently occur. With a view to determine the quality of thickly strewed surfaces yielding a very feeble per-centage of transmitted heat, it was desirable to use an intense incident beam. In order, however, to keep the comparison within the range of galvanometer degrees, whose numerical values have been tested (Second Series, arts. 7–8), the observations in the preceding table marked thus * were made in the following manner. The *direct* effect of the incident heat on the pile was never observed, but only that part of it which penetrated the wire-gauze, No. 3 of art. 33, which transmits almost exactly 30 per cent. of every kind of heat. The direct effect was estimated at $\frac{100}{30}$ of the degrees of deviation corresponding to this transmission, and then the wire-gauze being removed, and the medium to be examined substituted, the effect was compared to the computed direct effect. For example, with the copper powder, No. 2, the effect of the Lo-

* See the next article (42.)

catelli lamp, heat transmitted through thick plate-glass, and then modified by wire-gauze, was .... 22°·57

Direct effect $= 22{\cdot}57 \times \frac{10}{3}$ .... 75 ·2

Wire-gauze removed, and copper substituted ...... 4 ·15
Ratio to direct 5·52 : 100.

In this way per-centages may be obtained with very nice accuracy: another experiment gave in the same case 5·60 : 100.

43. The table in art. 41. demonstrates to my conviction (strengthened by a careful examination of the very consistent observations on which it is founded), (1.) That gold, silver, and tin powders, instead of having the property which I was disposed to assign to opake powders generally, do really transmit more heat of high than of low temperature; that is, act like glass, alum, and other transparent media in their common state. (2.) With respect to copper, two series give one result, and a third the opposite. Yet all of these were made with great care, and contain internal evidence of their accuracy. I am confident that the differences are not due to errors of observation; and I have observed other cases, in which an increase of thickness of the obstructing medium, and an increased intensity of the incident heat, gave altered results as to permeability, a result by no means paradoxical, since intense heat may be sensibly transmitted through a nearly opake substance, and thence acquire a new character, which a feebler beam, transmitted through a less obstructing medium, would not possess. At all events, I can offer no further explanation at present. That copper possesses a peculiar character, distinct from the other metals which I tried, I am fully persuaded.

44. The evidence which the experiments on metallic powders gave of the inadequacy of the *mere powdery form* to produce the effect of smoke, forced me to a more critical examination of other bodies in a similar state.

45. I repeated my experiments with increased care on the powders already employed. I tried a great number of new ones, chosen amongst substances differing as widely in nature as possible. Some of these substances were repeatedly tried in different specimens, the powder more or less thickly strewed, and at different times.

46. One circumstance in particular raised a doubt as to the result of my former conclusion, where it seemed most incontrovertible. I had argued, that if alum in powder arrested equally all kinds of heat, the *mechanical* action of the powder must have opposed and destroyed the *specific* action of the alum (36.). I was gradually, however, led to admit, that, *in the state of powder, most diathermanous bodies are almost*

equally opake, or, rather perhaps, I should say, equally indifferent to the *kind* of incident heat (*i. e.* colourless in optics).

47. So far as the eye could judge of the proportion of obstacles in surfaces strewed with different kinds of powders, there did not seem any very marked peculiarity in their transparency for heat. A surface dusted with alum or citric acid appeared to transmit nearly as much as one strewed with powdered rock-salt. Nor could this arise merely from the *minute thickness* of the substance, which is well known to produce in heat, as in light, an approximation to a colourless character; for the proportion stopped by the powder was always a large fraction (usually from $\frac{3}{4}$ths to $\frac{9}{10}$ths) of the incident heat. The opacity, then, is the result of the innumerable reflexions and interferences which *scatter* and *stifle* the transmitted heat; and this is almost equally effectually done, whatever be the nature of the substance. On reflection, therefore, this general result does not appear surprising. I will quote one experiment, in particular, in illustration of it.

48. When I was at a loss to procure fine metallic fibres, I thought of employing a diaphragm irregularly covered with fine threads of spun glass, with a view (just as in the case of the alum powder) of ascertaining how far the mechanical condition of the glass might modify its well-known qualities with respect to the transmission of heat. When Locatelli lamp-heat, having been transmitted by *thick plate-glass*, fell upon the spun-glass fibres, forming an irregularly reticulated diaphragm, no more than 47·5 per cent. of the incident heat was transmitted. Now, we know perfectly from the experiments of De la Roche and Melloni, that, after passing through such a thickness of plate glass, an additional film, the thickness of the glass fibres used would produce *no sensible resistance* to the further passage of the heat, excepting only its superficial reflexion. The loss of 52·5 per cent. of the heat was therefore due to the *scattering* and *stifling* of heat by reflexion at the surfaces of the fibres, refraction through their cylindric surfaces, and interference. We cannot, therefore, be surprised, if the refracted part of the heat reaching the pile (the only portion very materially affected by the nature of the medium) should not greatly alter the quantity of different sorts of heat indicated by the galvanometer. Accordingly, we find, that heat from a dark surface of brass warmed by an alcohol lamp, had 44 per cent. transmitted under the same circumstances; and even hot water had 42 per cent., although a small thickness of glass is sensibly opake for that kind of heat.

49. If this be the case,—if the differences be so trifling—for a reticulation of regularly-formed, transparent, and polished threads of glass, much more must it hold with impalpable crystalline or other powders, presenting (no doubt) mi-

nute surfaces at every angle, and minute fissures in every direction.

50. The following table contains the results of a large number of experiments on powders of various kinds, many of them repeated under various circumstances. The investigation is, as in the case of the metallic powders, confessedly imperfect; but since the broad simple principle which I at first tried to establish respecting the diathermanous quality of opake powders does not appear to hold universally, I stopped this series of experiments, which were troublesome and laborious, after establishing a few general facts, which I will presently lay down, without attempting to exhaust a subject, of which, by and by, we shall no doubt know more, but which at present it would be perhaps a waste of time to pursue into its insulated details. These powders were in all cases dusted between polished salt-plates, united at the edges, and then attached to diaphragms of card, so arranged as to transmit the heat in every case through the same parts of the surface.

Per-centage of Transmission of Heat, from different sources, through Non-metallic* Powders.

Powder of	Source of Heat.			
	Locatelli Lamp		Dark Hot Brass.	Hot Water.
	Through Glass.	Through Smoked Salt.		
Alum, No. 1.	17·0	...	17·1	...
—— No. 2.	15·2*	...	13·0*	...
Citric acid, No. 1	29	30	33 †	31·5
—— No. 2	12·9*	...	8·7*	...
Rock-salt, No. 1.	12·8* 13·4	11·8	11·3	...
—— No. 2.	31·5 ‡	...	29·2‡	...
Sulphur	50·0	...	44·7	...
Red-lead	30·2	...	34·0	...
Galena	26·3	22·4	...	...
Charcoal A.	5‖	...	9 ‖	...
—— No. 1. 1st Series §	11·4	13·9	...	...
—— 2nd Series §	15·1	...	16·0	17
—— No. 2	3·2*	...	3·5*	...
Chalk, No. 1	30·5	...	34·5	...
—— No. 2	15·5* 15·6	18·4	17·9	...
—— No. 3	27·5	...	32·0	...
Carbonate of magnesia	8·3	12·6	...	...

* The observations marked thus were made with a powerful beam of heat in the way described in art. 42. † Not directly comparable with the other two observations on the same line, and probably 3 or 4 per cent. too high. ‡ Extremely good observations.

‖ The intensities very feeble. § The circumstances in these two series varied, so as to make the one not directly comparable with the other; but each is perfectly good.

---

* By *non-metallic* is meant, not in the state of a *pure* or uncombined metal.

51. On the preceding table, I would observe, (1.) That the pulverized crystalline bodies, such as rock-salt, alum, citric acid, and sulphur, exhibit no decided tendency to transmit an excess of heat of low temperature, depending on their powdery form. The carefully repeated experiment with rock-salt is, on this point, very conclusive, since its indifference as a substance to the quality of the heat which it transmits would at once leave the effect, if any, due to mechanical condition, apparent. It even very evidently appears in this state to transmit *less* freely heat of low than heat of high temperature. (2.) Galena, the crystallized sulphuret of lead, in fine powder, appears to possess the qualities of gold, silver, and tin (43.). (3.) Red lead, charcoal, chalk and magnesia, all substances in an opake *earthy* condition, appear certainly to transmit an excess of *dark heat.* I think it probable that this list might be extended to most bodies having a similar mechanical constitution.

53. These distinctions, I am well aware, leave the *causes* of the difference of character of powders, and the peculiarities of tarnished surfaces, nearly in the same obscurity as before. In particular, I cannot but regard it as being singular, that a surface covered with powdered salt has no analogy, but even opposite properties, to one of the same material mechanically furrowed*. The contrariety of action of metallic powders to those of opake earths, is as singular as it was unexpected. I have already stated, however, my doubt whether a complete investigation of the peculiarities of specific substances would, *at present*, reward the necessary labour. I have made experiments on a few fibrous substances, as paper and membrane, which I thought might very probably act as tarnished surfaces do. There is an approximation to this, as will be seen, in the common cambric or tissue paper. In the kind of tracing paper employed (which is made in Paris, I believe, under the name of *papier végétal*), there is evidently some foreign matter introduced to produce the transparency, which modifies the transmission. A close reticulation of cotton fibres has already been shown to exercise no specific action (34.). The following table contains a few results not included in preceding ones, and illustrating in several substances the quality of *heat-colour*, which in this paper we have been considering.

---

* To put this in the most clear point of view, I used and compared two such plates in the same experiment.

Per-centage of Heat transmitted by several Bodies.

Substance.	Source of Heat.		
	Locatelli Lamp, with Glass.	Dark Hot Brass.	Hot Water.
Gold-beater's skin	60	28	...
Cambric or tissue paper	8·6	10·5	...
Tracing paper (*Papier végétal*)	36	28	...
Fibres of spun glass	47·5	44	42
Smoked salt	30·2	58	67
Roughened salt	49	73	76
Polished salt, scored into 200 × 200 squares per inch	49·5	73	77

54. The leading facts contained in this paper are these :—

55. I. The peculiar (*red-like*) character of films of smoke in transmitting heat of low temperature is partaken,—

A. By simple powder of charcoal.

B. By (at least some) other dull earthy powders.

C. By surfaces simply *dull* or devoid of polish.

D. By surfaces irregularly furrowed, as with emery or sand-paper.

F. By polished surfaces, on which fine distinct lines have been drawn.

F. By the mechanical lamination of transparent mica, which, as a continuous medium, possesses opposite properties.

56. II. The following media seem *indifferent* to the kind of heat which they transmit :—

A. The thinnest gold-leaf is impervious to any.

B. Metallic gratings transmit all kinds of heat in a proportion which is probably exactly as the area of the interstices which they present.

C. Thread-gratings.

D. In a state of powder, most crystalline bodies *approach* to a condition of opacity for heat.

57. III. The following bodies, in addition to those commonly known, transmit most heat of high temperature (*violet-like heat*).

A. Several pure metallic powders.

B. Rock-salt in powder; and many other powders.

C. Animal membrane.

58. IV. Heat of low temperature is most regularly reflected at imperfectly polished surfaces. It is also, we have seen, most regularly transmitted. These facts are of great importance to the theory of heat; and may probably sug-

gest inquiries of no small interest with regard to light, and especially the phænomena of absorption.

59. We have already (24.) noticed the analogy which the fact stated in the last article bears to the easier reflexion of red than violet light from depolished surfaces, and in that fact we find a confirmation of the application of the undulatory doctrine to heat, and of the opinion that the waves producing heat are longer in proportion as the temperature of the source is less. The phænomena of transmission are more obscure; they may be compared either to the diffraction, or to the absorption, of light.

60. The action of lines on polished surfaces, similar to those used in many diffraction experiments, led to the inquiry (31.) whether the mean colour of light transmitted by gratings was necessarily unchanged? The question does not seem to have occurred to any one to whom I have mentioned it; and though the most likely result would seem to be, that there should be no change, the grounds of such an *à priori* opinion do not appear absolutely conclusive. Professor Kelland, however, has, I believe, first succeeded in integrating the expression for the illumination of a screen placed behind a grating of any kind (see Airy's Mathematical Tracts, page 328) on which a plane wave falls, and he informs me, that in every case where the breadth of the interstices is any multiple of the breadth of the wires or opake spaces, the intensity is the same as if there were a diaphragm equal in size to the sum of the interstices of the grating.

61. This result (which seems quite sufficiently general for our purpose) is so far confirmed by the absolute *indifference* of metallic gratings to the quality of the incident heat.

62. It remains, however, to be explained how furrowed surfaces can act, except by intercepting, as an opake network would do, a part of the heat. I cannot give an explanation which appears full and satisfactory, but the condition of mica split into thin laminæ by heat, and producing the same effect, may serve to guide us, perhaps, to something like the true cause.

63. A number of thin plates, of *exactly uniform* thickness, would transmit a certain colour, and reflect the complementary one. If there be a great preponderance of plates approximating to a certain thickness, and if the disproportion of the lengths of the incident waves be great, a large proportion will be in like manner transmitted, and the remainder stifled or reflected. If this effect is not so frequently observed in bodies mechanically separated into films as we might expect, this is owing to the small range of length of wave in the visible parts

of the spectrum. A small variation in the thickness of the film transmits or annihilates by interference each colour of the spectrum in succession. If the waves of heat be much more heterogeneous (as I have already surmised) than those of light, such effects would be proportionably more sensible.

64. Possibly a grooved surface may be considered as presenting a number of polished surfaces, partially detached from the general surface, under small obliquities to the incident rays; and we may suppose that these rays, after separation by partial reflexion and refraction, reunite with unequal retardations, producing *first* a destructive effect upon the shorter waves, and suffering the others to persevere. I have already adverted to the fact, that most turbid fluids transmit chiefly the longer luminous waves. I offer these, however, but as vague conjectures upon a very obscure subject. I think that experiments on the colour of media, such as those we have employed, and especially of depolished plates, might not be without value in illustrating the phænomena of absorption in optics.

65. In conclusion, it might perhaps be expected that I should take some notice of the experiments and reasonings of which M. Melloni has addressed an account to M. Arago, in two letters dated the 4th and 14th of March last, and published in the *Comptes Rendus* for the 30th of the same month. These letters were occasioned by the announcement of my Researches, in the same work, for the 6th of January. The present paper, founded *solely* upon experiments undertaken and completed before the despatch of the earliest of M. Melloni's communications, will, I think, sufficiently answer all the questions which are started in his letters to M. Arago, at least all those in which my experiments are concerned.

May 12, 1840.

XXII. *On a new Theorem in the Calculus of Finite Differences, with its Application to the Development of the Cosine of a Multiple Arc in descending Powers of the Cosine of the Simple Arc. By* James Booth, *M.A., Principal of, and Professor of Mathematics in Bristol College*[*].

THERE is perhaps no problem in pure mathematics which has given rise to more discussion than the development of the cosine of a multiple arc in descending powers of the cosine of the simple arc. While Euler and Lagrange, on the one hand, have evolved coincident results by methods very different, so Poinsot, on the other, having shown that the methods pursued were fallacious, and the results themselves false, has given the true development by a method complex in the extreme, founded on the principles of a higher analysis than is generally introduced into such investigations.

---

[*] Communicated by the Author.

It appears somewhat strange, that even since the correct series has been given, the erroneous developments should have been retained in some recent publications on the subject.

In the following pages the method pursued will be found different from those generally adopted in the investigation, no principles being assumed beyond such as may be deduced from elementary algebra; while the method of determining the coefficients of the expansion, by the aid of the fundamental theorem first established, will, it is hoped, be considered simple.

Let $a, b, c, d \dots l$ be any m numbers, positive or negative, integral or fractional, or even imaginary, m being less than the integer n, and h being any finite number whatsoever, then we shall have the following theorem:—

$$\left.\begin{array}{l} 0 = \{a, b, c, d \dots l\} - n\,\{(a-h)(b-h)(c-h)\dots\} \\ + \dfrac{n \,.\, n-1}{1\,2}\{(a-2h)(b-2h)(c-2h)\dots\} - \dfrac{n n-1 \,.\, n-2}{1\,2\,3} \\ \quad \{(a-3h)(b-3h)(c-3h)\dots\dots\} \text{ \&c. \&c.} \end{array}\right\} \text{(I.)}$$

or, separating the symbols of operation from those of quantity, we may write the theorem thus:—

$$0 = (1-1)^n \; \mathrm{I}_0^n \left[(a - i\,h)(b - i\,h)(c - i\,h)\dots(l - i\,h)\right],$$

the symbol I_0^n denoting the sum of all the values which the quantity within the brackets takes, when i is made to assume every integral value from 0 to n inclusive.

This theorem may be proved as follows: Let the sum of the series (I.) be $= \mathrm{V}$, assume the arbitrary quantity k a multiple of h, and let $\alpha, \beta, \gamma, \delta \dots \lambda$ be the differences of the m numbers $a, b, c, d \dots l$ and k, so that $a = k - \alpha$, $b = k - \beta$, $c = k - \gamma$, $d = k - \delta, \dots l = k - \lambda$, then we shall have the equivalent expressions

$$\{a, b, c, d \dots l\} = k^m + \mathrm{A}\,k^{m-1} + \mathrm{B}\,k^{m-2} \dots\dots \mathrm{M}\,k + \mathrm{N}.$$

In the same way, if $k' = k - h$, we shall have

$$(a-h)(b-h)(c-h)\dots = k'^m + \mathrm{A}\,k'^{m-1} + \mathrm{B}\,k'^{m-2} \dots \mathrm{M}\,k' + \mathrm{N}.$$

Again, let $k'' = k - 2\,h$, and we find

$$(a-2h)(b-2h)(c-2h)\dots\dots = k''^m + \mathrm{A}\,k''^{m-1} + \mathrm{B}\,k''^{m-2} \dots\dots \mathrm{M}\,k'' + \mathrm{N}.$$

$$+ \mathrm{Q}\left\{n^{m-2} - n\,(n-1)^{m-2} + \frac{n\,.\,n-1}{1\;2}\,(n-2)^{m-2}\right.$$

$$\left. - \frac{n\,.\,n-1\,.\,n-2}{1\;2\;3}\,(n-3)^{m-2}\ldots,\ \&c.\right\}$$

……………………

$$+ \mathrm{S}\left\{n^1 - n\,(n-1)^1 + \frac{n\,.\,n-1}{1\;2}\,(n-2)^1\right.$$

$$\left. - \frac{n\,.\,n-1\,.\,n-2}{1\;2\;3}\,(n-3)^1\ldots,\ \&c.\right\}$$

$$+ \mathrm{T}\left\{n^0 - n\,(n-1)^0 + \frac{n\,.\,n-1}{1\;2}\,(n-2)^0\right.$$

$$\left. - \frac{n\,.\,n-1\,.\,n-2}{1\;2\;3}\,(n-3)^0\ldots\ldots\right\}.$$

The quantities between the brackets in the common notation are thus written: $\Delta^n\,0^m$, $\Delta^n\,0^{m-1}$, $\Delta^n\,0^{m-2}$, which are each cypher when n is greater than m; hence $\Delta^n\,x^m = 0$ when $n > m$.

The theorem $\Delta^n\,0^m = 0$, or its development,

$$\left.\begin{array}{l} n^m - n\,(n-1)^m + \dfrac{n\,.\,n-1}{1\;2}\,(n-2)^m \\ \quad - \dfrac{n\,.\,n-1\,.\,n-2}{1\;2\;3}\,(n-3)^m = 0\ \ . \end{array}\right\} \text{(III.)}$$

may be proved by the common principles of algebra as follows:

Assume $x = z - 1$, raising both sides of this equivalence to the nth power, we have

$$x^n = z^n - n\,z^{n-1} + \frac{n\,.\,n-1}{1\;2}\,z^{n-2} - \frac{n\,.\,n-1\,.\,n-2}{1\;2\;3}\,z^{n-3},\ \&c.$$

Let the first derivative of this equivalence be taken, or, in other words, let the equivalence $x = z - 1$ be raised to the $(n-1)$ power, and then multiplied by n, we thus find

$$n\,x^{n-1} = n\,z^{n-1} - n\,(n-1)\,z^{n-2} + \frac{n.n-1.n-2}{1\;2}\,z^{n-3},\ \&c.$$

Multiplying this series by the equivalence $x + 1 = z$, we obtain

$$n\,x^n + n\,x^{n-1} = n\,z^n - n\,(n-1)\,z^{n-1} + \frac{n.n-1}{1\;2}(n-2)\,z^{n-2},\ \&c.$$

Again, taking the first derivative of this series, and multiplying by $x + 1 = z$, there results the equivalence

M, N, and h are finite, all the terms of the above series are separately equal to zero, or

$$V = 0.$$

It is clear that, whatever be the nature of the numbers $a, b, c, .. l$ and h, we may assume the arbitrary number k, so that n may be an integer.

The theorem $\Delta^n (u - n)^m = 0$ may be reduced to another more simple, as follows: let $u = x + n$, then $\Delta^n (u - n)^m = \Delta^n x^m$; but

$$\left.\begin{aligned}\Delta^n x^m = (x + n)^m - n(x + n - 1)^m + \frac{n\,.\,n-1}{1\;2}(x + n - 2)^m \\ -\frac{n\,.\,n-1\,.\,n-2}{1\;2\;3}, \;\&\text{c.} \quad . \; . \; . \; . \; . \; . \; .\end{aligned}\right\}\text{(II)}$$

(*Lacroix.* tom. iii. p. 9.)

developing these powers in the following manner:—

$$(x + n)^m = n^m + \text{P}\,n^{m-1} + \text{Q}\,n^{m-2} \ldots\ldots \text{S}\,n + \text{T}$$

$$(x + n - 1)^m = (n - 1)^m + \text{P}\,(n - 1)^{m-1}$$

$$+ \text{Q}\,(n - 1)^{m-2} \ldots\ldots \text{S}\,(n - 1) + \text{T}.$$

$$\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

P, Q, S, T being functions of x and m. Substituting these values of $(x + n)^m$, $(x + n - 1)^m$, &c. in (II.), we find

$$\Delta^n x^m = n^m + \text{P}\,n^{m-1} + \text{Q}\,n^{m-2} \ldots \text{S}\,n + \text{T}$$

$$- n\,\{(n-1)^m + \text{P}\,(n-1)^{m-1} + \text{Q}\,(n-1)^{m-2} \ldots . \text{S}\,(n-1) + \text{T}$$

$$+ \frac{n\,.\,n-1}{1\;2}\Big\{(n - 2)^m + \text{P}\,(n - 2)^{m-1}$$

$$+ \text{Q}\,(n - 2)^{m-2} \ldots \text{S}\,(n - 2) + \text{T}, \;\&\text{c. }\&\text{c. }\&\text{c.}$$

Now, adding vertically, we obtain

$$\Delta^n x^m = \Big\{n^m - n\,(n - 1)^m + \frac{n\,.\,n-1}{1\;2}(n - 2)^m$$

$$- \frac{n\,.\,n-1\,.\,n-2}{1\;2\;3}(n - 3)^m \ldots, \;\&\text{c.}\Big\}$$

$$+ \text{P}\,\Big\{n^{m-1} - n\,(n - 1)^{m-1} + \frac{n\,.\,n-1}{1\;2}(n - 2)^{m-1}$$

$$- \frac{n\,.\,n-1\,.\,n-2}{1\;2\;3}(n - 3)^{m-1} \ldots, \;\&\text{c.}\Big\}$$

$$+ \mathrm{Q} \left\{ n^{m-2} - n\,(n-1)^{m-2} + \frac{n \,.\, n-1}{1\;2}\,(n-2)^{m-2} - \frac{n \,.\, n-1 \,.\, n-2}{1\;2\;3}\,(n-3)^{m-2} \ldots, \&c. \right\}$$

$$\ldots\ldots\ldots\ldots\ldots\ldots\ldots$$

$$+ \mathrm{S} \left\{ n^1 - n\,(n-1)^1 + \frac{n \,.\, n-1}{1\;2}\,(n-2)^1 - \frac{n \,.\, n-1 \,.\, n-2}{1\;2\;3}\,(n-3)^1 \ldots, \&c. \right\}$$

$$+ \mathrm{T} \left\{ n^0 - n\,(n-1)^0 + \frac{n \,.\, n-1}{1\;2}\,(n-2)^0 - \frac{n \,.\, n-1 \,.\, n-2}{1\;2\;3}\,(n-3)^0 \ldots\ldots \right\}.$$

The quantities between the brackets in the common notation are thus written: $\Delta^n\, 0^m$, $\Delta^n\, 0^{m-1}$, $\Delta^n\, 0^{m-2}$, which are each cypher when n is greater than m; hence $\Delta^n\, x^m = 0$ when $n > m$.

The theorem $\Delta^n\, 0^m = 0$, or its development,

$$\left. \begin{aligned} & n^m - n\,(n-1)^m + \frac{n \,.\, n-1}{1\;2}\,(n-2)^m \\ & - \frac{n \,.\, n-1 \,.\, n-2}{1\;2\;3}\,(n-3)^m = 0 \quad . \end{aligned} \right\} \quad \text{(III.)}$$

may be proved by the common principles of algebra as follows:

Assume $x = z - 1$, raising both sides of this equivalence to the nth power, we have

$$x^n = z^n - n\,z^{n-1} + \frac{n \,.\, n-1}{1\;2}\,z^{n-2} - \frac{n \,.\, n-1 \,.\, n-2}{1\;2\;3}\,z^{n-3}, \;\&c.$$

Let the first derivative of this equivalence be taken, or, in other words, let the equivalence $x = z - 1$ be raised to the $(n-1)$ power, and then multiplied by n, we thus find

$$n\,x^{n-1} = n\,z^{n-1} - n\,(n-1)\,z^{n-2} + \frac{n.n-1.n-2}{1\;2}\,z^{n-3}, \&c.$$

Multiplying this series by the equivalence $x + 1 = z$, we obtain

$$n\,x^n + n\,x^{n-1} = n\,z^n - n\,(n-1)\,z^{n-1} + \frac{n.n-1}{1\;2}\,(n-2)\,z^{n-2}, \;\&c.$$

Again, taking the first derivative of this series, and multiplying by $x + 1 = z$, there results the equivalence

$$n^2 x^n + n(2n-1)x^{n-1} + n.n-1.x^{n-2} = n^2 z^n - n(n-1)^2 z^{n-1} + \frac{n.n-1}{1\,2}(n-2)^2 z^{n-2}.$$

Performing the same operations m times successively, we find

$$\left.\begin{array}{l} A\,x^n + n\,B\,x^{n-1} + n.n-1\,C\,x^{n-2} \ldots + n(n-1)\ldots(n-m+2) \\ P\,x^{n-m+1} + n(n-1)(n-2)\ldots\ldots(n-m+1)\,x^{n-m} \\ = n^m z^n - n(n-1)^m z^{n-1} + \dfrac{n.n-1}{1\,2}(n-2)^m z^{n-2} \\ - \dfrac{n.n-1.n-2}{1\,2\,3}(n-3)^m z^{n-3} \quad . \quad . \quad . \quad . \end{array}\right\}\text{(IV.)}$$

A B C...P being functions of n.

Now let $z = 1$, or $x = 0$, and the first member of the last equation becomes a series of positive powers of 0, multiplied by constant finite coefficients, and therefore each term of the first member is equal to zero. Hence the second member becomes also zero, or

$$0 = n^m - n(n-1)^m + \frac{n.n-1}{1\,2}(n-2)^m, \text{ \&c.}$$

Let $m = n$, then all the terms of the first member of equation (IV.) vanish, as before, except the last, and it becomes $n(n-1)(n-2)\ldots 2.1.0^0$, but $0^0 = 1$. Hence

$$1\,2\,3\,(n-1)\,n = n^n - n(n-1)^n + \frac{n.n-1}{1\,2}(n-2)^n.$$

From these principles it follows that if n be an improper fraction greater than the integer m, that the series (III.) continued to infinity is equal to zero.

In the general theorem (I.) let $a = n-1$, $b = n-2$, $c = n-3 \ldots\ldots l = (n-t+1)$, $h = 1$, and $n = t$, then

$$\left.\begin{array}{l} (n-1)(n-2)\ldots(n-t+1) - t\{(n-2)(n-3)\ldots(n-t)\} \\ + \dfrac{t(t-1)}{1\,2}\{(n-3)(n-4)\ldots(n-t-1)\} -, \text{\&c.} = 0 \end{array}\right\}\text{(V.)}$$

This theorem will be found useful further on.

To develope $x^n + x^{-n}$ *in descending powers of* $(x + x^{-1})$.

Put $x + x^{-1} = z$, and assume the series

$$x^n + x^{-n} = A\,z^n + B\,z^{n-2} + C\,z^{n-4} + D\,z^{n-6} \ldots \text{\&c. \&c.} \quad \text{(VI.)}$$

The following considerations will show that the assumption of the development in this form is correct; for, in the first place, if n is an odd number, and x is changed into $-x$, z will be changed into $-z$; and the first member of (VI.) retains its value, changing its sign. Hence all the powers of z in the second member of (VI.) are odd, or the index n is diminished by even numbers.

Again, when n is an even number, and x is changed into $-x$, z is changed into $-z$; and the first member of (VI.) remaining unaltered, the second can contain only even powers of z, or n is diminished by even numbers.

The development of $x^n + x^{-n}$, in powers of $(x+x^{-1})$ or z, can contain no negative powers of z.

For if it were possible that the development should contain such powers, put $x = \sqrt{-1}$, then $z = 0$, and the first member of (VI.) becomes $+ 2$ when n is divisible by 4, $- 2$ when n is divisible by 2, and not by 4, and zero when n is odd, while the second member becomes infinite, as having positive powers of zero in the denominator.

Hence it follows that when n is even, the coefficient of z^0 is ± 2; for by putting $z = 0$, all the terms of the development vanish, except the one containing z^0, while the first member of the equation (VI.) becomes ± 2.

When n is a fraction $= \frac{p}{q}$, the development (VI.) *becomes impossible.* To show this, put $x^{\frac{1}{q}} = y$, $x^{-\frac{1}{q}} = y^{-1}$; then putting $y + y^{-1} = u$, $x^{\frac{p}{q}} + x^{-\frac{p}{q}} = y^p + y^{-p}$

$$= \mathrm{P}u^p + \mathrm{Q}u^{p-2} + \mathrm{R}u^{p-4}, \text{ \&c.}$$

by (VI.), since p is a positive integer number, and P, Q, R are functions of p, as will be shown further on.

We must now substitute for the powers of u, in the last development, their values in terms of z,

$$\text{now } z = y^q + y^{-q}.$$

Hence

$$y^q = \frac{z + \sqrt{z^2 - 4}}{2}, \quad y^{-q} = \frac{z - \sqrt{z^2 - 4}}{2},$$

or

$$u = y + y^{-1} = \frac{z^{\frac{1}{q}}}{2^{\frac{1}{q}}} \left[\left(1 + \sqrt{1 - \frac{4}{z^2}}\right)^{\frac{1}{q}} \right.$$

$$\left. + \left(1 - \sqrt{1 - \frac{4}{z^2}}\right)^{\frac{1}{q}} \right];$$

or developing

$$u = z^{\frac{1}{q}}\{S + Tz^{-2} + Uz^{-4} \ldots \&c.\}.$$

Hence the development of $x^{\frac{p}{q}} + x^{-\frac{p}{q}}$ in powers of $(x + x^{-1})$ or z, will necessarily contain negative powers of z, which has been just shown to be impossible.

To determine the coefficients A, B, C, &c. in the development (VI.).

As $z = x + x^{-1}$, we find by the binomial theorem,

$$\begin{aligned}
z^n &= x^n + n\,x^{n-2} + \frac{n\,.\,n-1}{1\,2}x^{n-4} + \frac{n\,.\,n-1\,.\,n-2}{1\,2\,3}x^{n-6}, \&c.\\
z^{n-2} &= x^{n-2} + (n-2)\,x^{n-4} + \frac{(n-2)\,(n-3)}{1\,2}x^{n-6}, \&c.\\
z^{n-4} &= \phantom{x^n + n\,x^{n-2} + (n-2)\,}x^{n-4} + (n-4)\,x^{n-6}, \&c.\\
z^{n-6} &= \phantom{x^n + n\,x^{n-2} + (n-2)\,x^{n-4} + (n-4)\,}x^{n-6}, \&c.
\end{aligned}$$

Substituting for the powers of z in (VI.) their developments here given, and bringing over the terms on the left-hand side of the equation, we get

$$\begin{aligned}
0 = A&\left\{x^n + n\,x^{n-2} + \frac{n\,.\,n-1}{1\,2}x^{n-4} + \frac{n.n-1\,.\,n-2}{1\,2\,3}x^{n-6}\ldots\right\}\\
&- x^n + B\left\{x^{n-2} + \frac{n-2}{1}x^{n-4} + \frac{n-2\;n-3}{1\,2}x^{n-6}\ldots\right\}\\
&\qquad + C\left\{x^{n-4} + \frac{n-4}{1}x^{n-6}\ldots\right\}\\
&\qquad + \;.\;.\;.\; D\left\{x^{n-6}\ldots\right\}.
\end{aligned}$$

Now the coefficients of the powers of x in this development are separately equal to cypher; and it is easy to show that the coefficient of x^{n-2t} or of the general term, in this development, is

$$\frac{A\,.\,n\,.\,n-1\,.\,n-2\ldots(n-t+1)}{1\,2\,3\ldots t} + \frac{B\,.\,n-2\,.\,n-3\ldots(n-t}{1\,2\,3\ldots(t-1)}$$

$$+\frac{C\,.\,n-4\,.\,n-5\ldots(n-t-1)}{1\,2\,3\ldots(t-2)} + \frac{D.n-6\ldots(n-t-2)}{1\,2\ldots(t-3)}\;\&c.$$

But this coefficient of x^{n-2t} may be thus written,

$$\left.\begin{array}{l} 0=\dfrac{n}{1\,.\,2\ldots t}\Big\{\mathrm{A}\,.\,(n-1)\,(n-2)\,.\,.\,(n-t+1) \\ \quad+\dfrac{\mathrm{B}\,t}{n}.(n-2)(n-3)\ldots(n-t)+\dfrac{\mathrm{C}\,t\,(t-1)}{n\,.\,n-3}(n-3) \\ \quad(n-4)\ldots(n-t-1)+\dfrac{\mathrm{D}\,t\,(t-1)\,(t-2)}{n\,.\,n-4\,.\,n-5}(n-4)\ldots \\ \quad(n-t-2)\text{ \&c. \&c.}\Big\}\;.\;.\;.\;.\;.\;.\;.\;.\;.\;. \end{array}\right\}\text{ (VII.)}$$

Now the part of this series within the brackets is manifestly the same as (V.); comparing then the two series, term by term, we find

$$\mathrm{A}=1,\quad \mathrm{B}=-n,\qquad \mathrm{C}=\frac{n\,.\,n-3}{1\;2},$$

$$\mathrm{D}=-\frac{n\,.\,n-4\,.\,n-5}{1\;2\;3},\quad \mathrm{E}=\frac{n\,.\,n-5\,.\,n-6\,.\,n-7}{1\;2\;3\;4},\ \text{\&c. \&c.}$$

and the coefficient of z^{n-2t} is

$$\pm\frac{n\,(n-t-1)\;(n-t-2)\ldots(n-2\,t+1)}{1\;2\;3\ldots\ldots t}\;.\quad.\ \text{(VIII.)}$$

When $2\,t=n$, the coefficient of z^0 becomes $\pm\,2$, as was shown above.

When n is odd, the coefficient of z is found by putting $2\,t=n-1$ in the general formula (VIII.), when it becomes $\pm\,n$.

We have now shown that

$$x^n+x^{-n}=z^n-n\,z^{n-2}+\frac{n\,.\,n-3}{1\,.\,2}z^{n-4}\ldots\text{\&c.}\quad.\quad\text{(IX.)}$$

or, as it may be more compendiously written,

$$x^n+x^{-n}=z^n\left[1\pm\mathrm{I}_1^{\frac{n}{2}}\left\{\frac{n.(n-t-1)\,(n-t-2)\ldots(n-2\,t+1)}{1\;2\;3\ldots t\;z^{2t}}\right\}\right]$$

Attributing to t all integer values from unity up to $\frac{n}{2}$ inclusive.

To develope the cosine of $n\,\theta$ in descending powers of cos θ.

Assume $2\cos\theta=x+x^{-1}$, then it may be easily shown that $2\cos n\,\theta=x^n+x^{-n}$. Substituting in (IX.), $2\cos n\,\theta$ for x^n+x^{-n}, $2\cos\theta$ for z, and dividing the whole equation by 2^n, we find

$$\left.\begin{aligned}\frac{\cos.n\theta}{2^{n-1}} &= \cos^n\theta - \frac{n\cos^{n-2}\theta}{1\ 2^2} + \frac{n.n-3}{1\ 2}, \frac{\cos^{n-4}\theta}{2^4}\\ &- \frac{n.n-4.n-5}{1\ 2\ 3}.\frac{\cos^{n-6}\theta}{2^6}\ \ldots\ldots\end{aligned}\right\}. \text{(X.)}$$

The expansion of $\cos n\theta$, in powers of $\cos\theta$, fails when n is either negative or fractional, as $x^n + x^{-n}$ cannot be expanded in those cases in powers of x. It is also clear that the expansion can contain no negative powers of $\cos\theta$, as negative powers of x cannot appear in the development of $x^n + x^{-n}$.

To expand $\sin n\theta$ *in powers of* $\sin\theta$.

The investigation of this development must be divided into two cases, as n is odd or even. First let n be odd.

Put $\theta = \frac{\pi}{2} - \phi$, then $n\theta = \frac{n\pi}{2} - n\phi$, $\cos n\theta = \sin n\phi$,

and $\cos\theta = \sin\phi$.

Making these substitutions in (X.), we find

$$\frac{\sin n\phi}{2^{n-1}} = \sin^n\phi - \frac{n}{1}\frac{\sin^{n-2}\phi}{2^2} + \frac{n.n-3}{1\ 2}\frac{\sin^{n-4}\phi}{2^4}\ldots \&c.\ \text{(XI.)}$$

Again, when n is even

Differentiating (X.), and dividing by $n\,d\theta$, we find

$$\left.\begin{aligned}\frac{\sin n\theta}{2^n} &= \sin\theta\left\{\frac{\cos^{n-1}\theta}{2^1} - \frac{n-2}{1}\frac{\cos^{n-3}\theta}{2^3}\right.\\ &\left.+\frac{n-3.n-4}{1\ 2}\frac{\cos^{n-5}\theta}{2^5}.\right.\end{aligned}\right\}. \text{(XII.)}$$

Now let $\theta = \frac{\pi}{2} - \phi$, then $\sin n\theta = \mp\sin n\phi$, $\sin\theta = \cos\phi$, $\cos\theta = \sin\phi$. Making these substitutions in (XII.), there results

$$\begin{aligned}\mp\frac{\sin n\phi}{2^n} &= \cos\phi\left\{\frac{\sin^{n-1}\phi}{2^1} - \frac{n-2}{1}\frac{\sin^{n-3}\phi}{2^3}\right.\\ &\left.+\frac{n-3.n-4}{1\ 2}\frac{\sin^{n-5}\phi}{2^5},\right.\end{aligned}$$

the upper sign being taken when n is divisible by 4, if otherwise, the lower.

XXIII. *On the Formation of the Cumulus Cloud*. *By* THOMAS HOPKINS, *Esq.*[*]

IN the summer of last year, 1839, I went to the top of Snowdon, and found the whole of the mountain covered with a thick mist or cloud. While descending from the upper part of it, about two o'clock in the afternoon, I looked back and observed the summit quite free from cloud: In June last I again ascended the mountain, and again found it enveloped in the same kind of thick mist; but remembering that on my previous visit the mist had cleared away in the afternoon, I resolved to wait on the top. About two o'clock in the afternoon the mist began to break, driving along from the south-west and passing round the summit of the mountain. The wind was moderate, and the mist for some time concealed the country below to the north and the east; it, however, in a short time disappeared, and left the country open to view.

Towards the end of August I was at New Brighton and Waterloo near Liverpool, when the wind was also moderate and generally from the south, south-west, or west, and I made use of the opportunity which then presented itself, to notice the appearances of the clouds, and took notes at the time, of which the following are a sample:—

"*New Brighton*, *Aug.* 26, 1840.—At six in the morning a slight wind from the west; the sea covered with a darkish cloud, apparently resting on the water; the land also covered with low cloud, but it was less heavy than that over the sea. As the sun rose in the heavens the lower portion of the thick cloud disappeared from the part immediately over the sea, and cumulus clouds formed over the land, apparently near to the two chains of the Lancashire and Welsh mountains. These increased in size from about nine o'clock in the morning until one in the afternoon, and became fine massive clouds. There being a slight breeze from the west they slowly floated eastward, where they assumed a darker appearance. Other cumuli, which during the afternoon remained over the sea, rose higher in the atmosphere, and the tops gradually wasted away, the whole mass becoming elongated, and looking like large fish floating in the air. As the sun went down in the west these clouds became more like dark lines, and when the sun had set the sky soon became clear, and the stars shone out brightly."

For a fortnight afterwards the weather was similar in its general character, and it may be described as follows:—At sun-rise a thick mist rested on the Irish Sea, so as to conceal the greater part of it from view. Any ship that was on this part of the sea, and not too far off, seemed to be floating in the air. As the sun became more powerful, a transparent space appeared between the water and the mist above it, and the mist began to take more decidedly the appearance of dense cloud. This transparent space by degrees enlarged, and the cloud as it rose became nearly a straight line or stripe. Between eight and nine o'clock little irregular protuberances or cones began to appear on the upper portion of the stripe of cloud. These cones by degrees swelled and enlarged their sizes, both vertically and horizontally. By placing my eye against a fixed object and looking along a line, I could see the gradual swelling of the masses. They had rough irregular cauliflower-like tops, and the particular forms varied as they grew larger. The lower part of the cloud at the same time preserved its horizontal level, but became darker in appearance. This process continued until large cumuli were formed, extending over the sea apparently from the Welsh to the Westmoreland hills. While this was going on the whole mass rose, and by twelve o'clock was at a considerable elevation, the upper part of which looked like a number of heaps of fleecy cotton wool piled into irregular hills. Under the highest of these hills the cloud was the darkest, and the swelling movement in them resembled that seen in smoke issuing from a chimney, but was much slower. The light west or south-west wind, which generally prevailed, carried these masses of cloud eastward, where they seemed to accumulate and become darker. About noon, or shortly after, we commonly had two distinct ridges of fine bold cumuli, one extending across the sea, apparently from the great Orme's Head to the Westmoreland hills, and the other stretching over Wales and Cheshire to the Yorkshire hills. Recollecting the clouds which I had noticed forming about the sides and top of Snowdon, I was strongly impressed with the idea that these ridges came from Snowdon and the other mountains in its vicinity. About the same time of the day that I had formerly observed Snowdon to become clear from mist, namely, about two or three o'clock in the afternoon, these clouds generally ceased to come from that quarter, and those already formed rose higher, and seemed disposed to separate into detached masses. Later in the afternoon they became lighter, and generally dissolved gradually, the whole frequently disappearing a little after sunset. When the clouds were very heavy a different result was witnessed, as they joined together and gave out sprinklings of rain, and showers afterwards fell more or less frequently during the night. Over the lands of Lancashire and Cheshire similar clouds formed, but they were more broken and irregular. At one time, when the morning mist had been raised, say apparently 100 or 150 yards above the surface, a most beautiful view was presented. An indefinite number of thin clouds seemed to be hung vertically in the air, of various degrees of transparency, with spaces intervening, so as to give a sufficiently distinct view of the lower part of each film of cloud. The whole looked like an immense hall with a mixture of glass and thin muslin drapery suspended in the upper part, the particular appearances changing every few minutes. Between these vertical films of cloud there appeared to be slight tremulous upward movements of the air, similar to those which are seen over heated sands or over the top of a hot stove, indicating that there were ascending currents of air in those places. Similar appearances were presented at other times, but they were less clear.

---

[*] Communicated by the Author; having been read before the Literary and Philosophical Society of Manchester in 1840.

In watching such scenes, one of the most striking circumstances presented to the notice is the formation of the cumulus cloud. It takes so definite a form, has such an uniformity of character, appears so plainly to grow up before your eyes, and ascend from the surface of the land or water to a considerable elevation in the heavens, and is so grand in its appearance, that it becomes invested with a peculiar interest. The prevailingopinion which has existed for a long time past respecting the formation of clouds has, I believe, been, that they resulted from an intermixture of different currents of air, and that the small particles of water which constitute the cloud, notwithstanding appearances, do *not* in reality rise in opposition to the laws of gravity. But any one who will take the trouble to watch the formation of the cumuli over the Irish Sea, say from eight to twelve o'clock in the morning, towards the latter end of summer, will find it difficult to resist the conviction that they really *do* rise in the atmosphere. The same identical mass of cloud may often be distinctly traced from the surface of the sea until it passes over your head at a considerable elevation; and the swelling prominences of the upper part of the cloud when seen in profile are so like in shape and motion to those which may be seen in smoke issuing from a chimney, or in steam when escaping from a boiler, as to induce an impression that similar causes produce all the effects. Mr. Espy, in his lectures recently delivered in this town, professed to account for the formation of "the Cumulus;" but some gentlemen think he did so in an unsatisfactory manner, it therefore seems desirable that his theory should be subjected to the test of an examination. This gentleman (Mr. Espy) says, that the sun when it rises increases the temperature of that part of the atmosphere which is near to the surface of the earth, and causes it to ascend to a greater elevation; this fact was previously known, and is, I presume, generally admitted. The part of the atmosphere near the earth which thus ascends has within it some certain quantity of steam or elastic aqueous vapour. When the air rises to an adequate height, and is consequently sufficiently cooled, a part of this steam is condensed. A new process now takes place. Upon the condensation of this portion of steam, latent caloric, or caloric of elasticity, is liberated, and this caloric attaches itself to surrounding bodies, and raises their temperature. In this way beds or strata of warm air are formed at different heights in the atmosphere.

But if liberated caloric raises the temperature of masses of air floating high up in the atmosphere, we know that those masses will expand through the increase of their elasticity. The additional elastic force thus acquired must be exerted in all directions, but as there will be the least resistance from above the force will be first developed principally in a vertical direction, and the whole column above will be raised, and caused to expand laterally in the higher regions of the atmosphere. On this second heating power, arising from condensation, being exerted, and a part of the atmosphere raised and thrown off laterally, the air below, relieved from a portion of the incumbent pressure, will spring upwards, and the surrounding air near the surface of the earth will flow in to restore the equilibrium of atmospheric pressure. Thus a local and temporary upward current will be created; and this upward current taking with it steam from near the surface of the earth, successive condensations take place—successive expansions follow; and the condensed steam, taking the shape of cloud, becomes a cumulus, and swells and grows in the way that has just been attempted to be described. The generally slow formation of the cloud, and the slight results which ordinarily follow, are, it is presumed, attributable to the limited portion of steam that is in the atmosphere. When steam is deficient in the raised air, no cloud is formed; when it is plentiful, clouds are freely formed; and when it is very abundant, the clouds are heavy and rain falls. The liberation of caloric by condensation it is evident may be effected in the smallest degree conceivable, and the process described may be slow, and the result only the elevation of the temperature of limited portions of the upper atmosphere, in the way in which the temperature of the tropical atmosphere is raised. Evaporation in the tropical regions furnishes a full supply of steam to the atmosphere, which steam being carried upwards a slight condensation takes place, that increases the temperature and expansive force of the mass of air, and carries it into higher regions. This being done successively and constantly, the whole upper region is sufficiently warmed and expanded to carry the small condensed particles of water, the remaining steam, and the atmospheric gases to the proper height, where the whole flow over north and south towards the poles. The hazy atmosphere of the tropical seas is probably nothing more than an immense cumulus cloud, continually formed and carried away, north and south, the causes in operation being so regular as to prevent the cloud from being broken up or dispersed. At the outer edges of this tropical cloud disturbances take place similar to those which occur when cumuli are formed in the temperate regions. When the steam is very abundant and atmospheric disturbance great, the process which produces cumuli may go on and give rise to heavy rains, and possibly to storms; whether that process is always the sole or even principal cause in operation during storms is a subject worthy of investigation, but is one into which it is not proposed to enter at present.

It has been said by some persons who object to the theory here advanced, that when steam rises until it is cooled by expansion, sufficiently to produce condensation, upon the smallest particle of the latent heat of the steam being given out the cooling is stopped. They therefore contend that condensation *could not continue*, as it would be terminated at the very commencement of the process. And this would be true, if steam alone existed in the atmosphere. Were nothing present but steam it does not appear how there could be any considerable or continued rise of temperature from condensation, because any, the smallest, increase of the temperature must stop the condensation, and thus destroy the only source from whence is derived the increase of that temperature. But there are gases present with the steam and diffused through it, forming with it a minute mechanical intermixture. And when the condensation of steam liberates caloric, the liberated caloric attaches itself to the gases, as well as to the remaining portion of the steam, increases the expansive force of these gases, causes them to spring up, and to carry with them the steam that is diffused through and entangled among their particles. Steam, it will be remembered, forms but a small proportion of the whole atmosphere. When the temperature and dew-points are at 32° of Fahrenheit, steam constitutes but a $\frac{1}{240}$th part of the atmosphere; when they are at 52° it forms a $\frac{1}{120}$th part of it; when at 73° it is a $\frac{1}{60}$th part; and when at 80° it is only $\frac{1}{48}$th part. And as the dew-point in this part of the world is not often above 52°, steam cannot be said to form at any time here much more than 1 per cent. of the atmosphere. The gases then being considered as 99, and steam as 1, the liberated caloric would attach itself to a much greater extent to the gases than to the remaining steam. The elasticity of the gases would be thereby increased; they would spring up, and carry the remainder of the steam with them to a sufficient height to cause fresh condensation to take place, and thus the process might be continued; not only is steam a very small portion of the whole atmosphere, but it cools according to a different law to that which regulates the cooling of the gases, and the difference in the laws by which the steam and the gases cool, causes the latter to cool the former. In ascending in the atmosphere, it is found that, when undisturbed by recent condensation, the temperature is reduced about 1° for every hundred yards of elevation. But the steam and the gases separately cool nearly according to the following table, taking whole numbers in a regular series as a sufficient approximation to the truth for our present purpose :—

At a height of about	1700	feet steam cools, say	1°,	while the gases cool	5°.
...	3400	...	2°	...	10°.
...	5000	...	3°	...	15°.
...	10,000	...	6°	...	30°.

Here we see that in an ascending column of steam and gases intermixed, while the former would by their ascension cool by expansion only one degree, the latter would cool five degrees; the gases, therefore, in their ascent would act the part of coolers to the steam. Being intimately intermixed the colder body would abstract heat from the warmer, and practically the gases when they rise must cool the steam that is intermixed with them. Then, as the steam is condensed more caloric is given out, and the gases again warmed and expanded; and thus these different elastic fluids, condensable and non-condensable, act and react upon each other, and give results different from those which would be found if they existed in the atmospheric space separately from each other. A portion of the atmosphere, nearly saturated with steam, being raised by a force acting from below to a height of 1700 feet, the steam within it would be cooled, according to its own law of cooling, 1°; but the gases, by their different law of cooling, would be cooled 5°; and as these fluids would be intimately intermixed, the lower temperature of the gases must, to a greater or less extent, be communicated to the steam, and a partial condensation of the latter must follow. This condensation we have seen would give out caloric, which, by raising the temperature of the remaining steam, would certainly have a tendency to prevent further condensation; but the temperature of the gases would also be raised, and their elastic force increased: they would therefore rise higher, and the remaining steam would be mechanically carried up with them sufficiently high to produce further condensation. That the liberated caloric would expand the gases enough to cause them to rise in the atmosphere is apparent, as it requires but little caloric to give considerable additional elasticity to the atmospheric gases. And on the condensation of steam much latent caloric is given out, there would therefore be an adequate supply of caloric to produce the effects named. It has been computed that the evolution of heat from the condensation of a pound of steam would be sufficient to raise the temperature of 3657 cubic feet of air 10°. Every pound of rain or cloud that is formed, will consequently raise the temperature of the atmosphere to that proportional extent.

This view, thowever, does not rest entirely on theoretical reasoning; that there are beds of warm air mixed with floating particles of water occasionally found in the atmosphere, is known from the experience of aëronauts, as well as from others who have ascended mountains. Mr. Green found such a bed at a height of nearly 10,000 feet from the earth, where the dew-point was at 64°, the same as at the surface of the earth at the time. At 11,060 feet, only 1·060 feet higher, the dew-point had fallen to 32°, only one half the temperature; because, there is no doubt, he had then got above the warming influence of condensation of steam. The effect of condensation in raising temperature, is seen also in the method practised of heating water by passing steam into it. It is found that one pound of steam will raise the temperature of 1000 pounds of water 1°. And Mr. Espy has shown, in the instrument he called "a Nephelescope," that rarefaction of the air, effected by removing pressure from it, cooled it after a certain rate or law; but this law of cooling was disturbed as soon as condensation took place. Condensation counteracted the cooling effect of expansion about one half, showing that if a column of air, in which steam had been recently condensed, were to be examined, it would be found that the temperature would be reduced, as we ascended in that column only one half of what it would be in a neighbouring column where no condensation had taken place. Suppose in the latter column the temperature to diminish 1° for every one hundred yards of elevation, as before shown, then, in the former column, where condensation had taken place, the temperature would be diminished only half a degree for every one hundred yards of height. If the foregoing facts and reasonings be admitted, it will follow that on condensation of steam taking place in the atmosphere there will be an upward atmospheric movement, which will cause fresh condensation and further upward movement, and this process may form the cumulus cloud, which may rise to a less or greater height in the atmosphere, and the cloud may be dissipated or may give out rain according to the quantity of steam contained in the atmosphere, and the condensing power of cold.

The atmospheric movements which have been here considered, are represented by Mr. Espy as originating with, and caused by, the direct heating power of the sun, but the action of the sun may be only one of the causes which produce the phænomena. Any cause which shall have the effect of elevating a column of the atmosphere sufficiently to cause condensation of steam to begin, may commence the process described, and cause cumuli to form. A current of air having nearly a maximum quantity of steam for the temperature, being forced by the inertia of the mass against a ridge of hills, the lower part must rise, and in rising must force up the whole of the column that rests upon it. This forcing up may be sufficient to raise the column high enough to condense some of the steam which it contains, when all the effects just described would take place in the same way as would have occurred, if the heating power of the sun, acting on the surface of the earth, had been the prime mover. In this way the ridges of mountains in Caernarvonshire and Merionethshire may force up the current of air flowing against them from the south-west, until the process of condensation commences and clouds are formed. And the long lines of cumuli flowing apparently from about Snowdon northward towards Westmoreland, or eastward towards Yorkshire, when they reach the English hills may get another lift, and the copious rains that fall in those parts may be thus accounted for. If mountains can force up atmospheric currents, it is evident that when different atmospheric currents move irregularly, one may force up another. On two currents meeting or crossing each other, one may force its way under the other, lift it up, and thus cause condensation to commence. In this way cumuli may be formed over land or sea, and rain may be discharged, and possibly even storms commenced, provided there be an adequate supply of steam for condensation; and all this may take place in the absence of the sun,—in the night as well as in the day. Should various currents moving in opposite directions penetrate each other at different elevations, it will follow, from what has been advanced, that clouds may be formed at the same time at different elevations, and each stratum of cloud may be affected according to the quantity of steam contained in it, and the condensing power of the elevation to which it should be raised. In this part of the world cumuli generally come from the westward, because the winds from that quarter are more fully charged with steam than those from any other quarter. But they may be sometimes seen coming from the eastward over the Yorkshire hills, and these may have originated either in the elevating power of the sun, or of the hills. The passage of small cumuli across the heavens does not produce a sensible effect on the barometer. An alteration may be observed in the flatness of the top of the mercury when one of considerable size has been passing. Very large ones, dark at the under side, sometimes lower the mercury a little; but it seems to require one of great extent sufficient to cover the horizon, to cause a decided fall in the barometer.

XXIV. *Reply to* Prof. Challis, *on the Investigation of the Resistance of the Air to an Oscillating Sphere. By* G. B. Airy, *Esq., Astronomer Royal.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I DID not intend to trouble you again on the subject of discussion between Professor Challis and myself. But the remarks of my esteemed friend, in his communication to your July Number, have tended so much to confuse the matter, and some of the doctrines which he has brought forward are (if I understand them rightly) so dangerous to the purity of mathematical reasoning, that I must crave your permission to place this series of remarks in your Journal. I promise that I will not occupy any more of your valuable space by continuing this discussion.

On the new equations discovered by Professor Challis as having claims for consideration, and marked by him (2.), (4.), (5.), page 65, I have only to remark, that I certainly supposed *every* person who knows anything of partial differentials (Professor Challis and myself included) to be fully aware that where there are n independent variables there are $\frac{n\,.\,\overline{n-1}}{2}$ such equations. But Professor Challis's language seems to imply that he is hardly aware that $\overline{n-1}$ of the equations are sufficient for nearly every purpose, and that they do contain absolutely and completely all the equations of the next order involving different independent variables in each differentiation, and that (with due care in integration) they do therefore contain the remaining equations of the same order. Thus Professor Challis's equation (2.), or $\frac{d}{dz}\left(\frac{dP}{dx}\right) = \frac{d}{dx}\left(\frac{dP}{dz}\right)$ gives, on differentiation with respect to y, $\frac{d^2}{dy\,dz}\left(\frac{dP}{dx}\right) = \frac{d^2}{dx\,dy}\left(\frac{dP}{dz}\right)$; but this same result may be obtained by differentiating my equations (1.) and (3.), or $\frac{d}{dy}\left(\frac{dP}{dx}\right) = \frac{d}{dx}\left(\frac{dP}{dy}\right)$, and $\frac{d}{dz}\left(\frac{dP}{dy}\right) = \frac{d}{dy}\left(\frac{dP}{dz}\right)$. Professor Challis will find, on examination, that I have used all which were required.

But Professor Challis states that these six equations "are exactly verified by the kind of motion which Mr. Airy considers to be impossible" (page 64, line 29), and then proceeds to show (page 66) that Poisson's law of motion satisfies these equations. What particular obscurity in my expressions, or what omission of my explanatory words in Professor Challis's reading of them, can have led to such a misinterpretation of my meaning as this sentence implies, I cannot imagine. My words (page 322, line 15) were, "If, in order to support Professor Challis's expression for the movement of the particles to or from a centre, we suppose, &c. &c." I did not object to Poisson's expression, or to any other involving cos θ as a multiplier; but I remarked (page 328, line 17) that "the *onus* of proving that the three equations are consistent rests with the supporter, &c."; in other words, that the possibility is not to be assumed without proof, and that each case must rest on its own proof. That I did not intend to object to every expression involving cos θ as a multiplier in the formula for the resolved part of the velocity in the direction of radius, will be sufficiently evident from this circumstance; that the simplest of all kinds of motion, namely, that of a uniform current of air, in which all the particles move with the same velocity in parallel lines, is included in this case. My objection was specially to "*Professor Challis's* expression."

Professor Challis, in the proof that Poisson's assumed motion is possible, has preferred referring to the function ϕ, where $\frac{d\phi}{dx} = u =$ velocity in the direction of x; $\frac{d\phi}{dy} = v =$ velocity in the direction of y, $\frac{d\phi}{dz} = w =$ velocity in the direction of z. I have no objection to this (although it was inconvenient at first for the examination of Professor Challis's expression, because Professor Challis had not given the form of ϕ adopted by him), and, taking the general equations for ϕ as given by Professor Challis, I will show that Poisson's solution is tenable, and that Professor Challis's solution is untenable. The general equations are these:—

ϕ must be possible as a function of x, y, z, t,

$$\frac{d^2\phi}{dt^2} = a^2\left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2}\right).$$

Velocity in the direction of radius $= \frac{d\phi}{dr}$.

I proceed now with the substitution of the two expressions.

I. Poisson's Solution.

Here $\phi = \left\{\frac{f(r-at)}{r^2} - \frac{f'(r-at)}{r}\right\}\cos\theta$

$$= x\left\{\frac{f(r-at)}{r^3} - \frac{f'(r-at)}{r^2}\right\}.$$

Differentiating, and remarking that $\frac{dr}{dx} = \frac{x}{r}$, $\frac{dr}{dy} = \frac{dy}{dr}$, $\frac{dr}{dz} = \frac{z}{r}$,

$$\frac{d\phi}{dx} = zx\left\{-\frac{3f(r-at)}{r^5} + \frac{3f'(r-at)}{r^4} - \frac{f''(r-at)}{r^3}\right\}$$

$$\frac{d^2\phi}{dx^2} = z\left\{-\frac{3f(r-at)}{r^5} + \frac{3f'(r-at)}{r^4} - \frac{f''(r-at)}{r^3}\right\}$$
$$+zx^2\left\{+\frac{15f(r-at)}{r^7} - \frac{15f'(r-at)}{r^6} + \frac{6f''(r-at)}{r^5} - \frac{f'''(r-at)}{r^4}\right\}.$$

Similarly,

$$\frac{d^2\phi}{dy^2} = z\left\{-\frac{3f(r-at)}{r^5} + \frac{3f'(r-at)}{r^4} - \frac{f''(r-at)}{r^3}\right\}$$
$$+zy^2\left\{+\frac{15f(r-at)}{r^7} - \frac{15f'(r-at)}{r^6} + \frac{6f''(r-at)}{r^5} - \frac{f'''(r-at)}{r^4}\right\}.$$

Then

$$\frac{d\phi}{dz} = \left\{\frac{f(r-at)}{r^3} - \frac{f'(r-at)}{r^2}\right\} + z^2\left\{-\frac{3f(r-at)}{r^5} + \frac{3f'(r-at)}{r^4} - \frac{f''(r-at)}{r^3}\right\}$$

$$\frac{d^2\phi}{dz^2} = 3z\left\{-\frac{3f(r-at)}{r^5} + \frac{3f'(r-at)}{r^4} - \frac{f''(r-at)}{r^3}\right\} + z^3\left\{+\frac{15f(r-at)}{r^7} - \frac{15f'(r-at)}{r^6} + \frac{6f''(r-at)}{r^5} - \frac{f'''(r-at)}{r^4}\right\}.$$

The sum, or $\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2}$ is

$$5z\left\{-\frac{3f(r-at)}{r^5} + \frac{3f'(r-at)}{r^4} - \frac{f''(r-at)}{r^3}\right\} + zr^2\left\{+\frac{15f(r-at)}{r^7} - \frac{15f'(r-at)}{r^6} + \frac{6f''(r-at)}{r^5} - \frac{f'''(r-at)}{r^4}\right\}$$

$$= z\left\{\frac{f''(r-at)}{r^3} - \frac{f'''(r-at)}{r^2}\right\}.$$

But $\frac{d^2\phi}{dt^2} = a^2 z\left\{\frac{f''(r-at)}{r^3} - \frac{f'''(r-at)}{r^2}\right\}.$

Therefore $\frac{d^2\phi}{dt^2} = a^2\left(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy^2} + \frac{d^2\phi}{dz^2}\right)$: and therefore Poisson's solution is possible.

II. PROFESSOR CHALLIS'S SOLUTION.

Professor Challis has not, in his first papers, given a value for ϕ, but he has assumed (Phil. Mag. and Journal, December 1840), in page 463, line 25, "the velocity and density of the fluid which passes the area m^2 are v and ρ," and in page 465, line 7, &c. $v = \frac{f'(r-at)}{r} - \frac{f(r-at)}{r^2}$. From page 463, line 14, it is evident that this is the velocity in the direction of the radius: and from page 465, line 23, as also from page 131 (February 1841), line 6, that it is to have the factor $\cos\theta$, without which it cannot be adapted to the different points of the sphere's surface. Hence we have

$$\frac{d\phi}{dr}=\left\{\frac{f'(r-at)}{r}-\frac{f(r-at)}{r^2}\right\}\cos\theta$$

$$\phi=\frac{f(r-at)}{r}\cos\theta+\psi,$$

where ψ is a function of the angles which determine the position of r, and of t. If we make $\frac{x}{r}=l$, $\frac{y}{r}=m$, $\frac{z}{r}=n$, we may consider ψ as a function of l, m, n, t.

Using therefore $\phi=z\frac{f(r-at)}{r^2}+\psi\left(\text{since }\cos\theta=\frac{z}{r}\right)$,

$$\frac{d\phi}{dx}=zx\left\{-\frac{2f(r-at)}{r^4}+\frac{f'(r-at)}{r^3}\right\}+\frac{d\psi}{dx}$$

$$\frac{d^2\phi}{dx^2}=z\left\{-\frac{2f(r-at)}{r^4}+\frac{f'(r-at)}{r^3}\right\}$$
$$+zx^2\left\{+\frac{8f(r-at)}{r^6}-\frac{5f'(r-at)}{r^5}+\frac{f''(r-at)}{r^4}\right\}$$
$$+\frac{d^2\psi}{dx^2}.$$

Similarly,

$$\frac{d^2\phi}{dy}=z\left\{-\frac{2f(r-at)}{r^4}+\frac{f'(r-at)}{r^3}\right\}$$
$$+zy^2\left\{+\frac{8f(r-at)}{r^6}-\frac{5f'(r-at)}{r^5}+\frac{f''(r-at)}{r^4}\right\}$$
$$+\frac{d^2\psi}{dy^2}.$$

Then

$$\frac{d\phi}{dz}=\frac{f(r-at)}{r^2}+z^2\left\{-\frac{2f(r-at)}{r^4}+\frac{f'(r-at)}{r^3}\right\}+\frac{d\psi}{dz}$$

$$\frac{d^2\phi}{dz^2}=3z\left\{-\frac{2f(r-at)}{r^4}+\frac{f'(r-at)}{r^3}\right\}$$
$$+z^3\left\{+\frac{8f(r-at)}{r^6}-\frac{5f'(r-at)}{r^5}+\frac{f''(r-at)}{r^4}\right\}$$
$$+\frac{d^2\psi}{dz^2}.$$

The sum, or $\frac{d^2\phi}{dx^2}+\frac{d^2\phi}{dy^2}+\frac{d^2\phi}{dz^2}$ is

$$5z\left\{-\frac{2f(r-at)}{r^4}+\frac{f'(r-at)}{r^3}\right\}$$

$$+ z r^2 \left\{ + \frac{8 f(r-at)}{r^6} - \frac{5 f'(r-a t)}{r^5} + \frac{f''(r-a t)}{r^4} \right\}$$

$$+ \frac{d^2 \psi}{d x^2} + \frac{d^2 \psi}{d y^2} + \frac{d^2 \psi}{d z^2}$$

$$= z \left\{ - \frac{2 f(r-at)}{r^4} + \frac{f''(r-a t)}{r^2} \right\} + \frac{d^2 \psi}{d x^2} + \frac{d^2 \psi}{d y^2} + \frac{d^2 \psi}{d z^2}.$$

But $\frac{d^2 \phi}{d t^2} = a^2 z \frac{f''(r-a t)}{r^2} + \frac{d^2 \psi}{d t^2}.$

And therefore $\frac{d^2 \phi}{d t^2} - a^2 \left\{ \frac{d^2 \phi}{d x^2} + \frac{d^2 \phi}{d y^2} + \frac{d^2 \phi}{d z^2} \right\}$

$$= 2 a^2 z \frac{f(r-a t)}{r^4} + \frac{d^2 \psi}{d t^2} - a^2 \left\{ \frac{d^2 \psi}{d x^2} + \frac{d^2 \psi}{d y^2} + \frac{d^2 \psi}{d z^2} \right\}.$$

It is necessary for the possibility of Professor Challis's solution, that the last expression be = 0. The question therefore is, Can ψ be so determined as to make it = 0? It clearly cannot. For, in order to make it = 0, ψ must contain functions of $r-a t$; but this is evidently inconsistent with its primary condition, that it should be a function of $\frac{x}{r}$, $\frac{y}{r}$, $\frac{z}{r}$, and t, and should contain r in no other way. Professor Challis's solution therefore is not possible.

In two or three passages (as page 64, line 21, and page 68, line 2), Professor Challis seems to intimate that there is a distinction to be made between the cases of motion in the direction of the radius from a fixed centre and motion in the direction of the radius from a moving centre. This distinction (as I understand it) is groundless; and for this reason, that the alteration which it makes in the place of a particle of air is of the *second* order with regard to the disturbance; and quantities of the second order are neglected through the whole investigation. Besides, it is evident that if the resolved part of the motion of the air, as measured from a fixed centre, is represented by cos θ × function of r, there will always be provided the proper vacuum of air to be occupied by the sphere (the oscillations being small). I trust, however, that I have misunderstood the meaning of the expressions.

Although I do not entirely agree with Professor Challis in attaching slight importance to this special problem, yet I consider the correctness or incorrectness of his result as almost insignificant when compared with the legitimacy or spuriousness of the process by which it has been obtained. With this estimation, I beg leave to call attention to the principle hinted at in page 67, line 22, &c. It will be remembered that ϕ is a function which is obtained by solving a partial differential equation, and that (as always happens) it contains in its expression another function f, whose form is arbitrary; *i. e.* any form may be used for f, provided that the quantity which is used as the subject of the function be that which is given by the solution, viz. $(r-at)$, and provided that the function chosen be placed exactly in the place of f in the solution, and be treated in no other way than that in which f is treated. Yet Professor Challis says, "The function ϕ may thus contain implicitly, *as a factor*, *another function* expressing the variation of velocity at a given instant in passing from one point to another in directions perpendicular to the motion, *but is not differentiated with respect to the variables of this factor*." And a little further on, having found an expression $\phi = \dfrac{f(r-at)}{r}$, he adds, "*the factor* $\cos\theta$, *depending on the mode of disturbance, being included in the arbitrary function.*" That is to say, having found that a certain set of differential equations is satisfied by the expression $\phi = \dfrac{f(\sqrt{x^2+y^2+z^2}-at)}{\sqrt{x^2+y^2+z^2}}$, Professor Challis assumes that we may use as a solution $\dfrac{z\,.f(\sqrt{x^2+y^2+z^2}-at)}{x^2+y^2+z^2}$, and that in substituting it in the differential equations we have no occasion to differentiate the factor $\dfrac{z}{\sqrt{(x^2+y^2+z^2)}}$. Against this I must record my solemn protest. The function which is to be put in the place of the arbitrary function is simply to be put in its place, and to be modified in no other way whatever. If the solution in that form cannot be adapted to the mode of disturbance or to the initial motion, it is a sign that (among the infinity of solutions applying to fluids) one has been chosen which is inapplicable to the conditions of the problem, and another must therefore be tried. As to the omission of certain terms in the process of differentiation, I confess that I am surprised. Any person so well acquainted as Professor Challis is with the transformations of the equations for fluids, must be aware that the introduction ofthe peculiar function ϕ is a matter of convenience only; that it does not at all modify the suppositions on which the fundamental equations are obtained; that these fundamental equations are the four given by Professor Challis at the bottom of page 64; that they are established on consideration of the complete and absolute change in the state of pressure, density, and velocity, of the particles of the fluid, in proceeding from a point where the coordinates have one value to another point where they have a different value; and that it is absolutely impossible that there can be, in forming them, any limitation as to the functions whose change is or is not to be taken into account. I am quite certain that the new doctrine of omitting certain functions in the differentiation cannot stand a moment, when examined with reference to the original understanding on which the first equations have been obtained.

Perhaps the error of this principle may be made more obvious by considering a simpler case. Suppose we consider the motion of parallel plane waves through air. It is well known that the disturbance of a particle in the direction of x may be expressed by $X = \cos\alpha . \phi(vt - x\cos\alpha - y\cos\beta - z\cos\gamma)$. Suppose that there is a wall in the air, whose equation is $x = 0$. The value of X must therefore vanish when $x = 0$. But it does not vanish then. What must be done to make it vanish? A follower of Professor Challis would say, "Multiply it by a function which will vanish when $x = 0$, as for instance $a\,x$, and omit the variation of this factor in all differentiations." (This mode of escaping from the difficulty is not at all more forced than that adopted by Professor Challis in the passages to which I have adverted.) But I am sure that Professor Challis himself would say, "Not so; the process proposed is illegitimate; the failure of the expression (in its susceptibility of adaptation to the circumstances at the surface of the wall) is to be remedied, not by multiplying it by a function which vanishes there, but by altering the whole formula, so as to preserve the condition of satisfying the original differential equations, and also to satisfy the new condition relating to the wall." And thus he would obtain the formula

$$X = \cos\alpha . \phi(vt - x\cos\alpha - y\cos\beta - z\cos\gamma)$$
$$- \cos\alpha . \phi(vt + x\cos\alpha - y\cos\beta - z\cos\gamma).$$

I will only, in conclusion, express my regret at finding myself compelled to place myself so distinctly in opposition to my excellent friend Professor Challis. Had the writer been one of lower character or in a less influential position, or had the subject been one familiar to a greater number of mathematicians, I should have let it pass. But in observing the publication of principles the most dangerous to the *honesty* of mathematics (if I may use such an expression) that I have ever seen, I could not avoid looking to the position of the writer, as well as to my own position, and forming my decision as to the course which was proper on my part. I have only to add, that nothing could be further from my intention than to give a personal character to this controversy, and that I trust no expression has escaped me which will bear such an interpretation.

I am, Gentlemen,
your obedient Servant,
G. B. Airy.

Royal Observatory, Greenwich,
July 10, 1841.

XXV. *On the Phænomena of Diffraction in the Centre of the Shadow of a Circular Disc, placed before a luminous Point, as exhibited by Experiment.* By Richard Potter, *Esq., M.A.*[*]

THE results of the undulatory theory, to which I called attention in the Number of the Magazine for October last[†], being opposed to the received opinions upon those points, it became desirable to undertake an experimental examination of the most decisive case; namely, the nature of the phænomena in the centre of the shadow of a circular disc when placed directly before a luminous point. According to that theory the point in the centre of the shadow ought to be as bright as if the disc were not interposed; and this being acknowledged on all sides, we have an admitted and clear test of the theory, as long as it is allowed to be a theory which may be tested by experiment.

The labour which the Astronomer Royal has devoted to deducing the numerical magnitudes, given in his paper in the January Number[‡], has also rendered this experiment more interesting, and I may anticipate the result at which I have arrived, by stating that the undulatory theory in this, as in so many other cases, fails entirely.

Before relating the experiments, I must notice some of Mr. Airy's statements as to the visibility of luminous points, and the relative brightness which renders neighbouring spaces which are differently illuminated distinguishable from each other by the eye; because an incorrect assumption on these points would affect the whole discussion. To commence with the latter, he says, "Thus there will be at the centre a bright spot of double the general intensity, surrounded by rings brighter than the great expanse of light; but the excess of intensity in the rings, even the first, is so small, that it probably could not be seen." Now the intensity of the minima being 1·, that of the maximum in the first bright ring is 1·16, in the second is 1·09, and in the third is 1·06.

---

[*] Communicated by the Author.

[†] Third Series, vol. xvii. p. 243.

[‡] vol. xviii. p. 1.

To judge whether these or any of them could be seen, we may compare them with the expression for the relative intensities in Newton's rings seen by transmission, as given in Mr. Airy's tracts, page 305. This expression gives the ratio of the intensities in a dark and bright ring as 1· to 1·173, a ratio of the same order of magnitude as the above, which Mr. Airy assumes as representing relative intensities which probably could not be seen, although Newton's rings seen by transmitted light are a well-known conspicuous phænomenon. This also affects Mr. Airy's statement on a similar case in his paper "On the Theoretical Explanation of an apparent new Polarity in Light," in the Phil. Trans. for 1840. At p. 242, he says, "The destruction of bands is here as to sense complete," &c., where the ratio of the intensities in a minimum and a maximum were as 1· to 1·287 in one case, and as 1· to 1·098 in another, and yet in the former the difference of intensities is greater than in Newton's rings, which are admitted to be distinguishable. In Mr. Airy's opinions and experience, respecting the magnitudes which are sufficient to render luminous points visible, we have to notice the following. In his paper in the January Number of the Magazine, he says, "Substituting in the expression above, we find the diameter of the bright spot = 0·000308 inch, or less than $\frac{1}{3000}$ inch, regarding the visibility of which in common experience we need not to disquiet ourselves.

"If the diameter of the circular plate had been taken 1 inch, the distance of the screen remaining the same, the diameter of the spot would have been 0·0037 inch, a speck difficult even for a philosopher to discover under these circumstances. If the diameter of the plate were 0·1 inch, the diameter of the spot would be 0·037 inch, a very fit subject for experimental measure." In the fifth volume of the Cambridge Phil. Trans., p. 110, we find Mr. Airy saying, "I have used both parallelogrammic and circular holes of different sizes (the largest circular hole being $\frac{1}{4}$ inch in diameter), and have sometimes diminished the aperture to as little as $\frac{1}{1000}$ inch (by estimation.)" Thus we have Mr. Airy estimating the magnitude of *an aperture* of about $\frac{1}{1000}$ inch at one time, and at another time he would not have us disquiet ourselves as to the visibility of a *bright point surrounded by darkness*, when its diameter was as little as $\frac{1}{3000}$ inch. Yet we find the fixed stars are visible without having any sensible diameter; and the images of Jupiter's satellites formed in a telescope, with an object-glass of 30 inches focal length, have their diameters only about $\frac{1}{8000}$ inch, and yet they are easily seen by such an eye-lens as in common experience we use in examining phænomena of diffraction: be it remembered these satellites are also bodies reflecting a very feeble light. Further, Mr. Airy says, a spot whose diameter is 0·0037 inch, or about $\frac{1}{300}$ inch, is "a speck difficult even for a philosopher to discover under these circumstances." Now $\frac{1}{300}$ inch is the breadth of an ordinary hair of the human head, and these are to be seen of a perceptible breadth, even as opake bodies *surrounded by opake bodies*: would not a philosopher both see, and expect to see, bright spots *when surrounded by darkness*, even if they were in diameter only a very small fraction of this magnitude?

To proceed to the immediate subject of the paper: I made preparation for trying the experiment several months ago, but when the apparatus was completed it was some time before a clear sunshine occurred to suit the aspect of the window of my darkened room, at a time when I could command leisure to pursue the experiments.

I prepared, by turning and polishing very carefully in a lathe, discs of brass of $\frac{1}{10}$, $\frac{2}{10}$, $\frac{3}{10}$, $\frac{4}{10}$, and $\frac{7}{10}$ inch in diameter, and with watch-makers' implements one of $\frac{1}{20}$ inch diameter. The luminous point was the sun's image formed by a lens of $\frac{1}{6}$ inch focal length, and therefore it was of considerably less diameter than $\frac{1}{600}$ inch. The discs were attached by thin films of cement to a plate of glass with flat and parallel surfaces, which had never been before used, and they were so formed that the diffracting circular edge was raised from the glass; in some of them this edge was formed by the junction of the side and base of the frustum of a very obtuse-angled cone, and in others the edge was formed by the junction of two such frustums. The base by which they were attached to the glass was in all of much smaller diameter than the diffracting edge, so that the cement which oozed out round the edge of this base, was far out of the way of the light concerned in the phænomena of diffraction. The sun's light was reflected horizontally through a window-shutter of a darkened room, and the sun's image, formed by the lens of $\frac{1}{6}$ inch focal length, was at 60 inches distance from the disc, at the same time that the focus of the eye-lens, by which the phænomena of diffraction were examined, was at 60 inches distance on the other side. The plate of glass to which the discs were cemented, was rendered perpendicular to the incident light by adjusting until the reflected light passed directly back again. I generally used the apparatus so that the plate of glass was between the disc and luminous point, but I found no difference in the phænomena when it was reversed, or the disc between the plate and the luminous point. It was also found, on trial, that the appearances were not sensibly changed if the focus of the eye-lens were rather more or rather less than 60 inches from the disc.

When the whole was adjusted, on looking through an eye-lens of about 1 inch focal length, at the centre of the shadow cast by a disc, there was seen a bright central spot, of a white colour slightly tinged with brown, surrounded by a greater or less number of coloured rings, according to the size of the disc. The central bright spot in the shadow of the disc of $\frac{1}{20}$ inch diameter was large, and so bright, that at the first view it would have been taken to be equally bright with the light which had passed uninterruptedly, further from the edge of the disc than any interference was visible. But experience in photometrical experiments immediately pointed out to me the allowance to be made for the heightening effect of contrast, the bright spot being surrounded by a dark ring. It was evidently necessary to place the portions of light, which were to be compared, in the same circumstances. To effect this, I perforated a plate of thin sheet brass with the point of a needle, making a number of small circular holes of different sizes and at different distances, so that they could be taken in pairs of equal size. Now placing this brass plate in the focus of the eye-lens, where it was moveable and capable of adjustment, and causing one of these holes to transmit the *central part* of the central bright spot, and then comparing it with the light passing through an equal hole at a distance beyond any visible interference, it was seen at once that the brightness of the centre of the central bright spot in the shadow was very much less intense than that of the light passing through the other hole. In the shadow of the larger discs the inferiority of the brightness of the central spot was evident at once without any precautions, and the intensity appeared to diminish rapidly with the increase of diameter of the disc. The capability of the undulatory theory to explain the phænomena is thus completely set at rest; for, according to it, the brightness of the centre of the shadow should be equal to that of the uninterrupted light for all magnitudes of discs.

To obtain an estimate of the relative intensity of the light of the central spot for the disc of $\frac{1}{20}$ inch diameter, I adopted the following contrivance:—Having cut a number of small plates of mica out of the same sheet, I tried how many of these placed before the uninterrupted light reduced its intensity to an equality with that of the centre of the central spot, when the brass plate perforated with the small circular holes was used. From the result of many trials I concluded that four plates produced the most accurate correspondence. To find how much light these mica plates transmitted, I looked through a rhomb of Iceland spar, at a strip of white paper so broad that the two images overlapped, and of course at such overlapping the intensity was the double of what it was on either side of it: a similar effect was produced by somewhere between two and three of the mica plates. I estimated that it was nearer two than three plates which were required; but to a scientific friend, with whom, on another occasion, I repeated it, it appeared to be nearer three than two that were requisite, so that I shall consider $2\frac{1}{2}$ of the mica plates as transmitting only one half of the light: and four such plates would therefore transmit one-third part very nearly. This shows how widely the fact was at variance with the result of the undulatory theory even with a disc of $\frac{1}{20}$ inch diameter.

To perform these experiments the eye-lens and brass plate in its focus were mounted on a stand so as to be raised or lowered at pleasure, and the mica plates, mounted in bundles of two, three and four, were attached to a rod of wood, so as to be interposed in the light in succession without inconvenience.

I have also the following memoranda: in the shadow of the $\frac{1}{20}$ inch disc, besides the central spot, two rings around it were visible; in that of the $\frac{1}{10}$ inch disc, four rings were visible; in that of the $\frac{2}{10}$ inch disc, five or six were visible; and in that of the $\frac{3}{10}$ inch disc, seven or eight were visible: and also as the diameter of the disc was increased, the intensity of the rings became more nearly equal to that of the central spot. The discs of $\frac{4}{10}$ and $\frac{7}{10}$ inch I did not use, as I must have provided larger plates of glass to attach them to, for the edge of the plates of glass to which the latter disc was attached caused interference sufficient to distort the rings when this plate was two inches square.

I must be allowed to state, that I consider the controversy, as to the undulatory theory being the physical theory of light, to be nearly terminated; and that the experiments necessary for completing the basis of a physical theory are those now most desirable to be undertaken.

Queen's College, June 9, 1841.

XXVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xviii. p. 561.]

May 13, 1841. THE following papers were read, viz.—

1. "Meteorological Observations for August, September, and October, 1840, taken on board H.M.S. *Erebus* and *Terror*, by and under the direction of Capt. James Clark Ross, R.N., Commander of the Antarctic Expedition." Presented by the Lords Commissioners of the Admiralty, and communicated by the President of the Royal Society.

2. "Hourly Meteorological Observations made at Plymouth, in latitude 52° 36′ 12″, longitude in time 6^m 55^s east, on the 22nd of March, 1841." By Arthur Utting, Esq. Communicated by Capt. Edward Johnson, R.N., F.R.S.

3. "Barometrical Observations taken at Naples at 9 A.M. on each day during the months of January and February, 1841." By Sir Woodbine Parish, K.C.H., F.R.S. Presented by direction of the Council of the Royal Geographical Society, and communicated by S. H. Christie, Esq., Sec. R.S.

4. "Memoir of the case of a gentleman born blind, and successfully operated upon in the eighteenth year of his age; with Physiological Observations and Experiments." By J. C. August Franz, M.D., M.R.C.S. Communicated by Sir Benjamin C. Brodie, Bart., F.R.S.

The young gentleman who is the subject of this memoir had been affected from birth with strabismus of both eyes; the right eye was amaurotic, and the left deprived of sight by the opacity both of the crystalline lens and of its capsule. At the age of seventeen, an operation for the removal of the cataract of the left eye was performed by the author with complete success. On opening the eye for the first time, on the third day after the operation, the patient described his visual perception as being that of an extensive field of light, in which everything appeared dull, confused, and in motion, and in which no object was distinguishable. On repeating the experiment two days afterwards, he described what he saw as a number of opake watery spheres, which moved with the movements of the eye, but when the eye was at rest remained stationary, and their margins partially covering one another. Two days after this the same phenomena were observed, but the spheres were less opake and somewhat transparent; their movements were more steady, and they appeared to cover each other more than before. He was now for the first time capable, as he said, of looking through these spheres, and of perceiving a difference, but merely a difference, in the surrounding objects. The appearance of spheres diminished daily; they became smaller, clearer, and more pellucid, allowed objects to be seen more distinctly, and disappeared entirely after two weeks. As soon as the sensibility of the retina had so far diminished as to allow the patient to view objects deliberately without pain, ribands differently coloured were presented to his eye. These different colours he could recognize, with the exception of yellow and green, which he frequently confounded when apart, but could distinguish when both were before him at the same time. Of all colours, gray produced the most grateful sensation: red, orange and yellow, though they excited pain, were not in themselves disagreeable; while the effect of violet and of brown was exactly the reverse, being very disagreeable, though not painful. Brown he called an ugly colour: black produced subjective colours; and white gave rise to a profusion of *muscæ volitantes*. When geometrical figures of different kinds were offered to his view, he succeeded in pointing them out correctly, although he never moved his hand directly and decidedly, but always as if feeling with the greatest caution. When a cube and a sphere were presented to him, after examining these bodies with great attention, he said that he saw a quadrangular and a circular figure, and after further consideration described the one as being a square, and the other a disc, but confessed that he had not been able to form these ideas until he perceived a sensation of what he saw in the points of his fingers, as if he really touched the objects. Subsequent experiments showed that he could not discriminate a solid body from a plane surface of similar shape; thus a pyramid placed before him, with one of its sides towards his eye, appeared as a plane triangle.

Two months after the above-mentioned operation, another was performed on both eyes, for the cure of the congenital strabismus, by the division of the tendons of the recti interni muscles, which produced a very beneficial effect on the vision of the left eye; and even the right eye, which had been amaurotic, gained some power of perceiving light, and, from being atrophied, became more prominent. Still it was only by slow degrees that the power of recognizing the true forms, magnitudes, and situations of external objects was acquired. In course of time, the eye gained greater power of converging the rays of light, as was shown by the continually increasing capacity of distinct vision by the aid of spectacles of given powers[*].

May 20.—The following papers were read, viz.—

1. "Catalogue of Geological Specimens procured from Kerguelen's Land during the months of May, June, and July, 1840."

2. "Catalogue of Birds collected on board Her Majesty's Ship *Terror*, between the Cape of Good Hope and Van Diemen's Land."

3. "Description of Plants from Kerguelen's Land, collected in May, June, and July, 1840."

The above papers are by John Robertson, Esq., Surgeon of Her Majesty's Ship *Terror*, and were presented to the Society by the Lords Commissioners of the Admiralty, and communicated by the President of the Royal Society.

4. "On the Fossil Remains of Turtles discovered in the Chalk

---

[*] In Phil. Mag., First Series, vol. xxviii. p. 203, will be found an account, by Sir E. Home, of two children born with cataracts, and the nature of their vision after the operations of depression and extraction.—Edit.

Formation of the South-East of England." By Gideon Algernon Mantell, Esq., LL.D., F.R.S.

In this paper the author gives a description, accompanied with drawings, of a remarkable fossil Turtle, referable to the genus *Emys*, and named from its discoverer, Mr. Bensted, the *Emys Benstedi*, which has been lately found in a quarry of the lower chalk of Kent, at Burham, which is situated near the banks of the Medway, between Chatham and Maidstone. The specimen discovered consists of the carapace or dorsal shell, six inches in length and nearly four inches in breadth, with some of the sternal plates, vertebræ, eight ribs on each side of the dorsal ridge, a border of marginal plates, and one of the coracoid bones. It is adherent to a block of chalk by the external surface of the sternal plates. The marginal plates are joined to each other by finely indented sutures, and bear the impress of the horny scales or tortoise-shell, with which they were originally covered. The expanded ribs are united together throughout the proximal half of their length, and gradually taper to their marginal extremities, which are protected by the plates of the osseous border. Mr. Bell considers the species to which it belonged as being closely allied in form to the common European Emys, and as possessing a truly fluviatile or lacustrine character. The plates of the plastron, however, as also the coracoid bone, resemble more the corresponding bones of marine than of freshwater turtles.

5. "Researches tending to prove the Non-vascularity of certain Animal Tissues, and to demonstrate the peculiar uniform mode of their Organization and Nutrition." By Joseph Toynbee, Esq. Communicated by Sir Benjamin Brodie, Bart., F.R.S., &c.

The above was only in part read.

May 27.—The following papers were read, viz.—

1. "On the Compensations of Polarized Light, with the description of a Polarimeter for Measuring Degrees of Polarization." By Sir David Brewster, K.H., D.C.L., F.R.S., and V.P.R.S. Ed.

In four papers published in the Philosophical Transactions for 1830, the author maintained, in opposition to the prevailing theory, that light, either reflected or refracted at angles different from that at which it is completely polarized, does not consist of two portions, one completely polarized, and the other completely unpolarized, but that every portion of it has the same physical property, having approximated in an equal degree to the state of complete polarization. This conclusion, which had been derived from reasoning on the hypothesis that a pencil of light, composed of two pencils polarized respectively at angles of + and — 45° with the plane of reflexion, was equivalent to a pencil of common light, is confirmed in this paper by experiment, made with common light itself, reflected from different parts of the atmosphere, and from which the uniaxal or biaxal systems of rings were obtained. On placing such a system between light partially polarized in an opposite plane, the author found that the rings disappeared, the direct system being seen on one side of the plane of disappearance, and the complementary system on the other side. In this experiment the polarization of the light in one plane was compensated by the polarization of the same light in the opposite plane; and, consequently, both the pencils, which had undergone the two successive polarizing actions, had received the same degree of polarization in opposite planes; and in virtue of these two equal and opposite polarizations, the light at the point of compensation, where the system of rings disappeared, had been restored from partially polarized to common light; and the light on each side of this point of compensation was in opposite states of partial polarization.

In order to give a distinct view of the nature of this experiment, the author details the phenomena observed at particular angles of incidence on glass. From the results at an angle of incidence of 24°, the ray suffering one refraction at 80°, and a second reflexion at 83½°, he concludes that the compensation which takes place is produced neither by an equality of oppositely polarized rays, nor by a proportional admixture of common light, but by equal and opposite physical states of the whole pencil, whether reflected or refracted.

The remarkable phenomena produced at an angle of incidence on glass of 82° 44′ (at which angle $\cos(i + i') = \cos^2(i + i')$), led the author to the construction of what he terms *the compensating rhomb*, consisting of a well-annealed rhomb of glass, or any other uncrystallized substance, having the angles of its base 130° 25′ and 46° 35′ respectively, when the index of refraction is 1·525. When a ray of light is incident upon the first surface at an angle of 82° 44′, exactly one-half of it is reflected; and the other half, after refraction, is reflected at the second surface, and emerges perpendicularly to the adjacent surface, without suffering refraction; each portion having, in the first instance, the same quantity of polarized light. The second portion is found, on examination, to be in the state of common light, although the ray at the second incidence consisted of more than one-half of polarized light. Hence if the pencil, previously to reflexion at the second surface, consist of 145 rays of polarized light, and 188 of common light, the effect of a single reflexion must be to depolarize polarized light, and to produce no change whatever upon common light; a property of a reflecting surface never yet recognized, and incompatible with all our present knowledge on the subject of the polarization of light.

The author then describes an instrument which he has invented for the purpose of accurately measuring the degrees of polarization, and which he therefore terms a *Polarimeter*. It consists of two parts; one of which is intended to produce a ray of compensation having a physical character susceptible of numerical expression, and the other to produce polarized bands, or rectilineal isochromatic lines, the extinction of which indicates that the compensation is effected. The construction and mode of operation of this instrument are, by the aid of figures, described and explained.

The following is the general law established by these researches; namely, that the compensations of polarized light are produced by equal and opposite rotations of the planes of polarization. Thus, when a ray of common light is incident, at any angle, upon the polished surface of a transparent body, the whole of the reflected pencil suffers a physical change, bringing it more or less into a state of complete polarization, in virtue of which change its planes of polarization are more or less turned into the plane of reflexion; while the whole of the refracted pencil has suffered a similar, but opposite change, in virtue of which its planes of polarization are turned more or less into a plane perpendicular to the plane of reflexion.

The author then enters into a theoretical investigation of the subject, and concludes by pointing out a few of the numerous applications of his theory.

2. Continuation of the paper of which the reading commenced at the last Meeting, and entitled, "Researches tending to prove the Non-vascularity of certain Animal Tissues, and to demonstrate the peculiar uniform mode of their Organization and Nutrition." By Joseph Toynbee, Esq. Communicated by Sir Benjamin C. Brodie, Bart., F.R.S.

In the introduction to this paper, the author first speaks of the process of nutrition in the animal tissues which are pervaded by ramifications of blood-vessels; pointing out the circumstance, that even in these there is a considerable extent of tissue which is nourished without being in contact with blood-vessels. The knowledge of this fact leads us to the study of the process of nutrition in the non-vascular tissues; which tissues he divides into the three following classes; namely, first, those comprehending articular cartilage, and the cartilage of the different classes of fibro-cartilage. Under the second head he comprises the cornea, the crystalline lens, and the vitreous humour; and, under the third, he arranges the epidermoid appendages; viz. the epithelium, the epidermis, nails and claws, hoofs, hair and bristles, feathers, horn and teeth.

The author then proceeds to show that the due action of the organs, into the composition of which these tissues enter, is incompatible with their vascularity. In proof of the non-existence of blood-vessels in these tissues, he states that he has demonstrated, by means of injections, that the arteries, which previous anatomists had supposed to penetrate into their substance, either as serous vessels, or as red-blood vessels too minute for injection, actually terminate in veins before reaching them; he also shows that around these non-vascular tissues there are numerous vascular convolutions, large dilatations and intricate plexuses of blood-vessels, the object of which he believes to be to arrest the progress of the blood, and to allow a large quantity of it to circulate slowly around these tissues, so that its nutrient liquor may penetrate into and be diffused through them. The author states that all the non-vascular tissues have an analogous structure, and that they are composed of corpuscles, to which he is induced to ascribe the performance of the very important functions in the process of their nutrition, of circulating throughout, and perhaps of changing the nature of the nutrient fluid which is brought by blood-vessels to their circumference. The author then brings forward facts in proof of the active and vital properties of these corpuscles, and concludes his Introduction by stating, that it appears to him, that the only difference in the mode of nutrition between the vascular and the non-vascular tissues is, that in the former, the fluid which nourishes them is derived from the blood that circulates throughout the capillaries contained in their substance; whilst, in the latter, the nutrient fluid exudes into them from the large and dilated vessels that are distributed around them: and that in both classes, the particles of which the tissues are composed derive from this fluid the elements which nourish them.

The author then enters on an examination of the structure and mode of nutrition of the several tissues of each of these three classes. In considering the first class, he commences with articular cartilage, which he describes at great length in the various stages of its development, and at the different periods of life. He gives in detail the account of numerous dissections of the ovum and fœtus illustrating the first stage, during which he shows that no blood-vessels enter into the substance of any of the textures composing a joint; but that the changes its component parts undergo, are effected by the nutrient fluid from the large blood-vessels, by which, at this stage, each articulation is surrounded. In the second stage of the development of articular cartilage, the author shows, by numerous dissections, the process by which the blood-vessels are extended into the substance of the epiphysal cartilage, and converge towards the attached surface of articular cartilage, and how, at the same time, blood-vessels are equally prolonged over a certain portion of its free surface. He shows that none of these blood-vessels enter the substance of the articular cartilage, and he points out that in them the arteries become continuous with the veins; first, by their terminating in a single vessel, from which the veins arise; secondly, by their forming large dilatations from which the veins originate; and, lastly, they become directly continuous with the veins in the formation of loops of various characters. In the third stage, that which is exhibited in adult life, the epiphysal cartilage is converted into osseous cancelli. These contain large blood-vessels, which are separated from the articular cartilage by a layer of bone composed of corpuscles, and the author believes that the principal source of nutrition to this tissue is the nutrient fluid which exudes into it from these vessels, by passing through the articular lamella just noticed. The free surface of adult articular cartilage is nourished by vessels which pass to a slight extent over it. The author points out the presence of fine tubes which pervade the attached portion of adult articular cartilage, to which he ascribes the function of transmitting through its substance the nutritive fluid derived from the vessels of the cancelli. He also advances the opinion that the articular cartilage becomes thinner during the whole of life, by being gradually converted into bone.

Fibro-cartilage constitutes the second tissue of the first class. The author first enters upon an examination of its structure; and in order to arrive at some definite conclusions on this subject, whereon anatomists of all ages have so much differed, he made numerous dissections of fibro-cartilages in the different classes of animals at various periods of their development, the results of which he details. He arrives at the conclusion that this tissue is composed of cartilaginous corpuscles and of fibres; the latter preponderating in adult life, the former in infancy; and that during life the corpuscles are gradually converted into fibres. He enters at length into the question of the vascularity of these cartilages; and from a careful study of many injected specimens of man and animals at various periods of their development, the particular results of which he relates, he believes that blood-vessels are contained only in their fibrous portion, and have the function of nourishing that which is cartilaginous, and which, on account of its being subject to compression and concussion, does not contain any.

Among the second class of extra-vascular tissues, the cornea is first treated of; and its structure is described as being very lax, and as containing corpuscles only in a small quantity. The opinions in favour of its vascularity are combated; and it is shown that the blood-vessels which converge to its attached margin, and which are the principal source of the fluid that nourishes it, are large and numerous, and that at the circumference of this tissue the arteries, without any diminution of their calibre, return in their course, and become continuous with the veins. A second set of vessels, devoted to the nutrition of the cornea, is also described; they extend to a short distance over the surface of the tissue, but do not penetrate into its substance.

The crystalline lens is described as being composed of corpuscles, of which the radiating fibres are constituted. The arteria centralis retinæ is described as ramifying over the posterior surface of the capsule, where it forms large branches; these pass round the circumference of the lens, and reach its anterior surface, at the periphery of which they become straight: the arteries terminate in loops frequently dilated, and become continuous with the veins. With respect to the vascularity of the vitreous humour, the author states that although many anatomists have, in general terms, represented the arteria centralis retinæ as giving off, in its course through this organ, minute branches into its substance, still those who have paid especial attention to the subject, have not been able to find such vessels. He believes that the nutrition of this structure is accomplished by the fluid brought to its surface by the ciliary processes of the choroid, which fluid is diffused with facility through its entire substance by means of the corpuscles of which its membrane is composed, assisted by the semifluid character of the humour.

The third class of extra-vascular tissues comprehends the epidermoid appendages. The author describes them all as composed of corpuscles, which are round and soft where they are in contact with the vascular chorion, compressed and flattened where they are farther removed from it. He points out, in the substance of the hoof of the Horse, the existence of fine canals, which he supposes to conduct fluid through its mass; and he states that the perspiratory ducts of the human subject possess a structure analogous to the spiral vessels of plants. The author describes each of the tissues of this class at length, and shows that the various modifications presented by the vascular system with which each is in contact, have the sole object of enabling a large quantity of blood to approach and circulate slowly around them. He also points out, in connexion with this subject, the remarkable vital properties which are possessed by these non-vascular tissues.

In concluding this paper, the author states that his object has been to establish as a law in animal physiology, that tissues are capable of being nourished, and of increasing in size, without the presence of blood-vessels within their substance. He shows the analogy which is presented between the extra-vascular animal and the extra-vascular vegetable tissues. He expresses a hope that the application to surgery of the above law, with reference to the prolongation of blood-vessels into the extra-vascular tissues during disease, and to pathology in the investigation of the nature of morbid structures, particularly of those classes which contain no blood-vessels, will be not devoid of interest, and will be productive of some advantage.

The Society then adjourned over the Whitsun recess, to meet again on the 10th of June.

June 10.—The following papers were read, viz.—

1. "Magnetic-term Observations made at Milan, on the 21st and 22nd of April, 1841." Communicated by Professor Carlini, For. Memb. R.S.

2. "Register of Tidal Observations made at Prince of Wales's Island, in July, August and September, 1840."

3. "Register of Tidal Observations made at Singapore in July, August and September, 1840."

These two papers were presented by the Directors of the East India Company, and communicated by P. M. Roget, M.D., Sec. R.S.

4. "On the Anatomy and Physiology of certain structures in the Orbit, not previously described." By J. M. Ferrall, Esq., M.R.I.A. Communicated by Sir Benjamin C. Brodie, Bart., F.R.S.

The author describes a distinct fibrous tunic, which he terms the *tunica vaginalis oculi*, continuous with the tarsal cartilages and ligaments in front, and extending backwards to the bottom, or apex of the orbit; thus completely insulating the globe of the eye, and keeping it apart from the muscles which move it. The eye-ball is connected with this fibrous investment by a cellular tissue, so lax and delicate as to permit an easy and gliding motion between them. The use which the author assigns to this tunic is that of protecting the eye-ball from the pressure of its muscles while they are in action. This tunic is perforated at its circumference, and a few lines posterior to its anterior margin, by six openings, through which the tendons of the muscles emerge in passing to their insertions, and over which, as over pullies, they play in their course. A consequence of this structure is that the recti muscles become capable of giving rotatory motions to the eye without occasioning its retraction within the orbit, and without exerting injurious pressure on that organ. In those animals which are provided with a proper retractor muscle, the recti muscles are, by means of this peculiar mechanism, enabled to act as antagonists to that muscle.

5. "An account of some recent improvements in Photography." By H. F. Talbot, Esq. F.R.S.

The author had originally intended, in giving an account of his recent experiments in photography, to have entered into numerous details with respect to the phænomena observed; but finding that to follow out this plan would occupy a considerable time, he has thought that it would be best to put the Society, in the first place, in possession of the principal facts, and by so doing perhaps invite new observers into the field during the present favourable season for making experiments. He has, therefore, confined himself at present to a description of the improved photographic method, to which he has given the name of *Calotype**, and reserves for another occasion all remarks on the theory of the process.

The following is the method of obtaining the Calotype pictures.

Preparation of the Paper.—Take a sheet of the best writing paper, having a smooth surface, and a close and even texture.

The water-mark, if any, should be cut off, lest it should injure the appearance of the picture. Dissolve 100 grains of crystallized nitrate of silver in six ounces of distilled water. Wash the paper with this solution, with a soft brush, on one side, and put a mark on that side whereby to know it again. Dry the paper cautiously at a distant fire, or else let it dry spontaneously in a dark room. When dry, or nearly so, dip it into a solution of iodide of potassium containing 500 grains of that salt dissolved in one pint of water, and let it stay two or three minutes in this solution. Then dip it into a vessel of water, dry it lightly with blotting-paper, and finish drying it at a fire, which will not injure it even if held pretty near; or else it may be left to dry spontaneously.

All this is best done in the evening by candlelight. The paper so far prepared the author calls *iodized paper*, because it has a uniform pale yellow coating of iodide of silver. It is scarcely sensitive to light, but, nevertheless, it ought to be kept in a portfolio or a drawer, until wanted for use. It may be kept for any length of time without spoiling or undergoing any change, if protected from the light. This is the first part of the preparation of Calotype paper, and may be performed at any time. The remaining part is best deferred until shortly before the paper is wanted for use. When that time is arrived, take a sheet of the *iodized paper* and wash it with a liquid prepared in the following manner:—

Dissolve 100 grains of crystallized nitrate of silver in two ounces of distilled water; add to this solution one-sixth of its volume of strong acetic acid. Let this mixture be called A.

Make a saturated solution of crystallized gallic acid in cold distilled water. The quantity dissolved is very small. Call this solution B.

When a sheet of paper is wanted for use, mix together the liquids A and B in equal volumes, but only mix a small quantity of them at a time, because the mixture does not keep long without spoiling. I shall call this mixture the *gallo-nitrate of silver*.

---

* See our last Number, p. 88.—Edit.

Then take a sheet of *iodized paper* and wash it over with this *gallo-nitrate of silver*, with a soft brush, taking care to wash it on the side which has been previously marked. This operation should be performed by candlelight. Let the paper rest half a minute, and then dip it into water. Then dry it lightly with blotting-paper, and finally dry it cautiously at a fire, holding it at a considerable distance therefrom. When dry, the paper is fit for use.' The author has named the paper thus prepared *Calotype paper*, on account of its great utility in obtaining the pictures of objects with the camera obscura. If this paper be kept in a press it will often retain its qualities in perfection for three months or more, being ready for use at any moment; but this is not uniformly the case, and the author therefore recommends that it should be used in a few hours after it has been prepared. If it is used immediately, the last drying may be dispensed with, and the paper may be used moist. Instead of employing a solution of crystallized gallic acid for the liquid B, the *tincture of galls* diluted with water may be used, but he does not think the results are altogether so satisfactory.

Use of the Paper.—The *Calotype paper* is sensitive to light in an extraordinary degree, which transcends a hundred times or more that of any kind of photographic paper hitherto described. This may be made manifest by the following experiment:—Take a piece of this paper, and having covered half of it, expose the other half to daylight for the space of *one second* in dark cloudy weather in winter. This brief moment suffices to produce a strong impression upon the paper. But the impression is latent and invisible, and its existence would not be suspected by any one who was not forewarned of it by previous experiments.

The method of causing the impression to become visible is extremely simple. It consists in washing the paper once more with the *gallo-nitrate of silver*, prepared in the way before described, and then warming it gently before the fire. In a few seconds the part of the paper upon which the light has acted begins to darken, and finally grows entirely black, while the other part of the paper retains its whiteness. Even a weaker impression than this may be *brought out* by repeating the wash of gallo-nitrate of silver, and again warming the paper. On the other hand, a stronger impression does not require the warming of the paper, for a wash of the gallo-nitrate suffices to make it visible, without heat, in the course of a minute or two.

A very remarkable proof of the sensitiveness of the Calotype paper is afforded by the fact stated by the author, that it will take an impression from simple moonlight, not concentrated by a lens. If a leaf is laid upon a sheet of the paper, an image of it may be obtained in this way in from a quarter to half an hour.

This paper being possessed of so high a degree of sensitiveness, is therefore well suited to receive images in the camera obscura. If the aperture of the object-lens is one inch, and the focal length fifteen inches, the author finds that *one minute* is amply sufficient in summer to impress a strong image upon the paper of any building upon which the sun is shining. When the aperture amounts to one-third of the focal length, and the object is very white, as a plaster bust, &c., it appears to him that *one second* is sufficient to obtain a pretty good image of it.

The images thus received upon the Calotype paper are for the most part invisible impressions. They may be made visible by the process already related, namely, by washing them with the gallo-nitrate of silver, and then warming the paper. When the paper is quite blank, as is generally the case, it is a highly curious and beautiful phænomenon to see the spontaneous commencement of the picture, first tracing out the stronger outlines, and then gradually filling up all the numerous and complicated details. The artist should watch the picture as it developes itself, and when in his judgment it has attained the greatest degree of strength and clearness, he should stop further progress by washing it with the fixing liquid.

The fixing process.—To fix the picture, it should be first washed with water, then lightly dried with blotting paper, and then washed with a solution of *bromide of potassium*, containing 100 grains of that salt dissolved in eight or ten ounces of water. After a minute or two it should be again dipped in water and then finally dried. The picture is in this manner very strongly fixed, and with this great advantage, that it remains transparent, and that, therefore, there is no difficulty in obtaining a copy from it. The Calotype picture is a *negative* one, in which the lights of nature are represented by shades; but the copies are *positive*, having the lights conformable to nature. They also represent the objects in their natural position with respect to right and left. The copies may be made upon Calotype paper in a very short time, the invisible impressions being *brought out* in the way already described. But the author prefers to make the copies upon photographic paper prepared in the way which he originally described in a memoir read to the Royal Society in February 1839[*], and which is made by washing the best writing paper, *first* with a weak solution of common salt, and *next* with a solution of nitrate of silver. Although it takes a much longer time to obtain a copy upon this paper, yet when obtained, the tints appear more harmonious and pleasing to the eye; it requires in general from 3 minutes to 30 minutes of sunshine, according to circumstances, to obtain a good copy on this sort of photographic paper. The copy should be washed and dried, and the fixing process (which may be deferred to a subsequent day) is the same as that already mentioned. The copies are made by placing the picture upon the photographic paper, with a board below and a sheet of glass above, and pressing the papers into close contact by means of screws or otherwise.

After a Calotype picture has furnished several copies, it sometimes grows faint, and no more good copies can then be made from it. But these pictures possess the beautiful and extraordinary property of being susceptible of revival. In order to revive them and restore their original appearance, it is only necessary to wash them again by candlelight with gallo-nitrate of silver, and warm them: this causes all the shades of the picture to darken greatly, while the white parts remain unaffected. The shaded parts of the paper thus acquire an opacity which gives a renewed spirit and life to the copies, of which a second series may now be taken, extending often to a very considerable number. In reviving the picture it sometimes happens that various details make their appearance which had not before been seen, having been latent all the time, yet nevertheless not destroyed by their long exposure to sunshine.

---

[*] Printed in Phil. Mag., Third Series, vol. xiv. p. 209.—Edit.

The author terminates these observations by stating a few experiments calculated to render the mode of action of the sensitive paper more familiar.

1. Wash a piece of the *iodised paper* with the gallo-nitrate; expose it to daylight for a second or two, and then withdraw it. The paper will soon begin to darken spontaneously, and will grow quite black.

2. The same as before, but let the paper be warmed. The blackening will be more rapid in consequence of the warmth.

3. Put a large drop of the gallo-nitrate on one part of the paper and moisten another part of it more sparingly, then leave it exposed to a very faint daylight; it will be found that the lesser quantity produces the greater effect in darkening the paper; and in general, it will be seen that the most rapid darkening takes place at the moment when the paper becomes nearly dry; also, if only a portion of the paper is moistened, it will be observed that the edges or boundaries of the moistened part are more acted on by light than any other part of the surface.

4. If the paper, after being moistened with the gallo-nitrate, is washed with water and dried, a slight exposure to daylight no longer suffices to produce so much discoloration; indeed it often produces none at all. But by subsequently washing it again with the gallo-nitrate and warming it, the same degree of discoloration is developed as in the other case (experiments 1 and 2). The dry paper appears, therefore, to be equal, or superior in sensitiveness to the moist; only with this difference, that it receives a *virtual* instead of an *actual* impression from the light, which it requires a subsequent process to develope.

5. "New mode of preparation of the Daguerréotype plates, by which portraits can be taken in the short space of time of from five to fifteen seconds, according to the power of light, discovered by A. Claudet in the beginning of May 1841." Communicated by the Marquis of Northampton, Pres. R.S.

"My improvement," says the author, "consists in using for the preparation of the plates, a combination of chlorine with iodine, in the state of chloride of iodine. I follow the preparation recommended by Daguerre. After having put the plate in the iodine box for a short time, and before it has acquired any appearance of yellow colour, I take it out, and pass it for about two seconds over the opening of a bottle containing chloride of iodine; and immediately I put it again in the iodine box, where it acquires very soon the yellow colour, which shows that the plate is ready to be placed into the camera obscura. I have substituted to the chloride of iodine, chloride of bromine, and have found nearly the same result; but I prefer chloride of iodine as producing a better effect; and besides, on account of the noxious smell of bromine.

"The result of my preparation is such, that I have operated in ten seconds with the same apparatus, which, without any chlorine, required four or five minutes; when using only the original preparation of Daguerre, I have obtained an image of clouds in *four seconds*."

GEOLOGICAL SOCIETY.

[Continued from vol. xviii. p. 590.]

Dec. 16, 1840.—A paper "On the Relative Connection of the Eastern and Western Chalk Denudations," by P. J. Martin, Esq., F.G.S., was read.

The author advances this as the first of a series of papers on the construction of that part of the country usually considered as appertaining to the great chalk denudation of the Weald, or more properly, the upburst of the secondary formations between the tertiary of the respective basins of London and Hampshire.

In venturing on this field of inquiry, he professes also to take up the subject where it was left by him in two former memoirs, one published in 1828 under the title of a 'Geological Memoir of Western Sussex, with some Observations on Chalk Basins and the Weald Denudation*,' the other in the 'Philosophical Magazine' for February 1829†; and to extend the number of demonstrative facts that bear upon the theory of denudation by disruptive violence and contemporaneous aqueous abrasion, there brought forward as a corollary to Dr. Buckland's theory of 'Valleys of Elevation.'

In pursuance of this object, he begins by an examination into the arrangement of the great chalk dome of Hampshire and Wiltshire,—the *Patria* of the chalk of Pennant and Conybeare; its anticlinal lines of disturbance or upheaval, and their connections with those of the Weald and the smaller western denudations of Pewsey, Wardour and Warminster.

He finds that six great anticlinal lines are the main instruments of the upbearing of this abraded chalk; that the three which characterize the smaller anticlinal western valleys are projected onward, and in a manner decussate three others which emanate from the western extremity of the greater valley of the Weald, the vale of Wolmar Forest, from whence he starts his inquiry; and that these lines do not inosculate or enter into each other; approximating, indeed, but little in any part of their course; severally dying out, and their respective synclinal lines playing off into each other. Their course is rather irregular, and their force exceedingly variable; but their general parallelism is maintained throughout, their progress being E. and W., with a point to the N.

---

* An analysis of this memoir appeared in Phil. Mag., Second Series, vol. iv. p. 38.—Edit.

† Second Series, vol. v. p. 111.

The Pewsey line, after passing through the valleys of Ham and Kingsclere, is traceable between Woolverton and Hannington, on towards Monks Sherborne, and fades away at Old Basing, apparently without entering the tertiary beds of the London basin *. This meets in synclinal relation with a line projected from the north-west corner of the Wolmar valley from Pease Marsh, near Guildford, through Farnham and the high chalk range of Froyle, Shaldon, Dummer and Popham, and appears to fade away in the country west of Andover, where it is lost in the greater swell of the Burghclere Hills, and the more dominant power of the Pewsey upheaval.

The anticlinal line of Wardour, left by Dr. Fitton (in his 'History of the Beds below the Chalk' †) at Harnham Hill, S. of Salisbury, Mr. Martin finds traceable eastward, north of Dean Hill, and east of the Avon, to the banks of the Test, where it dips under the tertiary beds between Michaelmarsh and Romsey, and appears to fade away between the above-mentioned river and the Itching. In synclinal relation this line is also met and passed by a very remarkable anticlinal, traceable in strict approximation with, and by-and-by to be proved to be the *proximate cause* of, the whole line of the South Down escarpment (with a small exception between Lewes and Poynings) from Beachy Head to East Meon. In the vicinity of this place, at Langrish, it enters the chalk, passes through the anticlinal valley of Chilcomb near Winchester and that city, and is lost in the Bossington Hills, pointing towards, but not satisfactorily traced into, the Warminster line.

The details of all three lines of elevation are made out in the Ordnance Map, and sections given of the most illustrative points: and Mr. Martin adds some observations respecting the entrance of the great central line of elevation of the Weald into the chalk at Selborne, and its progress westward between the lines of Peasemarsh on the north, and of Greenhurst or the South Down on the south, till it fades away in the great plateau of Salisbury Plain.

The author concludes this paper with some reference to the subject of transverse fractures in these several longitudinal fissures, and the cross drainage, to which, like that of the Weald, he proposes to return, in extension and emendation of the disquisitions formerly published by him, as above alluded to, and which will be adduced as illustrative of the strong probability, if they do not amount (in connexion with the phænomena of drift) to absolute proof, of the close relation of the acts of upheaval and violent aqueous abrasion. This necessarily implies the belief that the date of these lines of disturbance is posterior to that of all the stratified beds of the south-east part of England, as maintained in the author's former essays; but into the full discussion of which he declines to enter till the whole subject is before the Society.

---

* The author thinks, that although this line fades away as it enters the tertiary beds at Old Basing, it is probable that, after passing silently along the London basin, it is revived again in the Isle of Thanet, which is a chalk outlier, by protrusion; in the same way that the parallel line of Portsdown Hill, High-down, near Worthing, and the Seaford Cliff (figured by Dr. Mantell) does on the southern coast.

† Geol. Trans., Second Series, vol. iv. p. 244. *et seq.*

Jan. 6, 1841.—A paper was first read "On the Illustration of Geological Phænomena by means of Models," by Thomas Sopwith, Esq., F.G.S.

Mr. Sopwith commences by stating, that drawings cannot convey to the mind a correct notion of geological phænomena where more planes than one are required; and that few persons are aware of the extraordinary changes which are produced in the combinations of strata by viewing them on different planes, especially if the strata are dislocated; or in undisturbed and parallel strata, of the totally different apparent forms which are exhibited on an undulating surface and in any plane section of the interior. This difference is of great importance in mining plans, where the surface only in the first instance is accessible to examination, and the observer is too apt to infer from it, the subterranean relations of the strata. To convey clear notions of these differences Mr. Sopwith has prepared a series of hand models, about two inches square, formed of layers of differently coloured woods, and capable of being dissected, to a certain extent, vertically, obliquely or horizontally. They are also so shaped on the upper surface as to exhibit the undulations of the ground. It is not possible to convey verbally, a clear notion of the many curious combinations exhibited by these models.

A paper was next read, "On the Geology of the island of Madeira," by James Smith, Esq., of Jordan Hill, F.G.S.

The crust of that island, Mr. Smith states, is composed to the depth of several thousand feet of subaërial volcanic matter, erupted during the tertiary period; and he adds, perhaps no other volcanic region offers more favourable opportunities for investigation. To account for its rugged and fragmentary character, it is not necessary, he says, to have recourse to the supposition that Madeira is one of the remains of a vast continent, as there are in the volcanic constitution of the island, and in the action of the mountain torrents, sufficient elements to afford data for explaining every physical phænomenon.

The igneous rocks composing the greater part of the island, are lavas, sand, and ashes, with bombs, lapilli, pumice, volcanic scoriæ or cinders, tufas and conglomerates; and the non-igneous, the limestone of San Vincente, the coal or lignite of San George, and the sands of Caniçal.

Volcanic Rocks.

The lavas are wholly basaltic, containing numerous crystals of olivine; and they are compact, scoriaceous and vesicular.

The compact variety occurs in beds or *coulées* interstratified with the other volcanic products, and in dykes which intersect all the igneous rocks. It is occasionally amorphous, but more often rudely columnar; it also, though rarely, presents regular columns; and it is sometimes schistose, possessing planes of cleavage as well as of regular stratification.

The scoriaceous basalt is rough and porous, resembling the slag of a foundry. Where the bed is thin it is scoriaceous throughout, but where it is of a certain thickness only the upper and lower surfaces exhibit this character. Two caverns of considerable magnitude occur immediately to the west of the principal landing-place at Funchal, and there are others in the island.

The vesicular lava or basalt presents through its whole mass a porous texture. The large vesicles have been flattened by the gravity of the lava, and elongated in the direction in which the *coulée* flowed. Where they are numerous and minute, they permit the rock to be easily hewn; and this variety is called *cantaria rija*, or the hard building stone.

The lapilli, sand, ashes and volcanic bombs, appear to have been projected simultaneously, as the bombs were evidently half imbedded in the finer materials by the force of their fall, the laminæ beneath them being bent upwards, and in some instances to a greater height on one side than the other, indicating the direction in which the bombs fell.

The pumiceous lapilli are white or light yellow, and rarely exceed in size a pigeon's egg. Beds of pumice, varying in thickness from a few inches to several feet, occur either on the surface or interstratified with the basalt and tufa; and they often contain portions of heavier volcanic products, as cinders or scoriæ, dispersed without regard to gravitation, proving, Mr. Smith says, that these various materials could not have been deposited under the sea, because in water they would instantly have separated according to their respective weights. The scoriæ or cinders also form extensive beds. They are generally reddish, and vary in size according to the distance from the orifice of eruption.

The ashes, both dark and light-coloured, are incoherent, except where they are mixed with earthy matter, or apparently fell on a heated zone of eruption; and in these cases they form a scoriaceous mass. Tufas and conglomerates compose a large proportion of the volcanic rocks of Madeira, and are considered to owe their consistency to water. Fragments of vegetables are not uncommon in them, but Mr. Smith is not aware that they contain any other organic remains. Many of these beds have been converted into vegetable soils, and it is interesting, the author says, to observe the roots of plants still in the attitude in which they grew; and to witness traces of the very same phænomena which are now taking place at the surface, in strata which have been buried for so many ages under solid rocks. The remains of plants are chiefly found in the vegetable soils, but their roots occasionally occur in the hard rock, and the cracks or fissures are in many cases filled with closely-matted masses of what was once roots and fibres, but now consist of carbonate of lime. Where the soils have been overflowed with lava, the vegetable remains are charred, and the soils have been burnt to the colour and hardness of brick; and if the overlying lava is of great thickness, a columnar structure appears. This conversion of soil into brick has been observed in the Azores and elsewhere.

The principal chain of mountains must at one time, Mr. Smith states, have been much higher, because their very summits consist of beds which are met with only at the base of active volcanic cones. There is consequently no great crater in the island, but there are the ruins of several truncated craters, and many small lateral cones. The most extensive of the former is the Curral dos Freiras, an immense ravine about three miles in length, one in breadth, and 2000 feet in depth, and open on its southern side. The beds of basalt, tufa and ashes of which it is composed, dip outwards to the base of the mountain, and parallel to its surface. Mr. Smith is convinced, the volcanic products of the island being subaërial, that this is not a crater of elevation, though it agrees with the characters which have been assigned to such craters; and he is further induced to infer, from the resemblance of the Curral dos Freiras to the more ancient portions of Teneriffe and the other Canary Islands, said to be craters of elevation raised from beneath the level of the sea, that a wrong conclusion has been drawn respecting them. He does not object to there being elsewhere true craters of elevation.

The principal lateral cones are to the west of Funchal, but they are in general so completely blotted with vegetation that their structure is concealed. In the ditch of a fort constructed on one of them, called Pico de St. João, a scoriaceous conglomerate intersected by minute basaltic veins is exposed; and a similar conglomerate occurs in the fortified island in Funchal Bay, also at the eminence at the landing-place. Some of these cones are covered by beds of lava and tufa erupted from craters at Cape Giram, in one instance to the thickness of 1400 feet. The beauty and regularity, within limited distances, of these volcanic strata, and the richness and variety of their colouring, are exceedingly striking. The most remarkable volcanic series, amounting to many hundred beds, is at Cape Giram, the cliff, 1600 feet in height, being stratified from the base to the summit. It has been rent in many places, and the fissures which terminate upwards in acute angles, have been filled with lava ejected from below.

Non-volcanic Rocks.

The limestone of San Vincente, Bowditch considered to be transition, but Mr. Smith shows that it belongs to the tertiary epoch, yet he believes it to be the fundamental rock of the island. It crosses a mountain stream between 2000 and 3000 feet above the level of the sea, and abounds in zoophytes and marine testacea belonging to the genera *Cardium, Pecten, Pectunculus, Spondylus, Cypræa, Voluta, Fasciolaria, Strombus* and *Murex*. The state of preservation, generally in that of casts, rendered it impossible to determine accurately the species. The limestone is traversed by two dykes of basalt, and it lies immediately under the Paul de Serra, a volcanic plateau which rises 2500 feet above the limestone.

The coal or lignite occurs on the north side of the island, on the banks of one of the tributaries of the St. George. Professor Johnston considers it to be the dried relict of an ancient peat bog, and its lustre, compactness and rhomboidal fracture to be due to the action of the basalt which overlies it. An analysis gave

Carbon	60·7
Hydrogen	5·82
Oxygen and nitrogen	33·47
	99·99

and 20·05 per cent. of ash. This is the organic constitution of true peat; but no peat exists at present in Madeira, nor as far as Mr. Smith is aware, has any been noticed so near the equator. He therefore suggests that this deposit may indicate a former colder climate in that latitude.

The sands of Caniçal are found near the eastern extremity of the island, in a valley which extends from the northern to the southern shore; and they consist of small particles of basalt and comminuted testacea, enclosing vast numbers of land shells as well as calcareous incrustations of plants. The shells have been most carefully examined by the Rev. Mr. Lowe, and one sixth ascertained to belong to species not now found living in the island; the Caniçal sands therefore are assigned by Mr. Smith to the Pleistocene or newest tertiary æra. The calcareous incrustations have been considered by some observers not to be of vegetable origin, in consequence of the general absence of organic structure; and Dr. Macaulay is of opinion, from their consisting of carbonate and phosphate of lime, silica and animal matter, that they are of animal origin, and probably belonged to the family of *Alcyonidæ*. As however they are mere casts, Mr. Smith conceives that an analysis cannot throw light on their origin; and as all of them bear the most perfect resemblance to trunks or branches, and one of his specimens exhibits impressions externally of cellular structure, he has no doubt of their being the calcareous casts of plants.

In one of the small islets adjoining Porto Santo is a bed of fossiliferous limestone, which supplies the kilns of Funchal. The fossils consist almost exclusively of casts, but Professor Agassiz having been enabled to identify some of them with casts of recent species, Mr. Smith infers that the limestone is an extremely modern formation, though it has all the characters of primary marble. In this case, the volcanic action, Mr. Smith states, was evidently submarine, as the contact of the basalt and the limestone is so intimate that the two rocks never separate when a mass composed partly o each is detached by force. The elevation of the islet above the level of the sea has not, however, disturbed the horizontal position of the beds.

On the island of Porto Santo the volcanic action was sub-aërial, as the basalt is scoriaceous on the surface, and rests on volcanic brick. In this island there is a sandy deposit similar to that at Caniçal. The Disertas, lying about three leagues to the south-east of Madeira, Mr. Smith describes as a chain of volcanic mountains ranging north and south, or nearly at right angles to that of Madeira. The sea-cliffs reach to their very summits, and exhibit a series of beds of basalt, ashes, tufas, and volcanic brick, intersected by innumerable dykes. No fossils have yet been discovered on these islands.

The occurrence of the marine limestone of San Vincente at an elevation of 2500 feet, proves a relative change in level of land and sea to that amount, previously, Mr. Smith is of opinion, to the ejection of the overlying volcanic products; but he has not observed in Madeira any proofs of elevation of the land during or subsequent to the volcanic period; though there are strong indications of subsidence, the beds of scoriæ and ashes, and those containing vegetable remains, dipping under the sea, and occurring in situations where they could not have remained, had the sea level been always the same as at present.

A letter, dated Madras, July 1840, addressed to John Taylor, Esq., Treas. G.S., by Mr. Frederick Burr, on the Geology of Aden, on the coast of Arabia, was afterwards read.

The promontory of Aden, eighty miles eastward of the Straits of Bab-el-mandeb, consists of a bold cluster of volcanic rocks with lofty jagged peaks, and is connected with the main land by a low isthmus. Its extreme length is about six miles, and its breadth is about three miles, and the summit of the highest point is about 1776 feet above the level of the sea. The loftier portions of the promontory are wholly volcanic, and the lower are partly volcanic and partly consolidated sea-sand. The most interesting portion of the district is an immense, nearly circular crater, situated at the extremity of the promontory next the main land, and in the centre of which, upon a flat little raised above the sea-level, stands the town of Aden. The diameter of the crater is about one and a half mile, and it is surrounded on all sides but the eastern with precipices chiefly composed of lava, and rising from 1000 to 1776 feet in height. Although the crater appears at first sight almost perfect, Mr. Burr says, it has been affected by some rude shocks which have cleft it entirely through from north to south, forming two rents, known as the northern and southern passes. The portion to the west of the fissures, and called the Gebel Shunsam, rising to the height of 1776 feet, stands entire; but that to the east has evidently undergone a partial subsidence, attaining to not more than half the height of the western side, and for the distance of about half a mile it has been broken down, allowing the sea to come almost close to the town and form a little bay; but the direction of the original outline of the crater is indicated by the island of Seerah, situated in about the middle of the gap.

To the northward of this great crater is an immense mass of lofty and jagged volcanic products, probably the remains of smaller craters.

The prevailing rock is a dark brown or chocolate lava, generally of a very cellular structure. About the middle of the east side of the great crater, it contains a very thick mass, composed of alternations of greenish porphyry, slightly lamellar in structure; and of red ochreous clay. Near the northern pass Mr. Burr noticed a granular rock, or volcanic breccia. The inclination of the beds is generally 15° from the crater.

Numerous perpendicular dykes intersect the volcanic rocks, and are harder and more compact than the beds they traverse. Small veins of calcedony also occur.

Dr. Malcolmson showed Mr. Burr some specimens of black and green obsidian obtained on the promontory, but the conditions under which they exist Mr. Burr was prevented from ascertaining.

The deposits of consolidated sea-sand occur more especially near the northern pass, towards the base of the volcanic ridges. The stratification is diagonal, and this arrangement Mr. Burr conceives to have been produced by the drifting of opposing currents. The flat line of coast on the northern part of the promontory, the author says, is evidently a raised beach, and the consolidation of the sand he assigns to the action of a tropical sun upon the calcareous materials. The stone incloses numerous shells and corals of species existing in the Arabian Sea.

METEOROLOGICAL OBSERVATIONS FOR JUNE 1841.

Chiswick.—June 1, 2. Very fine. 3, 4. Fine, with very dry air. 5. Slightly overcast. 6. Very fine: slight rain. 7. Cold and dry: showery and cold. 8, 9. Cloudy and cold. 10. Very fine. 11. Cloudy. 12. Slight rain: clear. 13. Cloudy and cold: clear at night. 14. Very fine. 15. Slight drizzle: cloudy and fine. 16, 17. Very fine. 18. Sultry: rain, with distant thunder and lightning at night. 19. Sultry: rain. 20. Very fine: rain. 21. Heavy showers, with sultry intervals. 22. Very fine: cloudy. 23. Overcast and fine: very
at noon. 24. Showery: heavy rain at night. 25. Rain. 26. Cloudy.
28. Rain. 29. Cloudy: showery. 30. Fine.—The mean tem-
was about 2° below the average.
Fine. 5, 6. Cloudy. 7. Cloudy: rain A.M. and P.M.
9. Cloudy. 10. Fine. 11—13. Cloudy. 14—17.
ck 74°. 19. Cloudy: rain early A.M.: rain P.M.
: brisk wind. 22, 23. Fine. 24. Fine: rain,
Rain. 26. Cloudy: rain P.M. 27. Fine.
thunder and lightning P.M. 30. Fine.
1. Clear and warm. 2. Bright and
co 5. Cloudy: rain P.M. 6. Fine but
clou hering. 9—11. Dry and cool.
12. D 14. Slight showers. 15,
16. Dry y. 18. Fine rain and
thunder. 21. Heavy showers.
Fair all : thunder. 25. Wet
all day r till 4 o'clock: rain.
ery all Rain P.M.
out days. Hail 1 day.
2 day day. East 3½ days.
South-south-west
4 1 day. North-
breeze 9 days.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTON; *by* Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. VEALL *at Boston, and by* Mr. DUNBAR *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. June.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8¾ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	30·214	30·137	30·122	29·50	29·98	30·00	64·3	80·5	56·2	72	47	65	62	51	S.	NE.	W.	W.	...	...	...	...	61
2.	30·272	30·177	30·167	29·55	29·98	30·08	65·0	82·3	55·3	75	53	66	56	41½	S.	W.	W.	W.	...	...	...	...	62
3.	30·250	30·254	30·157	29·62	29·98	30·05	63·0	73·4	56·5	73	41	63	55	38½	N.	W.	W.	SW.	...	...	...	...	59
○ 4.	30·440	30·357	30·300	29·79	30·15	30·11	62·7	73·6	52·2	72	46	57	56	47½	W.	NW.	NW.	SW.	...	...	...	...	53
5.	30·250	30·210	30·004	29·65	30·00	29·92	62·2	73·6	52·0	73	46	58	58	48	W.	W.	W.	SW.	...	·03	...	...	54
6.	30·036	29·981	29·923	29·55	29·95	29·92	55·3	69·0	51·3	59	40	51·5	61	40½	WNW.	NE.	NW.	N.	·019	·01	...	...	54
7.	29·964	29·956	29·921	29·40	30·00	30·04	51·3	60·0	45·3	57	46	49	56½	41	NW.	N.	NW.	N.	...	·02	...	0·06	50
8.	29·996	29·974	29·958	29·48	30·09	30·04	51·0	56·4	47·6	56	46	51·5	59½	43½	NW.	N.	N.	NE.	...	...	·25	...	51
9.	29·970	29·947	29·911	29·47	30·01	29·95	50·3	57·2	47·4	57	39	51·5	61	44	NNW.	NE.	N.	NE.	...	...	...	...	47
10.	29·838	29·815	29·669	29·27	29·87	29·69	52·3	57·6	47·8	71	41	57	76	50	NW.	N.	N.	NW.	...	...	...	...	47
11.	29·722	29·775	29·687	29·22	29·74	29·84	50·5	68·7	49·3	54	45	50	59	46½	NW.	N.	N.	NE.	...	...	...	...	50
☾ 12.	29·850	29·973	29·807	29·37	29·89	29·88	50·7	54·7	46·8	53	37	50	63½	41½	N.	NE.	N.	SW.	...	·01	...	...	47
13.	30·064	30·040	29·944	29·53	30·00	29·96	50·2	55·2	47·0	65	37	57	61	38	NW.	NW.	calm	NW.	...	...	...	...	47
14.	30·100	30·047	29·906	29·47	29·93	29·78	57·3	72·6	46·7	72	53	59·5	54	39	W.	NW.	calm	W.	...	...	...	0·04	48
15.	29·940	30·097	29·895	29·25	29·81	30·00	57·4	66·0	55·6	69	36	57	61	47½	W.	NW.	W.	W & E.	...	·01	...	...	53
16.	30·274	30·208	30·111	29·65	30·02	29·97	60·5	67·8	47·7	72	41	59	61½	44	S.	S.	calm	SW&S.	...	...	...	...	50
17.	30·136	30·079	29·875	29·39	29·91	29·83	61·7	80·3	51·5	70	41	66	61	52½	S.	S.	calm	S.	...	...	...	...	56
18.	29·822	29·788	29·605	29·18	29·70	29·61	64·3	76·4	53·6	80	54	65	59½	53½	NNW.	S.	calm	SE.	...	·24	...	...	56
● 19.	29·650	29·689	29·607	29·05	29·58	29·58	60·7	75·0	58·2	69	45	60	63	51½	W.	W.	calm	E.	·111	·08	·38	...	59
20.	29·842	29·759	29·724	29·22	29·58	29·40	64·3	87·0	51·2	66	54	63	65½	52	S.	SW.	calm	E.	·027	·02	·08	...	57
21.	29·848	29·959	29·769	29·18	29·42	29·62	64·2	71·0	56·3	71	47	65	61	52½	S var.	SW.	W.	SSE.	...	·26	·06	0·88	57
22.	30·118	30·059	29·994	29·50	29·78	29·80	61·7	70·2	53·7	73	46	64	61	49½	S.	W.	W.	SSE.	·366	·01	...	...	56
23.	30·030	30·028	29·812	29·42	29·81	29·75	62·0	69·7	53·4	73	46	64	64	50	S.	S.	calm	SSW.	...	·40	...	...	56
24.	29·800	29·741	29·639	29·25	29·70	29·68	63·0	69·4	53·8	72	54	66	67	46	E.	W.	calm	SE.	...	·49	...	...	55
☽ 25.	29·542	29·543	29·494	29·10	29·61	29·50	55·7	68·4	56·2	70	54	57	54	47	E.	SW.	E.	E & SE.	·525	·04	1·08	...	58
26.	29·686	29·774	29·605	29·10	29·43	29·51	64·0	78·0	56·2	67	52	64	66	48½	S.	SW.	S.	S.	·133	...	·32	...	59
27.	30·012	30·128	29·951	29·38	29·73	29·89	62·5	67·6	56·4	70	52	67	64	49½	W.	W.	W.	WNW.	...	·10	·34	...	59
28.	30·012	29·981	29·773	29·44	29·65	29·60	58·3	68·7	55·3	62	50	58	57	51	S.	S.	W.	S.	·430	·65	·01	1·00	58
29.	29·820	29·817	29·743	29·22	29·58	29·70	61·3	67·4	52·8	69	48	63	64	47½	S.	SW.	W.	E.	·675	·07	·30	...	57
30.	30·046	30·093	29·927	29·43	29·88	29·86	59·7	73·6	52·7	67	52	63	60	50½	W.	NW.	calm	W.	·022	·01	·23	...	56
Mean.	29·985	29·979	29·866	29·38	29·825	29·818	58·9	69·7	52·2	67·30	49·56	59·5	61	47·4					Sum. 2·308	2·45	3·05	1·98	Mean.

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

SEPTEMBER 1841.

XXVII. *On the Form and Optical Constants of Anhydrite. By* W. H. Miller, *M.A., F.R.S., Professor of Mineralogy in the University of Cambridge*[*].

THE following values of the angles between normals to the faces of anhydrite were obtained from the measurements of a single crystal,—the only one exhibiting any forms, except those of cleavage, which I have been able to procure. The faces being extremely dull, the only signal that could be used in observing with the reflective goniometer was a small opening in a screen, through which sun-light was thrown by reflexion from a plane mirror. The results obtained under such circumstances cannot be very accurate; they are, however, certainly much nearer the truth than those which Haüy has given, and which appear to have been adopted by all succeeding mineralogical writers.

The most perfect cleavages are parallel to the faces *m*, *t*; a cleavage a little less perfect than the preceding exists parallel to the face *p*, which is usually rough; and cleavages still less perfect may be obtained parallel to the faces *r*. The face *s* is common to the zones *m t*, *o p*.

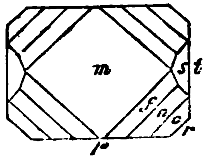
m t	90° 0′	*t f*	72° 56′
t p	90 0	*p o*	51 51
m p	90 0	*p n*	63 43
p s	90 0	*p f*	70 47
t r	48 18	*m o*	55 50
r p	41 42	*m n*	36 23
t o	56 36	*m f*	26 10
t n	66 45	*m s*	44 25

---

[*] Communicated by the Author.

The symbols of the simple forms are,—

t {100}, *r* {110},
p {010}, *o* {111},
m {001}, *n* {112},
s {101}, *f* {113}.



The optic axes lie in a plane parallel to the face *m*, and make angles of 21° 46′ with a normal to *p*. The ratios of the velocity of light in air to its velocity within the crystal are 1·571, 1·576, 1·614 for rays in planes parallel to *t*, *m*, *p*, polarized in those planes respectively.

St. John's College, July 30, 1841. W. H. MILLER.

XXVIII. *Abstract of recent Researches on the quantity of Heat evolved in Chemical Combination, particularly those of* MM. Dulong, Hess *and* Andrews.

[Continued from p. 25, and concluded.]

THE law described at the conclusion of the former portion of this abstract was founded on the following evidence, the apparatus used by M. Hess being the same employed in the experiments by which the first law was found. Sulphuric acid of various degrees of concentration, but of definite atomic constitution, was mixed with a slight excess of water of ammonia (sp. gr. 0·935), and the following results obtained:—

Oil of vitriol $HO + SO_3$ gave 595·8 units of caloric.
Acid of ... $2HO + SO_3$ ... 518·9 ...
... ... $3HO + SO_3$ ... 480·5 ...
... ... $6HO + SO_3$ ... 446·2 ...

As the experiment was not made with anhydrous acid, it need not be taken into account, and therefore commencing with oil of vitriol, and adding to the quantity of heat evolved by the ammonia in combining with each dilute acid, the quantity of heat which had been previously evolved by the oil of vitriol, therein, to form the dilute acid, the following numbers are obtained:—

Acid, of strength	Had evolved in combining with Water.	Evolves in combining with Ammonia.	Total.
$HO + SO_3$	.....	595·8	595·8
$2HO + SO_3$	77·8	518·9	596·7
$3HO + SO_3$	116·7	480·5	597·2
$6HO + SO_3$	155·6	446·2	601·8

giving a mean of 597·9. But since real sulphuric acid evolves in combining with water 510·1 units of heat, and since acid of $6HO + SO_3$ would still evolve with water 38·9, the total quantity of heat which would have been evolved by the mixture of dry sulphuric acid with water of ammonia would be $510·1 - 38·9 + 597·9 = 1069·1$.

The second series of experiments was executed with potash, and gave the following results. M. Hess was obliged, however, to modify the mode of working. In consequence of potash combining with water in various proportions, and evolving thereby different quantities of heat, it was necessary to use the solution of potash so weak as not to be affected by any further addition of water; and the mass of materials became then so large as to preclude the use of the calorimeter, which had hitherto served so well. The method of mixtures was therefore adopted. The vessel in which the mixture was effected was nearly globular in form, and about a litre in capacity; it weighed 290 grammes, equivalent to fifty-five grammes of water. The water was first introduced, and then so much solution of potash as was necessary for neutralizing a certain quantity of acid. The temperature of the liquid was observed immediately on the addition of the potash, and the rise of temperature on the addition of the acid. All these fluids were measured, and when the temperature had fallen to the standard, the specific gravity was taken which gave the weight. The specific heat of the solution was found to be 0·943.

Sulphuric acid $6HO + SO_3$ gave 445·4 units of heat.
... $3HO + SO_3$ ... 483·4 ...
... $2HO + SO_3$ ... 527·1 ...
... HO, SO_3 ... 597·2 ...

By establishing the same comparison here as in the case of ammonia, we find that

Acid, of strength	Had evolved in combining with Water.	Evolves in combining with Potash.	Total.
$HO + SO_3$	.....	597·2	597·2
$2HO + SO_3$	77·8	527·1	604·9
$3HO + SO_3$	116·7	483·4	600·1
$6HO + SO_3$	155·6	445·4	601·0

giving a mean of 601·0.

The experiments with soda were carried on in the calorimeter, and gave with $HO + SO_3$, 608·7 units of heat; and with $6HO + SO_3$, 447·4 units. But $447·4 + 155·6 = 603·0$.

M. Hess remarks that, as these numbers are sensibly the same as those obtained for potash, the rise of temperature, which occurs when $Na\,O + S\,O_3$ is mixed with $10\,H\,O$, does not take place when the solutions are mixed together; and hence infers, that the $Na\,O$ is already combined with at least 10 Aq.

For carrying on the experiments with lime, some additional circumstances required to be observed. The lime was weighed dry, and then put into a bottle with 700 grammes of water, when it slaked and evolved heat. After having waited until the temperature had fallen to that of the surrounding air, the acid was added. In this, as in all other experiments of this kind, the alkaline substance was left slightly in excess. After mixture the elements are evidently differently arranged from before, and consequently M. Hess always determined the specific heat of the mixture by a direct experiment when it was liquid; but as this could not be done in the case of lime, it was necessary to determine the composition of the mixture, and the specific heat of each of its ingredients.

The numerical results obtained were,—

Sulphuric acid $6\,H\,O + S\,O_3$ gave 481·8 units of heat.
... $2\,H\,O + S\,O_3$ ... 543·5 ...
... $H\,O + S\,O_3$ ... 628·3 ...

Adding, as before, the heat evolved by the previous dilution with water, we obtain with

$H\,O + S\,O_3$ ... 628·3
$2\,H\,O + S\,O_3$ ... $77{\cdot}8 + 543{\cdot}5 = 621{\cdot}3$
$6\,H\,O + S\,O_3$ ... $155{\cdot}6 + 481{\cdot}8 = 637{\cdot}4$

These experiments were repeated in the calorimeter, and the results were,—

Sulphuric acid $6\,H\,O + S\,O_3$ gave 489·2 units of heat.
Second experiment, same acid ... 490·9 ...
Acid of $2\,H\,O + S\,O_3$ ... 564·6 ...
Second experiment, same ... 556·4 ...
Acid of $H\,O + S\,O_3$ ... 645·0 ...

Adding thereto the heat of dilution, we have,

$H\,O + S\,O_3$ .... $645 + 0 = 645$
$2\,H\,O + S\,O_3$ .... $564{\cdot}6 + 77{\cdot}8 = 642{\cdot}4$
Ditto .... $556{\cdot}4 + 77{\cdot}8 = 634{\cdot}2$
$6\,H\,O + S\,O_3$ .... $490{\cdot}9 + 155{\cdot}6 = 646{\cdot}5$
Ditto .... $489{\cdot}2 + 155{\cdot}6 = 644{\cdot}8$

The mean of these is 642·6. The mean of the former series was 628·9, which, from a number of sources of loss, is necessarily too small. But if we take the higher number, it may be asked how lime differs so much from the other bases, which give about 600. This may depend upon two things; *first*, that the two atoms of water, which gypsum contains, evolve heat, or else it could not be a chemical combination; or, *secondly*, that it may arise from the solidification of the sulphuric acid. In order to obtain some indications on this point, M. Hess made two experiments on the quantity of heat evolved by gypsum in setting, and found that for $S\,O_3$ there are evolved 36·5 to 37·7 units of heat.

If we subtract that from the 642·5, we have 604·8 for the heat evolved with oil of vitriol, or 1076·1 with anhydrous sulphuric acid.

M. Hess also ascertained that dry lime, in combining with water, evolves 167·2 units of heat.

Experiments with Muriatic Acid.

M. Hess employed the acid $H\,Cl + 12\,H\,O$, which contains 0·252 of dry acid, and has sp. gr. 1·125. This acid still evolves heat when mixed with water, and the quantity being determined, as in the case of sulphuric acid, was found to be 50·84 units of heat for one atom of muriatic acid. In calculating this result, however, the atom of sulphuric acid is still taken as unit, which makes the atom of muriatic acid 0·908.

The acid $H\,Cl + 12\,H\,O$ (previously diluted) being saturated with ammonia, gave out 318·8 units of heat; and in another trial exactly the same number was obtained.

Three experiments were made by means of the acid $H\,Cl + 12\,H\,O$, the water and the ammonia being all mixed at the same time; the heat evolved was 367·7, 364·9, and 374·7, giving a mean of 369·1. If we then take the former result, we have $318{\cdot}8 + 50{\cdot}84 = 369{\cdot}64$.

The acid $H\,Cl + 12\,H\,O$, mixed with water and potash, evolved 361·9 units of heat.

The acid $H\,Cl + 12\,H\,O$, mixed with water and soda, evolved 376·4 units in one and 360· units in another experiment.

It has been already remarked, that the general principle of the quantity of heat evolved in combustion being proportional to the quantity of oxygen absorbed, was supported, at least approximately, by the example of carbon, in forming carbonic oxide and carbonic acid. This subject has, however, given rise to some difference of interpretation of Dulong's number, given in page 20. M. Hess, who considers that carbonic oxide contains its own volume of gaseous carbon, deduced therefrom, that the quantity of heat evolved in forming carbonic oxide, was, to that evolved in burning carbonic oxide, as three to two. As, according to his view,

A vol. of carbon burning to carbonic acid evolves heat	7858
A vol. of carbonic oxide burning evolves heat . . .	3130
∴ A vol. of carbon forming a vol. of carbonic oxide evolves	4728

From this he deduced the curious result, that it would produce more heat in practice (as in smelting-works) to employ all oxygen to form carbonic oxide, than to produce carbonic acid in the proportion of six to five; and hence, that in changing carbonic acid into carbonic oxide, heat should be evolved. These views have, however, been shown to be erroneous by M. Ebelmen, who proved, by a comparison of Despretz's results with those of Dulong, that Dulong considered carbonic oxide to contain half its volume of gaseous carbon, and, consequently, that as

A vol. of carbon, burning to carbonic acid, evolves	7858 heat,
Two vols. of carbonic oxide, burning, evolves . .	6260
One vol. of carbon, in forming carbonic oxide, evolves only	1598

and hence the quantity thus employed is, to the total quantity, as one to four.

M. Hess has very ingeniously endeavoured to apply his second law to the determination of several important questions. Thus, to explain the great combustibility of gunpowder, he considers that in forming nitric acid, each atom of oxygen has evolved a certain quantity of its heat, and more in the order of its combination. Thus, if the total quantity of heat belonging to an atom of oxygen be represented by 16, then the first atom which unites with the nitrogen may be supposed to lose all; the second 8, the third 4, the fourth 2, and the fifth 1. There is thus expelled in forming nitric acid, $16 + 8 + 4 + 2 + 1 = 31$ units of heat; but the total quantity belonging to the oxygen being $16 \times 5 = 80$, there remain 49 units of heat available for the carbon and sulphur. A very important consequence from these considerations, deduced by M. Hess, is, that the heat evolved in the combustion of a compound body is, in all cases, less than the heat evolved in the combustion of its constituents; because some heat has been already eliminated in the combination of these constituents with each other. This principle M. Hess considers to derive great support from experiments made by Dr. Ure on the combustion of certain kinds of fuel and fatty matters, of which there is a notice in the Athenæum, 1839, No. 620; but it must be remarked, that the results of Dulong, as given in page 22, with Berzelius's remarks, are directly opposed to this idea.

In connexion with the subject of this abstract we have to notice a paper by Dr. Andrews of Belfast, 'On the Heat developed during the Combination of Acids and Bases,' read before the Royal Irish Academy, January 11, 1841[*].

The general conclusions at which the author arrives are contained in the two following laws:—

Law 1. The heat developed during the union of acids and bases is determined by the base, and not by the acid; the same base producing, when combined with an equivalent of different acids, nearly the same quantity of heat, but different bases a different quantity.

Law 2. When a neutral is converted into an acid salt, no change of temperature occurs.

In the commencement of the paper a preliminary experiment is described, the object of which is to determine the exact quantity of heat evolved during the combination of nitric acid and potash. The solutions, both acid and alkaline, were taken so weak in this and all the other experiments detailed in the communication, that subsequent dilution with water did not produce any change of temperature. On neutralizing the solution of caustic potash, containing 0·353 gramme of pure alkali, with nitric acid, the temperature of the resulting solution of nitrate of potash, whose weight amounted to 30 gr., was found (after all corrections had been made) to rise 6·75° F.

To illustrate law first, the author adduces tables, which show, at a glance, the heat produced when an equivalent of each base is neutralized by different acids. Thus, when the same proportion of pure potash is combined under similar circumstances with the arsenic, phosphoric, nitric, boracic, hydrochloric, hydriodic and oxalic acids, the elevations of temperature, indicated by the thermometer, vary only from 6·8° to 6·6°. Sulphuric acid produces rather a higher temperature than any other acid (7·3°), and the acetic, formic, tartaric, citric, and succinic acids, give rather less heat than those before mentioned (from 6·4° to 6·1°). In like manner, ammonia produces an increase of temperature varying from 5·7° to 5·5°, when neutralized by the nitric, hydrochloric, hydriodic, arsenic, oxalic, and acetic acids; the greatest divergence from these numbers occurring, on the one hand, with the sulphuric acid (6·3°), and on the other, with the citric, tartaric, and succinic acids (5·1°). Analogous results are described as having been obtained with other bases, such as soda, barytes, magnesia, lime, and the oxides of zinc and lead. On the contrary, the heat developed by each base is peculiar to itself; and, consequently, the same acid gives different elevations of temperature, with equivalents of different bases. To take, as an example, the nitric acid, which also produces very nearly the mean quantity of heat given by all the acids, the following numbers express the increments of temperature obtained on combining the same quantity of it with each base:—magnesia, 8·1°; lime, 7·2°; barytes, 6·9°; potash, 6·8°; soda, 6·5°; ammonia, 5·6°; oxide of zinc, 4·8°; oxide of lead, 4·2°; oxide of silver, 3·2°. The numbers for barytes, potash, soda and ammonia, are strictly comparable with one another (except a slight correction for differences in the specific heats of the solutions); but in the case of the other bases, an absorption of heat, unknown in amount, takes place in consequence of their conversion from the solid to the fluid state. Hence the numbers for these bases are all below the truth.

---

[*] From the Proceedings of the Royal Irish Academy: the paper will appear in vol. xix. Part II. of the Transactions of the Academy.

Two singular anomalies are described as occurring in the combinations of the peroxide of mercury with the hydracids, and in those of the hydrocyanic acid with the bases.

In confirmation of the second law, the author adduces a series of experiments, which prove, that during the conversion of a neutral into a supersalt no heat is produced. Thus while the normal development of heat occurs when a solution of caustic potash is neutralized by oxalic acid, the subsequent additions, first of one, and afterwards of two more atoms of the same acid, so as to convert the neutral oxalate into the binoxalate, and the latter again into the quadroxalate of potash, are not accompanied by any change of temperature in the solutions. In testing the accuracy of this law, it is necessary to select examples where all the compounds are soluble in water, otherwise the heat arising from the formation of precipitates would interfere with and complicate the result.

The second law does not extend to the case of the conversion of neutral into basic compounds,—a part of the subject which the author has carefully investigated.

XXIX. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. Francis *and* H. Croft.

[Continued from p. 49.]

Preparation of Urea.

THE usual method of preparing urea by precipitating it from urine by means of nitric acid, is attended with great expense of time and material. Prof. Liebig has recently published the following new and easy method :—Twenty-eight parts of perfectly dried ferrocyanate of potash are mixed with fourteen parts of the black oxide of manganese, both finely powdered, the mixture heated on a smooth iron plate, (not in a crucible) over a coal fire, to incipient redness, stirring frequently. The mass on cooling is washed out with cold water, and the liquid mixed with twenty and a half parts of dry sulphate of ammonia. It is advantageous to place aside the first strong ley obtained, and to dissolve the sulphate of ammonia at the common temperature in the last water, and to add this to the first concentrated solution. Usually a strong precipitate of sulphate of potash is immediately formed, from which the liquid is separated by decantation; it is then evaporated in the water bath or on some warm place, boiling being avoided, and the liquid poured off, as long as it is possible, from the deposit of sulphate of potash. It is then evaporated to dryness and treated with boiling alcohol of 80-90 per cent., which dissolves the urea, while the sulphate salts remain undissolved. In this manner nearly four ounces of perfectly colourless and beautifully crystallized urea may be obtained from a pound of the ferrocyanate of potash. It frequently happens that the solution containing the sulphate of potash and urea is coloured yellow by ferrocyanide of ammonium, or of potassium, which dissolves in the alcohol, and gives to the crystals of urea a yellowish colour; it may easily be separated by the addition of a solution of copperas, after separation of the Prussian blue carbonate of ammonia is added to the liquid, which decomposes the excess of iron salt, and the liquid becomes clear and colourless, and may then be evaporated and treated as above. (*Ann. de Chem. und Pharm.* vol. xxxviii. part 1.)

Arseniuretted Hydrogen.

It is well known that a solution of the perchloride of mercury most easily decomposes arseniuretted hydrogen, on which account it is employed not only to destroy every trace of this gas, but likewise to indicate its presence. The composition of this precipitate, which is yellow with a slight brownish tinge, and is thus distinguished from the precipitate produced by the action of perchloride of mercury on phosphuretted hydrogen, is quite unknown; Stromeyer seems to be the only person who has examined it. According to him, arseniuretted hydrogen forms, with a solution of perchloride of mercury, arsenious acid and protochloride of mercury, and, finally, an amalgam of mercury and arsenic. The precipitate decomposes by preservation in much water; it becomes black, and consists at last only of globules of mercury; the liquid above it contains hydrochloric and arsenious acids. This decomposition is perfectly similar to that which is caused by water in the precipitate produced in solutions of the perchloride of mercury by phosphuretted hydrogen, only that it is effected more rapidly. The similar decomposition of both precipitates by water, and also by dilute nitric acid, supposes a similarity in their constitution, which was confirmed by a quantitative analysis, according to which the precipitate proved to be composed according to the formula $As^2 Hg^3 + 3\, Hg\, \underline{Cl}$. This precipitate differs from that produced by phosphuretted hydrogen, by its being anhydrous, while the latter contains three atoms of water. This is the reason of their different action at a high temperature. The precipitate produced by antimoniuretted hydrogen in solutions of the perchloride of mercury, has quite a different composition to that produced by phosphuretted or arseniuretted hydrogen in such solutions. Whence we may conclude that the composition of antimoniuretted hydrogen differs from those of phosphuretted and arseniuretted hydrogen. (H. Rose in *Pogg. Ann.* li. p. 423.)

On the Salts of Lead formed by Nitrous Acid and Hyponitrous Acid.

Proust was the first to observe that lead dissolves in considerable quantity when brought into a hot solution of the nitrate of lead; the salt thus produced is deposited in the form of yellow lustrous scales. Proust concluded from this experiment that the oxide of lead was reduced to a lower degree of oxidation; however, Berzelius subsequently showed that the solution of the lead was effected at the expense of the nitric acid in the salt employed. About the same time Chevreul described two different salts produced by the action of different quantities of lead on the nitrate of lead, and arrived at nearly the same conclusions as Berzelius. Péligot has recently shown that three very distinct salts are formed by the action of lead on the nitrate of lead, two of which contain not nitrous acid[*], as supposed by Berzelius and Chevreul, but hyponitric acid[†]; accordingly the latter acid, which, according to Dulong's analyses, consists of two volumes of nitrogen and four of oxygen, would, contrary to the general opinion, be able, if not to combine directly with bases, yet at least to exist in combination with them. Proust's salt is best prepared by bringing together one equivalent of lead and one equivalent of the nitrate; the reaction proceeds to the last at a temperature below 60° or 70°, without any evolution of ni-

[*] Hyponitrous acid of the English chemists.

[†] Nitrous acid of the English chemists.

tric oxide; this gas escapes only when the yellow salt is decomposed at a higher temperature. Should this salt be mixed with any of the orange-coloured salt which will presently be noticed, it may, from its far greater solubility, be separated from it by treating the mixture with a quantity of warm water, not sufficient to dissolve the whole. The analysis consisted in the direct determination of the oxide of lead, nitrogen and water; the results agree with the formula $\underline{N}\,O^4, 2\,Pb\,O, \underline{H}\,O$. Berzelius adopts hypothetically that it contains 6·4 per cent. water; experiment affords but 3·2. The following simple equation, $\underline{N}\,O^5, Pb\,O + Pb + \underline{H}\,O = \underline{N}\,O^4, 2\,Pb\,O, \underline{H}\,O$, gives good account of the origin of the salt. The second salt is of an orange colour, and is obtained by dissolving two and a half equivalents of lead in the boiling solution of one equivalent of the nitrate of lead; on cooling, a mixture of the yellow and orange salts is obtained; the former is removed with boiling water. All the analyses of this salt give the formula $\underline{N}\,2\,O\,8, 7Pb\,O, 3\,\underline{H}\,O$. The constitution of this salt is confirmed by synthesis, for when the bibasic hyponitrate is boiled with oxide of lead, the orange-coloured salt is obtained. Continued boiling of nitrate of lead with more than two or three equivalents of lead gives Chevreul's rose-red salt, which is represented by the formula $\underline{N}\,O_3, 4\,Pb\,O, \underline{H}\,O$. (*Compt. Rend.* t. xi. p. 860.)

Deville *on Oil of Turpentine.*

M. Deville calls the oil which is contained in the artificial camphor Camphen, that in the fluid product Tereben. Tereben may be obtained by distilling oil of turpentine with sulphuric acid. The temperature must not exceed 200°, for then colophen passes over. By repeated distillation with sulphuric acid, the tereben may be obtained pure; the only proof of its purity is its having lost all power of rotation. By distilling the liquid camphor with lime it cannot be obtained pure. It has the same boiling point and specific gravity as oil of turpentine. Formula $C^{20}H^{32}$. Tereben combines with the hydrochloric, hydriodic and hydrobromic acids in two proportions. The monohydrochlorate of tereben is $C^{20}\,H^{32} + H\,Cl$; the bihydrochlorate is $C^{20}\,H^{32} + H^2\,Cl^2$. This latter was examined by Soubeiran and Capitaine; it is the liquid product obtained in preparing artificial camphor; it has probably no power of rotation. The monohydrochlorate is obtained by passing hydrochloric acid into tereben; it is liquid; specific gravity at 20° = 0·902; smells like camphor; contains four volumes of vapour of tereben, and two of hydrochloric acid.

Power of rotation = 0, which is the case with all the other compounds of this class. The monohydrobromate is a colourless fluid; specific gravity = 1·021 at 24°; obtained by acting on tereben with hydrobromic acid, and purifying by means of chalk, animal charcoal and chloride of calcium.

The bihydrobromate is obtained with the camphen compound by acting on oil of turpentine with hydrobromic acid; it is difficult to procure in a pure state; its power of rotation could not, therefore, be accurately determined. The monohydriodate is obtained in the same manner. Sp. gr. = 1·084 at 21°; decomposes by exposure to the air. The bihydriodate can only be obtained mixed with the camphen compound.

By the action of chlorine and bromine on tereben, chlorides and bromides are produced. The specific gravities are respectively, 1·360 and 1·978 at 20°. Formulæ $C^{20} H^{24} Cl^{8}$, $C^{20} H^{24} Br^{8}$; eight atoms of hydrogen have been replaced by eight atoms of chlorine or bromine. By distilling these compounds two others may be obtained, $C^{20} H^{28} Cl^{4}$, or $C^{20} H^{28} Br^{4}$. The monochloride of tereben, $C^{20} H^{28} Cl^{4}$, has a specific gravity = 1·137 at 20°.

The action of iodine appears to be more complicated.

Hydrobromate and hydriodate of camphen may be obtained in the same manner as the hydrochlorate; the former is solid; power of rotation = − 0·4264. The hydriodate is a fluid. Sp. gr. at 15° = 1·0597; power of rotation = − 0·1597. They both decompose by exposure to the air. Formulæ $C^{20} H^{32} + H^{2} Br^{2}$, and $C^{20} H^{32} + H^{2} I^{2}$.

By the action of chlorine on hydrochlorate of camphen, a fluid body is first produced, the formula of which is $C^{20} H^{24} Cl^{8}$, $Cl^{2} H^{2}$; this body then loses hydrochloric acid and forms a solid crystallized substance, chloride of camphen, $C^{20} H^{24} Cl^{8}$. Chloride of camphen has no power of rotation; sp. gr. at 8° = 1·50; melts at 110—115°. By its distillation another chloride is formed, the formula of which is probably $C^{20} H^{28} Cl^{4}$.

Oil of turpentine is decomposed by chlorine and bromine, and forms two bodies, whose formulæ are $C^{20} H^{24} Cl^{8}$, and $C^{20} H^{24} Br^{8}$. The power of rotation of the chlorine compound is (for the yellow ray) + 0·2854.

Colophen is formed, as above stated, by the action of sulphuric acid on oil of turpentine; it passes over at a temperature above 200°. Formula $C^{20} H^{32}$; specific gravity at 9° = 0·940; boils at 310—315°; sp. gr. of its vapour is 11·13.

Colophen is also formed by the rapid distillation of colophonium, but is always impure. Deville supposes that Unverdorben's pinic and sylvic acids are respectively oxides of colophen and tereben; for, by the distillation of colophonium, a product is also obtained which is probably tereben, but mixed with a large quantity of colophen. Chloride of colophen is crystallizable. Formula is probably $C^{40} H^{64} Cl^{8}$. Colophen also combines with hydrochloric acid, but the compound is decomposed by treatment with carbonate of lime, &c. &c.—(*Annales de Chimie et de Phys.*, t. lxxv. p. 37.)

On the Æthereal Oils. By Gerhardt *and* Cahours.

Æthereal oils are mixtures of two substances, a hydrocarbon and a peculiar oil containing oxygen. The hydrocarbon may be driven off by distillation, but cannot thus be obtained pure; this is only effected by means of fused potassa. By the action of this substance on the oil, the hydrocarbon is separated, and the peculiar oil, which was combined with the hydrocarbon, is converted into an acid. By thus treating Roman caraway oil (*Cuminum Cyminum*), cuminic acid is obtained; from oil of valerian (*Valeriana officinalis*) and Roman oil of camomile (*Anthemis nobilis*), valerianic acid is obtained. The hydrocarbons contained in these two last-mentioned oils are different; that from oil of valerian is true camphogen, and oxidizes in the air with great rapidity, forming camphor. The hydrocarbon from caraway oil is called cymen; the body, which is changed into an acid by means of potassa, is cuminol.

Cuminol may be obtained by distilling oil of caraway at a temperature of 200°; cymen mixed with cuminol passes over, and cuminol remains behind, which may then be distilled in an atmosphere of carbonic acid. Its formula is $C^{20} H^{24} O^{2}$; sp. gr. of the vapour 5·24, according to calculation 5·094. It is colourless, boils at 220°, absorbs oxygen and becomes acid; this change takes place more rapidly when a base is present to saturate the so-formed acid. By means of bichromate of potassa and sulphuric acid, it is also converted into cuminic acid. Aqueous chlorine acts in the same manner, also strong nitric acid. Cuminic acid is best obtained by dropping the oil into fused potassa, the mass is then dissolved in water and precipitated by nitric acid. Cuminic acid is white, crystallizable, sublimes in beautiful needles, melts at 92°, boils at 250°; formula $C^{20} H^{24} O^{4}$. By the action of potassa on cuminol, two atoms of water are decomposed, hydrogen is set free, and two atoms of oxygen taken up for the formation of cuminic acid; almost insoluble in cold water; somewhat soluble in dilute acids; easily soluble in alcohol and æther. The silver salt has the formula $C^{20} H^{22} O^{3}, Ag O$. By heating this salt a carburet of silver, $Ag C$, is obtained. Cuminic æther is obtained by saturating an alcoholic solution of cuminic acid with hydrochloric acid, heating and distilling over oxide of lead.

It is a colourless liquid, lighter than water, boils at 240°; insoluble in water, soluble in alcohol and æther. Formula $C^{20} H^{22} O^{3}, C^{4} H^{10} O$. Specific gravity of vapour = 6·65, calculated 6·583.

By the action of potassa on cuminol, without the aid of heat, a peculiar gelatinous substance is formed—cuminol-potassium (*potassio-cuminol*), $C^{20} H^{22} K O^{2}$. This is decomposed by water into cuminol and potassa; it absorbs oxygen and forms cuminate of potassa. Chloride of cuminol is obtained by acting with chlorine or cuminol in the sunlight. The chloride is a colourless fluid, but decomposes with great rapidity. Formula $C^{20} H^{22} Cl^{2} O^{2}$. Forms, by boiling with potassa, cuminate of potassa and chloride of potassium; by exposure to a moist atmosphere it also forms cuminic acid. An amide compound does not appear to exist. The action of bromine is exactly the same as that of chlorine. By distilling one part of cuminic acid with four parts of caustic baryta, a colourless oil is obtained, and carbonate of baryta remains in the retort. This oil is *cumen*; it is colourless; smells like benzin; boils at 144°; formula, $C^{18} H^{24}$; sp. gr. of the vapour = 3·96, calculated 4·12. Cumen, $C^{18} H^{24}$, is cuminic acid, minus carbonic acid, $C^{20} H^{24} O^{4} - C^{2} O^{4}$. Cumen is insoluble in water; soluble in alcohol and æther; forms, with anhydrous sulphuric acid, a peculiar compound—cumensulphuric acid. The baryta salt crystallizes in beautiful lamellæ. Formula $C^{18} H^{22}, S^{2} O^{5} + Ba O$. Retinyl (*Retinylène*) has the same formula as cumen; it boils, however, at 150°; it combines in the same manner with sulphuric acid. The baryta salt, however, forms only crystalline crusts. Formula is the same, viz. $C^{18} H^{22}, S^{2} O^{5} + Ba O$. It also differs from the cumensulphate in being insoluble in absolute alcohol.

By the distillation of one part of cinnamic acid with four parts of caustic baryta, a similar fluid, cinnamen, may be obtained. Formula, $C^{16} H^{16}$; sp. gr. of the vapour = 3·55, calculated = 3·57; boils at 140°. Bromine forms with it a crystalline compound, $C^{16} H^{16} Br^{4}$.

Cymen is obtained pure by treating the oil of caraway with fused potassa, by which means all cuminol is separated. Cymen is colourless; boils at 165°; soluble in alcohol and æther; forms, with fuming sulphuric acid, the cymensulphuric acid: forms, with nitric acid, a new acid, which is difficultly crystallizable. Formula of cymen is $C^{20} H^{28}$; sp. gr. of vapour = $\overset{\text{I}}{4\cdot59}$, $\overset{\text{II}}{4\cdot70}$, calculated 4·69.

Cymensulphate of baryta has the formula $C^{20} H^{26}, S^{2} O^{5} +$

$BaO + 2aq$. It is isomeric with Delalande's camphensulphate, &c.—(*Annales de Chim. et de Phys.* III. t. i. p. 60.)

On account of the great length of this memoir, many imperfectly examined, and not as yet analysed substances, have been passed over in this abstract without notice. Liebig's remarks, moreover, showing that the French *type* is nothing more than *radical*, could not be extracted without encroaching too much on other important papers.

On the Action of Potassa on Indigo-blue.

Fritzsché has continued his researches on Anilin, a substance which had been previously described by Unverdorben under the name of Crystallin, as has been already mentioned. (Notices, Phil. Mag., s. 3, vol. xviii. p. 280.) The action does not commence until the potassa solution is very concentrated and has been boiled some time; the boiling point must be as high as 150°. If the solution be kept at this temperature, and pure indigo-blue added from time to time, the whole is dissolved, forming a yellowish red liquid, in which, after a time, crystals are formed; it is then better to stop the operation and allow the whole to crystallize. By this operation hardly a trace of any volatile substance is formed, and no gas evolved. A small quantity of a dark-coloured substance is formed with the yellow potash salt, from which it cannot be separated; it appears to be an accidental product. If the whole crystallized mass be treated with water, this dark coloured substance causes a formation of indigo-blue, but the excess of potassa also acts on the yellow salt, and indigo-blue is precipitated. It is better therefore to saturate the greater part of the potassa in the solution with an acid. A bluish green precipitate is formed, and by filtering a golden-yellow fluid is obtained, out of which acids precipitate a voluminous flocky reddish-brown body—chrysanilic acid. The colour of this new acid is similar to that of kermes mineral. It is not at all crystalline, assumes a darker colour by drying. It is very little soluble in water, easier soluble in alcohol; from a concentrated solution in a mixture of water and alcohol it may be obtained in a crystalline state. It dissolves in alkalies with a golden-yellow colour, but care must be taken not to use an excess of alkali, for then the fluid becomes green. The metallic salts are insoluble, lead and zinc salts are of a beautiful red colour. The formula is not as yet determined; the results obtained do not agree well with one another. By dissolving chrysanilic acid in dilute sulphuric acid two new bodies are formed, one of which crystallizes out on cooling, the other remains dissolved. The soluble one has received the name of Anthranilic acid, It may be prepared in another way. The crystalline mass obtained by acting on indigo-blue with potassa is dissolved in alcohol, and allowed to stand exposed to the air until its greenish colour has passed into brown. A stream of carbonic acid is then passed through the solution in order to saturate the free alkali, and the alcohol is distilled off after the carbonate of potassa has been separated. On concentration, anthranilate of potassa crystallizes out in thin lamellæ, the mother liquor is separated by bibulous paper, the salt dissolved in as small a quantity of water as possible, and acetic acid added, which produces a crystalline precipitate of hydrated anthranilic acid. Anthranilic acid is, when pure, perfectly colourless, has a sweet taste even when in combination with alkali. If the decomposition of the potassa salt be conducted very slowly it may be obtained in scaly crystals, like benzoic acid. It is difficultly soluble in cold water, easier in boiling water. In alcohol and æther it is easily soluble. It melts at 135°, and sublimes; at a higher temperature it is decomposed. The alkaline salts are soluble, the copper, zinc and lead salts are crystalline powders. The formula of the hydrated acid is $C^{14} H^{12} N^2 O^3 + H^2 O$, that of the silver salt $C^{14} H^{12} N^2 O^3 + Ag\,O$. Hydrated anthranilic acid heated above its point of fusion is decomposed into carbonic acid and anilin, $C^{14} H^{12} N^2 O^3 + H^2 O = C^{12} H^{14} N^2 + 2\,C\,O^2$. In an experiment it was found that the carbonic acid was 31·49 per cent. of the employed acid; according to calculation it should be 31·93. By distilling an anthranilate anilin is also obtained, but the decomposition is much more complicated; carbon is separated. It has been already stated that by the action of dilute sulphuric acid on chrysanilic acid a substance is obtained which crystallizes as the fluid cools; it is bluish black, soluble in alcohol, with a purple-red colour. After drying it loses this solubility; it is easily decomposed by alkalies; indigo-blue is formed, &c. (*Journal für Praktische Chemie*, B. 23, p. 67.)

On the Products of the Decomposition of Chloride and Bichloride of Isatin.

Erdmann has continued his experiments on these highly interesting bodies, and in the following memoir the products of decomposition are examined. When a stream of sulphuretted hydrogen is passed through a solution of chloride or bichloride of isatin, or the analogous bromides, the solution becomes colourless, and a white sediment is formed, consisting of sulphur and a new body, which may be freed from the sulphur by digestion with bisulphuret of carbon. A better method is to dissolve the chlorides in hydrosulphuret of ammonium. By standing, or more rapidly by the application of heat, the solution deposits a white substance, which, if quickly washed with water, freed from air, may be obtained pure.

Chlorisatyd (chloride of isatyd?) is a white pulverulent substance. When dried at 120° assumes a reddish colour. Insoluble in cold water, but little in boiling water; soluble in boiling alcohol. Formula, $C^{16} H^{10} N^2 Cl^2 O^3$, *i. e.* chloride of isatin *plus* 2 atoms of hydrogen. It is not changed by the action of chlorine.

Bichlorisatyd (bichloride of isatyd) is perfectly similar to the protochloride. Formula $C^{16} H^{10} N^2 Cl^4 O^3$, *i. e.* bichloride of isatin *plus* 2 atoms of hydrogen.

Bibromisatyd is very similar, $C^{16} H^{10} N^2 Br^4 O^3$.

Chlorisatyd heated to 180° gives off water, in greater quantity at 200°; the remainder in the retort is of a brownish-violet colour. Chloride of isatin sublimes in crystals, and the remaining mass evidently contains more of the same, which, as well as the undecomposed chlorisatyd, may be extracted by boiling alcohol. The insoluble part is pulverulent, dirty violet-coloured, insoluble in water, alcohol and hydrochloric acid; soluble in caustic potash solution with yellow colour. The alkaline solution gives with hydrochloric acid a yellow precipitate, soluble in boiling water. The violet body is chloride of indin. The composition does not appear to be yet accurately determined. Bichloride of indin is obtained in the same manner, and is quite similar; bichlorisatyd, as well as bibromisatyd, are decomposed, however, at a lower temperature than the protochloride (chlorisatyd). Bibromide of indin is of a deep blackish red colour, somewhat soluble in alcohol. Chlorisatyd is easily soluble in a solution of caustic potash; out of the solution a salt crystallizes, which is chlorisatinate of potassa; out of the mother liquor acetic acid precipitates a yellow body, soluble in boiling water; by boiling the solution (which has been filtered from the yellow matter so obtained) with hydrochloric acid, flocks of a brownish violet substance are precipitated, precisely similar to the above-mentioned chloride of indin. In the fluid is chloride of isatin. Bichlorisatyd behaves exactly similar, only that the salt which first crystallizes out is not bichlorisatinate of potassa. Its solution gives with hydrochloric acid a yellowish white precipitate, soluble in hot water, from which it is deposited by cooling. Formula, $C^{16} H^{10} N^2 Cl^4 O^4$, *i. e.* isomeric with the bichlorisatinic acid. The potash salts of these acids are precisely similar; they differ in their decomposition by hydrochloric acid. The actions of nitric acid and ammonia on chloride and bichloride of isatin are too little examined to enable us to make any abstract. Chlorine does not appear to exert any action on chloride and bichloride of isatin, when they are suspended in water, or even under the influence of sunlight. If, however, they are dissolved in alcohol, and chlorine is passed through the solution, they are soon decomposed. A thick fluid is formed. When no more of this is produced, the operation is finished. Sal-ammoniac is also formed in large quantity. In the thick oily fluid is suspended a small quantity of shining lamellæ, which may be separated by dissolving the oily fluid in alcohol, after the whole has been freed from sal-ammoniac by means of water. This crystalline substance has received the name of chloride of anil. The alcoholic solution contains hydrochloric æther; by distillation a resinous mass remains in the retort; it still contains chloride of anil, from which it may be separated by solution in alcohol; if its solution is then distilled, and heated still further, a volatile product is formed, which sublimes in needles. This is called *chlor[illegible] chloride of indopten* (gechlortes chlorindopten). This substance, when treated with potassa, forms a new acid, *ch[illegible]retted chlorindoptenic acid* (hyperchlorindoptenic acid?), [illegible] has great similarity to the chlorindoptenic acid, but its [illegible]mula is $C^{12}\,Cl^{10}$.

Chloride of anil may be sublimed if carefully heated; [illegible] soluble in water, almost so in cold alcohol; soluble in hot alcohol. It is not changed by nitric, sulphuric and hydrochloric acids. Formula $C^6\,Cl^4\,O^2$. It dissolves in solution of potassa with a purple-red colour; on cooling brownish purple-[illegible] crystals are formed, which contain chloranilic acid. The [illegible] soluble red silver salt is $C^6\,Cl^2\,O^3 + Ag\,O$. The potash salt is $C^6\,Cl^2\,O^3,\ K\,O + H^2\,O$. It explodes on heating. The metallic salts are insoluble. On adding hydrochloric or sulphuric acid in excess to the warm solution of chloranilate of potassa, reddish grains, or sometimes yellowish-red lamellæ of chloranilic acid are precipitated. Dried at 125° it contains one atom of water, the crystals contain two atoms.—(Conclusion in our next.)—(*Journal für Praktische Chemie*, xxii. p. 257–299.)

On Catechin (Catechinic Acid) and Pyrocatechin (Pyrocatechinic Acid).

Wackenroder has examined the properties of catechin, Zwenger the properties and the chemical composition, and Hagen the constitution alone. The formulæ obtained agree neither with one another, nor with those formerly obtained by Svanberg, we need only therefore give those proposed.

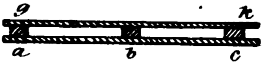


Fig. 1

Fig. 2.

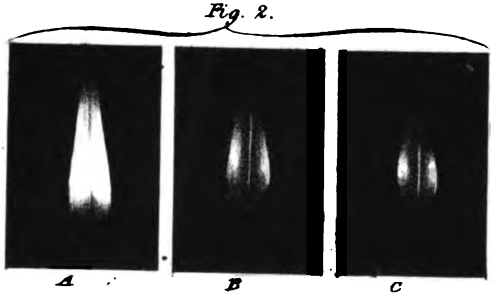


Fig. 3.

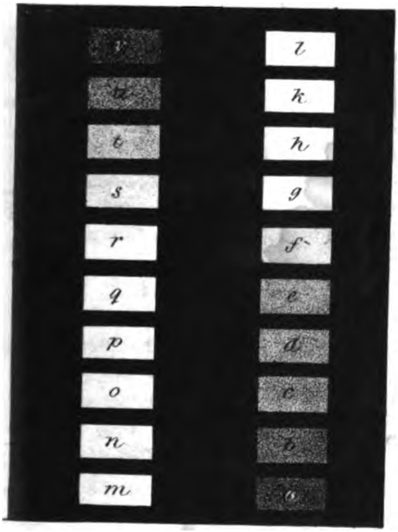


Fig. 5.

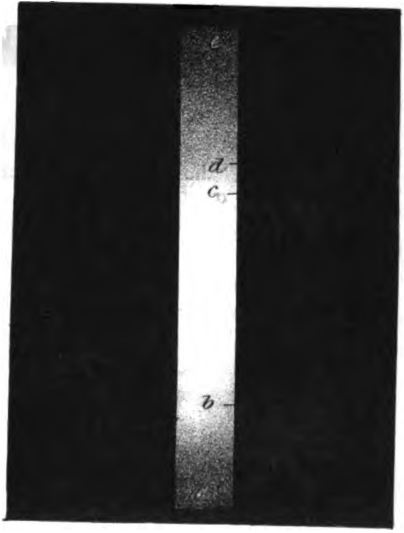


Fig. 4.

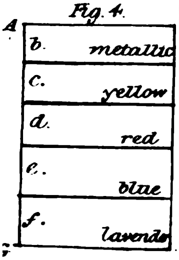
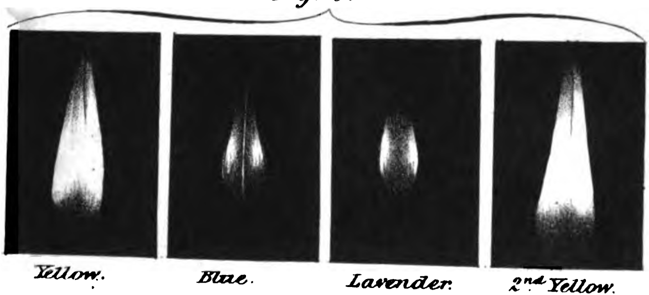


Fig. 6.



Yellow. Blue. Lavender. 2nd Yellow.

J. Basire, lith.

Professor Draper on the Analogies of Radiant Heat and the Chemical Rays.

	Svanberg.	Zwenger.	Hagen.
Catechin, dried *in vacuo*,	$C^{15} H^{12} O^6$		$C^{14} H^{18} O^9$
Dried at 100° C		$C^{20} H^{20} O^9$	$C^{14} H^{14} O^7$
Fused catechin		$C^{20} H^{18} O^8$	
Combined with oxide of lead			$C^{14} H^{12} O^6$

The pyrocatechin has been discovered and examined both by Wackenroder and Zwenger. It is obtained by heating catechin above its point of fusion; white acid vapours are evolved, and a more or less brown coloured acid fluid distils over; by evaporation the pyrocatechin may be obtained crystallized, and may then be purified by sublimation. Melts at 20° C.; easily soluble in water, alcohol and æther. The aqueous solution is easily decomposed by exposure to the air. Decomposed by alkalies and their carbonates. Formula $C^6 H^6 O^2$. In combination with oxide of lead, $C^6 H^4 O$.—(*Annalen der Chemie und Pharmacie*, xxxvii. pp. 306, 320, 336.)

XXX. *On some Analogies between the Phænomena of the Chemical Rays, and those of Radiant Heat.* By John William Draper, *M.D.*, *Professor of Chemistry in the University of New York**. [Illustrated by Plate I.]

(1.) It is the object of this memoir, to establish some striking analogies which exist between the phænomena of the chemical rays and those of radiant heat.

(2.) As most of the experimental illustrations which I shall here give depend upon the use of M. Daguerre's preparation, (though I have numerous others which serve to extend these truths to other combinations, and which will be published in due time,) I shall also incidentally give what appears to be the proper theory of the Daguerreotype.

(3.) Without saying anything of the laws of reflexion, refraction, polarization, and interference, to which these rays are subject, the study of which I commenced more than five years ago on paper rendered sensitive by the bromide of silver, further than that a general similitude holds in all these cases between the rays of heat and the chemical rays, I shall at present confine my observations to establishing the following propositions.

(4.) 1st. That the chemical action produced by the rays of light, depends upon the ABSORPTION of those rays by sensitive bodies; just as an increase of temperature is produced by the absorption of those of heat.

---

* Communicated by the Author.

(5.) 2nd. That as a body warmed by the rays of the sun gradually loses its heat by radiation, or conduction, or contact with other bodies, so likewise, by some unknown process, photographic effects produced on sensitive surfaces are only transient, and gradually disappear.

(6.) 3rd. That, as when rays of heat fall on a mass of cold ice, its temperature rises degree by degree, until it reaches 32° Fahr., and there stops, until a certain molecular change (liquefaction) is accomplished, and after that proceeds to rise again, so also the chemical rays impress certain changes proportional to their quantity, up to a certain point, and there a pause ensues;—a very large amount of light being now rendered latent or absorbed, without any indication thereof being given by the sensitive preparation (as the heat of fluidity is latent to the thermometer)—a molecular change then setting in,—the increments of the quantity of light are again indicated, by changes in the sensitive preparation.

(7.) 4th. That it depends on the CHEMICAL nature of the ponderable material what rays shall be absorbed.

(8.) 5th. That whilst the *specific rays* thus absorbed depend upon the chemical nature of the body, the *absolute amount* is regulated by its OPTICAL qualities, such as depend on the condition of its surfaces, and interior arrangement.

(9.) 6th. It will be proved from this, that the SENSITIVENESS of any given substance depends on its chemical nature and optical qualities conjointly, and that it is possible to exalt or diminish the sensitiveness of any given chemical compound, by changing the character of its optical relations. We shall here meet with an explanation of some of the facts noticed by Sir J. Herschel, Mr. Hunt, Mr. Talbot, and others, respecting the increase of sensitiveness of the chloride of silver and other bodies.

(10.) 7th. That, as when radiant heat falls on the surface of an opake body, the number of rays reflected is the complement of those that are absorbed, so in the case of a sensitive preparation, the number of rays reflected from the surface is the complement of those that are absorbed.

OF THE DAGUERREOTYPES.

(11.) In relation to the condition of these tablets, I shall prove the following facts:—

1st. That metallic mercury exists all over the surface of an ordinary Daguerreotype, in the shadows as well as in the lights,—in the shadows it is as metallic mercury, in the lights as silver amalgam.

(12.) 2nd. That in an iodized Daguerreotype, as taken from the mercury-bath, there is no order of superposition of the parts, that is to say, the iodide is neither *upon* nor *beneath* the mercury, but both are as it were in the same plane.

(13.) 3rd. That when a ray of light falls upon the surface of this preparation, through all the intervening steps, and up to the point of maximum action, no iodine is evolved from the plate, but that in the common Daguerreotype the light communicates a tendency to the atoms of the iodide to yield up to the mercurial vapour their silver, whilst the iodine retires and combines with the unaffected silver around. It follows, that when such a plate is withdrawn from the mercurial vapour, there is all over it an uniform film of iodide of silver, of the very same thickness as at first, and that this has happened through a *direct corrosion* of the silver, by the iodine, whilst it was undergoing the mercurial operation.

(14.) I pass at once to the proofs of these several propositions, commencing, for the sake of perspicuity, with those relating to the Daguerreotype first: and 1st, *That metallic mercury exists all over the surface of an ordinary Daguerreotype, in the shadows as well as in the lights,—in the shadows it is as metallic mercury, in the lights as silver amalgam.*

(15.) I took a plated copper three inches by four in surface, and having prepared it with care, I exposed half of it to the diffused light of the day, screening the other half; it was then mercurialized at 175° Fahr., the iodide removed by hyposulphite of soda and washed. And now, a plate on which a gold-leaf was spread, was placed over it, but separated, as shown in Plate I. fig. 1., in the points *a*, *b*, *c* by three slips of glass. By means of a spirit-lamp the photographic plate *a*, *b*, *c* was heated, and the gilded plate *g k* kept cool, by occasionally wetting it. On parting the plates, it was perceived that faint but distinct traces of whitening were visible all over the gold, as well on that part which was over the whitened half of the photograph, as over that which was unchanged.

(16.) But as it might happen that the mercury diffused itself laterally past the imperfect obstacle *b*, I made the following decisive trials:—

I iodized three silver plates, A, B, C, each three inches by four in surface, conducting the processes for each in the same way; and having exposed each for two minutes to a faint daylight, I laid them aside in the dark, to be presently used as test plates, in lieu of the gilded plate (*g*, *k*).

Then I took three other plates, D, E, F, of the same size, and conducting the preparatory processes for each as before, I iodized D in the dark, and mercurialized it forthwith at 170° Fahr., taking the utmost care that not a ray of light should be suffered to impinge upon it.

E was iodized, and exposed for two minutes to diffused daylight, and then mercurialized at 170° Fahr.

F was iodized, and exposed to the sun until it began to turn brown, an effect occurring almost at once. It was then mercurialized at 170° Fahr.

All these plates, then, had their sensitive coating removed by hyposulphite, and were thoroughly washed in distilled water and dried.

(17.) I had therefore three plates, representing accurately the conditions proposed to be investigated. D was in the condition of the most perfect shadows, E in that of the highest lights, and F solarized. In appearance D was black, E was white, and F bluish-gray.

Upon D, E, F, I placed A, B, C respectively, separating each pair of plates one-sixteenth of an inch, or thereabouts, by slips of glass. Then I laid them on the level surface of the sand-bath, the test plates being kept cool by sponging occasionally with water. Temperature of the sand 200° Fahr., duration of the experiment fifteen minutes.

On examination, A, B, C were all found powerfully mercurialized, nor did there seem to be any difference between them.

(18.) I consider, therefore, that the shadows, the demitints, the lights, and the solarized portions of a Daguerreotype, are covered with mercury; for at a temperature of 200° Fahr. they all evolve it alike, a sufficiency of vapour rising from the parts that have not been exposed to the light, to bring a plate that has been so exposed to its maximum of whiteness*.

(19.) In a former Number of this Journal (s. 3. vol. xvii. p. 218), I described a remarkable effect which I had noticed in these investigations, that if an object such as a wafer be laid upon a piece of cold glass or metal, and you breathe once on it, and as soon as the moisture has disappeared remove the object, and breathe again on the glass, a spectral image of the wafer will make its appearance. The impression thus communicated to the surface, under certain conditions, remains there for a long time. During the cold weather last winter, I produced such an image on the mirror of my heliostate; it could be revived by breathing on the metal many weeks afterwards, nor did it finally disappear until the end of several months.

---

* I believe that the most delicate test for the presence of mercury, is a slip of silver iodized to a yellow colour, and exposed for two or three minutes to a weak daylight.

(20.) I do not at present know what is the reason of this result, but the analogy between it and the arrangement of mercurial globules, which cover the surface of a Daguerreotype, is too striking to be overlooked. It proves to us that surfaces may assume such a condition as to affect the deposition of vapours upon them, so as to give rise to the reproduction of appearances of external forms. I gave therefore particular attention to this point, but eventually found that silver exists in an ordinary Daguerreotype, in connexion with the mercury all over the plate, in a less proportion in the shadows, and in a greater proportion in the lights. This result was, however, only obtained after the following fact was discovered—that the mucilage of gum-arabic, when slowly dried in a thin layer on the surface of a Daguerreotype, splits up in shivers, bringing along with it the white portions of the picture, and leaving the plate clean.

(21.) Having therefore prepared three plates, D, E, F, exactly as before (16.), I poured on them a solution of gum, drained them so as to leave only a small quantity, and let them dry slowly over the sand-bath. The gum separated readily, and lay in chips on the surface of each plate; it was easily removed to three sheets of paper, by tapping with the finger on the back of the plate. Each was then treated alike as follows:—

(22.) The gummy matter was incinerated on a platinum leaf, and the remaining ashes transferred to a test tube, half an inch in diameter. One drop of nitric acid and one drop of water were added; it was boiled over a small flame, and diluted with a little water. Dilute muriatic acid was now added, and the chloride of silver immediately fell. In repeating this, it is necessary to attend to the state of dilution of the acid, for if too strong it wholly dissolves the minute quantity of chloride of silver generated.

(23.) As, from the minuteness of that quantity, it was impossible to obtain a direct quantitative analysis, I adopted the foregoing method, and added the dilute acid to all three tubes at the same time. In D there was a faint opalescence, in E and F a cloud; but I could not always determine whether the deposit of E or F was most copious, sometimes the one and sometimes the other appearing to have a slight advantage.

(24.) I conclude, therefore, that whilst the whole surface of the plate is coated with mercury, it exists as silver amalgam chiefly in the lights, and as uncombined mercury chiefly in the shadows, and in a mixed proportion in the demitints;

and that when a plate is solarized, both free mercury and amalgam are present.

(25.) Such is the state of surface in a Daguerreotype, *recently formed.* In the course of time, however, a great portion of the mercury that is in the shadows, and also free in the lights, evaporates away. When the picture has thus changed, the shadows are metallic silver, and the lights silver amalgam.

(26.) 2nd. *That in an iodized Daguerreotype, as taken from the mercury-bath, there is no order of superposition of the parts, that is to say, the iodide is neither upon nor beneath the mercury; but both are as it were in the same plane.*

Soon after I had ascertained the action of gum-arabic, some of it was applied to the surface of a plate, on which an impression had just been formed in the mercury-bath. This was without removing the coat of iodine. On drying it, the gum chipped up, as was expected, bringing away with it all the lights of the picture, and leaving an uniform coat of yellow iodide of silver beneath. It seems, therefore, that the film of iodide coheres more strongly to the metal plate than the amalgam; and further, from this result we should judge that the amalgam is *on the surface* of the iodide.

(27.) But this is not true; for on three different occasions I have found that when Russian isinglass was employed instead of gum, for purposes presently to be related (34.), the isinglass, from its stronger cohesive power, chipped off in the act of drying, tearing up the yellow film from end to end of the plate, and leaving the amalgam constituting the lights undisturbed. It is here to be understood that this action takes place without the *smallest disturbance* of the lights and demitints, the plate remaining in all the beauty and brilliancy and perfection that it would have had, if it had been carefully washed in hyposulphite of soda.

(28.) This is a result, however, which I cannot produce with uniformity. Most commonly the lights are torn up with the iodide. Had it occurred but once, I should still have cited it with decision, for from the very character of it, it is impossible to be mistaken, or to commit an error of judgement. It proves to us that the film of iodide may be mechanically *torn off* from the metallic surface as perfectly as it can be *dissolved off* by chemical agents,—a singular fact.

(29.) This result, therefore, proving that we can tear off the film of iodide and leave the amalgam, can only be coordinated with that (26.) by gum water, in which the amalgam is removed and the iodide left, by supposing that there is not anything like a direct superposition in the case, and that the particles of amalgam and iodide lie as it were side by side.

(30.) 3rd. *That when a ray of light falls upon the surface of this*, &c. &c.

There is no difficulty in proving this directly, and the indirect evidence is copious. If we lay a piece of paper imbued with starch on an iodized plate, and expose it to the sun, although the plate presently assumes a dark olive green colour, the starch remains uncoloured.

(31.) This dark substance is probably a subiodide of silver; the iodine therefore which has been disengaged from it not having been set free, must have necessarily united with the adjacent metallic silver,—this, for very obvious reasons, there is no difficulty in admitting.

(32.) Now, therefore, when a photogenic impression existing on the surface of a plate in an invisible state is brought out by the action of mercury vapour, we easily understand how this is effected. No iodine is ever evolved. But each atom of iodide of silver, that has been acted on by the light, yields to the attraction of the mercury its atom of silver, and the iodine thus set free unites with the metallic silver particles around it, reproducing the same yellow iodide by *a direct corrosion* of the plate: the proofs that we have of this are two in number.

(33.) 1st. Dry some mucilage of gum-arabic on a Daguerreotype, just brought from the mercury-bath; when it has split up, we perceive that the white amalgam of silver is removed, and an uniform coat of yellow iodide of silver, of the very same thickness as at first, as is proved by its colour, is left.

(34.) 2ndly. Dry upon the same plate a solution of Russian isinglass, and when it has split up, it will be seen that it uniformly rends off with it the yellow iodide, leaving the metallic plate with an exquisite polish; and wherever the light has touched, *there it is corroded.*

(35.) These two facts, taken together, prove that in mercurializing a plate no iodine is evolved, but that a new film of iodide of the same thickness is formed, at the expense of the metallic surface.

(36.) From these facts we readily gather, that on the presence of the metallic silver the sensitiveness of this preparation mainly depends, for to the tendency which the light has impressed on the elements of the iodide to separate, is added the strong attraction of metallic silver for nascent iodine.

(37.) This corrosion or biting in of the silver plates, by the conjoint action of the mercury and iodine, gives rise to etchings that have an inexpressible charm. Could any plan be hit upon of forcing the iodine to continue its action, the problem of producing *engraved* Daguerreotypes would be solved. By another process, which will be described hereafter, I have succeeded in producing deep etchings from Daguerreotypes.

(38.) I now commence with the proofs of the leading propositions set out with in this communication.

And 1st, *That the chemical action produced by the rays of light, depends upon the* ABSORPTION *of those rays by sensitive bodies*, &c.

Without embarrassing myself here with any considerations of the tints of thin plates, or the colours of natural objects, I shall use the term absorption as expressive of a loss of radiant matter, whether that loss arises from a direct union of the luminous molecules with ponderable matter, or is rather a disappearance of effect, caused by the interference of systems of undulations.

I iodized a plate to a golden yellow colour, and exposed it to the diffused light of day, setting it in such a position that it reflected specularly, the light falling upon it through the window, to the objective of a camera-obscura, which formed an image of it upon a second sensitive plate. The beams falling upon the sensitive plate, of course exerted their usual influence upon the iodide, which, after the lapse of a short time, began to turn brown. As soon as this effect was observed, I closed the aperture of the camera, and taking out its plate, mercurialized it, but it was found that the rays reflected from the sensitive plate, although they had been converged by a lens four inches in diameter, and formed a very bright image, *had lost the quality of changing the iodide of silver*.

(39.) We see, therefore, that a ray of light which has impinged on the surface of yellow iodide of silver, has lost the quality of causing any further change on a second similar plate on which it may fall.

(40.) In the practice of photogenic drawing, this observation is of much importance, especially when lenses having large apertures are used; the rays which converge upon the sensitive plate are reflected by it in all directions, and the camera is full of light; its sides reflect back again in all directions, on the surface of the plate, these rays, which, if they were effective, must stain the plate in the shadows. But if the plate has been iodized to the proper tint, this light is wholly without action, and hence the proof comes out neat and clean.

(41.) Upon an iodized plate I received a solar spectrum formed by a flint-glass prism, the ray being kept motionless by reflexion from a heliostat, and the plate so arranged as to receive the refracted rays perpendicularly. After five minutes it was mercurialized, and the resulting proof exhibited the place of the more refrangible colours in the most brilliant hues. The lesser refrangible colours had also left their impress of a whitish aspect, but the region of the yellow was unaltered. All the different rays, therefore, except the yellow, have the power of changing this particular preparation. Now, when a number of pieces of cloth of different colours are placed in the sun-beam, they absorb heat in proportion as their colour is deeper. A black cloth, which does not reflect any of those calorific rays, becomes presently *hot*; and in the same way Daguerre's sensitive preparation absorbs all the rays which have any chemical action on it, and reflects the yellow only, which does not affect it. In this particular lies the secret of its vast sensitiveness, compared with the common preparations of the chloride and bromide of silver.

(42.) 2nd. *That as a body warmed by the rays of the sun*, &c.

After a beam of light has made its impression on the iodide, if the plate be laid aside in the dark before mercurializing, that impression decays away with more or less rapidity; first the faint lights disappear, then those that are stronger.

Having brought three plates to the same condition of iodization, and received the image of a gas-flame in the camera on each for three minutes, I mercurialized one, A, forthwith; the second, B, I kept an hour, the third, C, forty-eight hours; the relative appearance of these three images is represented in fig. 2.

(43.) Those who are in the habit of taking Daguerreotypes, know how much they suffer when the process of mercurialization is deferred. To show this effect in the extreme, I took four plates, and having prepared all alike, I exposed half of the surface of each to a bright sky for eight seconds.

No. 1. mercurialized	immediately,	came out,	black solarized.
2.	in five hours,	... ...	white.
3.	twenty-two hours,	...	same effect.
4.	one hundred and forty-four,		no effect.

(44.) This last plate, on being submitted twice more to the vapour of mercury, gave an indistinct mark. On exposing a corner of it to the sun it blackened instantly, these results showing that the peculiar condition brought on by the action of the light gradually disappears, the compound all the time retaining its sensitiveness.

(45.) Similar results are mentioned by Daguerre in the case of the changes produced on surfaces of resinous bodies, and I have noticed them in a variety of other cases. Now to whatever cause these phænomena are due, whether to any thing analogous to radiation, conduction, &c., it is most active during the first moment after the light has exerted its agency, but it must also take effect even at the very time of exposure; and it is for these reasons that it comes to pass, that when light of a double intensity is thrown upon a metallic plate the time required to produce a given effect is less than one half.

(46.) I could conceive the intensity of a ray so adjusted, that in falling upon a given sensitive preparation, the loss from this cause, this casting off of the active agent, should exactly balance the primitive effect, and hence no observable change result. Hereafter we shall find, that one cause of the non-sensitiveness of a number of bodies is to be traced directly to the circumstance, that they yield up these rays as fast as they receive them.

(47.) It needs no other observation than a critical examination of the sharp lines of a Daguerreotype proof with a magnifying glass, to show that the influence of the chemical rays is not propagated laterally on the yellow iodide of silver. Of the manifestations which these rays may exhibit, after they have lost their radiant form and become absorbed, we know but little. If they conform to the analogous laws for heat, and if the absorbing action of bodies for this agent is inversely as their conducting power, we perceive at once *why* a photographic effect, produced on yellow iodide of silver, retains the utmost sharpness without any lateral spreading; the absorbing power is almost perfect, the conducting should therefore be zero.

(48.) 3rd. *That, as when rays of heat fall on a mass of cold ice*, &c. &c.

Although in the sun the iodide of silver blackens at once, this is only the result of a series of preliminary operations.

When we look at a Daguerreotype, we are struck with the remarkable gradation of tint, and we naturally infer that the amount of whitening induced by mercurialization, is in direct proportion to the amount of incident light; otherwise it would hardly seem that the gradation of tones could be so perfect.

(49.) But in truth it is not so. When the rays begin to act on it, the iodide commences changing, and is capable of being whitened by mercury. Step by step this process goes on, an increased whiteness resulting from the prolonged action or increased brilliancy of the light, until a certain point is gained, and now the iodide of silver apparently undergoes no further visible change; but another point being gained, it begins to assume, when mercurialized, a pale blue tint, becoming deeper and deeper, until it at last assumes the brilliant blue of a watch-spring. This incipient blueness goes under the technical name of solarization.

(50.) The successful practice of the art of Daguerreotyping, therefore, depends on limiting the action of the sun-ray to the first moments of change in the iodide; for if the exposure be continued too long, the high lights become stationary, whilst the shadows increase unduly in whiteness, and all this happens long before solarization sets in.

(51.) Let us examine this important phænomenon more minutely. Having carefully cleaned and iodized a silver plate three inches by four in size, it is to be kept in the dark an hour or two.

By a suitable set of tin-foil screens, rectangular portions of its surface, half an inch by one-eighth, are to be exposed at a constant distance to the rays of an Argand gas-burner (the one I have used is a common twelve-holed burner), the first portion being exposed fifteen seconds, the second thirty seconds, the third forty-five seconds, the fourth sixty seconds, &c. &c.

We have thus a series of discs or spaces upon the plate, *a*, *b*, *c*, *d*, fig. 3, each of which has been affected by known quantities of light; *b* being affected twice as much as *a*, having received a double quantity of light; *c* thrice as much as *a*, having received a triple quantity, &c. &c.

The plate now is exposed to the vapour of mercury at 170° Fahr. for ten minutes; the spaces or discs all come out in their proper order, and nothing remains but to remove the iodine.

(52.) An examination of one of these plates thus prepared, shows us* that, commencing with the first space *a*, we discover a gradual increase of whitening effect until we reach the seventh; that a perfect whiteness is there attained; that, passing on to the sixteenth, no increase of whitening is to be perceived, although the quantities of light that have been incident and absorbed, have been continually increasing; but as soon as the light thus latent has reached a certain quantity, visible decomposition sets in, indicated by a blueness, and the sensitive surface once more renders evident the increments of incident light.

---

* It is impossible to represent these changes in a drawing, which is simply black and white; it will be understood that the characteristic distinction of the spaces from the sixteenth to the twentieth, for example, depends on their assuming a *blue* tint, which continually deepens in intensity.

(53.) Or, by presenting a plate covered with a screen to a sky that is clear or uniformly obscured, and with a regular motion, withdrawing the screen deliberately from one end to the other, and then suddenly screening the whole; it is plain that those parts first uncovered will have received the greatest quantity of light, and the others less and less. On mercurializing, it will be seen that a stain will be evolved on the plate, as is represented in fig. 5; from *a* to *b* the changes have been successive; from *b* to *c* no variation in the amount of whitening is perceptible; at *d* solarization is commencing, which becomes deeper and deeper to the end, *e*, of the stain.

(54.) The plate from which the drawing of fig. 5 is taken, gives from *a* to *b* ten parts, from *b* to *c* seventeen parts, from *d* to *e* twelve parts; we perceive therefore how large an amount of light is absorbed, and its effects rendered latent, between the maximum of whiteness being gained, and solarization setting in.

(55.) 4th. *That it depends on the* CHEMICAL *nature of the ponderable material what rays shall be absorbed.*

I had prepared a number of observations in proof of this, very much of the same kind as those which have some time ago been published in the Phil. Trans. by Sir J. Herschel. These refer chiefly to the variable lengths of the stains, impressed by the prismatic solar spectrum on different chemical bodies, and the points of maximum action noticed in them. For the present I content myself with referring to that excellent memoir for proofs substantiating this proposition.

(56.) 5th. That whilst the specific rays thus absorbed depend upon the chemical nature of the body, the absolute amount is regulated by its OPTICAL QUALITIES, such as depend on the condition of its surfaces, and interior arrangement.

I took a polished silver plate, and having exposed it to the vapour of iodine, found that it passed through the following changes of colour:—1st, lemon yellow: 2nd, golden yellow: 3rd, reddish yellow: 4th, blue: 5th, lavender: 6th, metallic: 7th, yellow: 8th, reddish: 9th, green, &c. &c. the differences of colour being produced by the differences of thickness in the film of iodide, and not by any difference of chemical quality.

(57.) It is a common remark, originally made by M. Daguerre, that of these different tints that marked 2 is the most sensitive, and photogenic draughtsmen generally suppose that the others are less efficient from the circumstance of the film of iodide being too thick. Some suppose, indeed, that the first yellow alone is sensitive to light. We shall see in a few moments that this is very far from being the case.

(58.) Having brought nine different plates to the different colours just indicated, I received on each the image of an uniform gas-flame in the camera, treating all as nearly alike as the case permitted. I readily found that in No. 1 there was a well-marked action, No. 2 still stronger, but that the rays had less and less influence down to No. 6, in which they appeared to be almost without action; but in No. 7 they had recovered their original power, being as energetic as in No. 2, and from that declining again; this is shown in fig. 6.

(59.) Hence we see, that the sensitiveness of the iodide of silver is by no means constant; that it observes periodical changes which depend on the optical qualities of the film, and not on its chemical composition; and that by bringing the iodide into those circumstances that it reflects the blue rays we greatly reduce its sensitiveness, and still more so when we adjust its thickness so as to give it a gray metallic aspect. But the moment we go beyond this, and restore by an *increased* thickness *its original colour*, *we restore also its sensitiveness.* Here then, in this remarkable result, we again perceive a corroboration of our first proposition.

(60.) I may, however, observe in passing, that although I am describing these actions as if there was an actual absorption of the rays, and that films on metallic plates exhibit colours, not through any mechanism like interference, but simply because they have the power of absorbing this or that ray, there is no difficulty in translating these observations into the language of that hypothesis. When the diffracted fringes given by a hair or wire in a cone of diverging light are received on these plates, corresponding marks are obtained, a dark stripe occupying the place of a yellow fringe, and a white that of a blue. I found, more than four years ago, that this held in the case of bromide of silver paper, and have since verified in a more exact way with this French preparation. Similar phænomena of interference may be exhibited with the chloride of silver.

(61.) We have it therefore in our power to exalt or depress the sensitiveness of any compound, by changing its optical conditions. Until now, it has been supposed that the amount of change taking place in different bodies, by the action of the rays of light, depended wholly on their chemical constitution, and hence comparisons have been instituted, as to the relative sensitiveness of the chlorides, bromides, oxides, and iodides of silver, &c. But it seems this liability to change depends also on other principles, which, being liable to variation, the sensitiveness of a given body varies with them. Thus this very iodide of silver, when in a thin yellow film, is decomposed by the feeblest rays of a taper, and even moonlight acts with energy; yet simply by altering the thickness of its film it becomes sluggish, blackening even in the sunlight tardily, and recovers its sensitiveness again on recovering its yellow hue.

(62.) We have now no difficulty in understanding, how in the preparation of ordinary sensitive paper great variations ensue, by modifying the process slightly, and how even on a sheet which is apparently washed uniformly over, large blotches appear which are either inordinately sensitive, or not sensitive at all. If, without altering the chemical composition of a film on metallic silver, or even its mode of aggregation, such striking changes result by *difference of thickness*, how much more may we expect that the great changes in molecular condition, which apparently trivial causes must bring about on sensitive paper, should elevate or depress its capability of being acted on by light! If I mistake not, it is upon these principles that an explanation is to be given of the successful modes of preparation which Mr. Talbot and Mr. Hunt have described, and the action of the mordants of Sir John Herschel.

(63.) I therefore infer,

6th. *That the* SENSITIVENESS *of any given preparation depends on its chemical nature, and its optical qualities conjointly; and that it is possible to exalt or diminish the sensitiveness of a given compound, by changing its optical relations.*

(64.) 7th. *That, as when radiant heat falls on the surface of an opake body, the number of rays reflected is the complement of those that are absorbed, so in the case of a sensitive preparation, the number of chemical rays reflected from the surface is the complement of those that are absorbed.*

This important proposition I prove in the following way:—I take a plate, A G, fig. 4, three inches by four, and by partially screening its surface whilst in the act of iodizing, with a proper piece of flat glass, I produce upon it five transverse bands, *b*, *c*, *d*, *e*, *f*; the fifth, *f*, which has been longest exposed, is of a pale lavender colour; the fourth a bright blue; the third a red; the second a golden yellow; and the first uniodized metal; the object of this arrangement being to expose at the same time and on the same plate, a series of films of different colours and of different thickness, and to examine the action of the rays impinging on them, and the rays reflected by them.

Having prepared a second plate, B, and iodized it uniformly to a yellow, I deposit it in the camera, and now placing the first plate, A G, so that the rays coming on it through the window from the sky shall be specularly reflected to the object-glass of the camera, and the image of A G form upon B, I allow the exposure to continue until the yellow of A G is beginning to turn brown; then I shut the camera and mercurialize both plates.

In consequence of what has been said (58.), it will be readily understood, that of the bands on A G, the first one, which is the bare metal, does not whiten in the mercury vapour; the second, which is yellow, mercurializes powerfully; the third, which is red, is less affected; the fourth, which is blue, still less; and the fifth, which is lavender, hardly perceptible.

But the changes on B, which have been brought about by the rays reflected from A G, are precisely the converse; the band, which is the image of *b*, is mercurialized powerfully; that of *c* is untouched and absolutely black, *d* faintly stained, *e* whitened, and *f* mercurialized, but little less than *b*.

(65.) It follows from this, that a white stripe on B corresponds to a black one on A G, and the converse: and for the depth of tint of the intermediate stripes those of the one are perfectly complementary to the corresponding ones of the other.

By the aid of these results, we are now able to give an account of the variability of sensitiveness in photogenic preparations; the yellow iodide of silver is excessively sensitive, because it absorbs all the chemical rays that can disturb it, whilst the lavender is insensitive, because it reflects them. Under this point of view, sensitiveness therefore is directly as absorption and inversely as reflexion.

The superiority of Daguerre's preparation over common sensitive paper may now be readily understood. It absorbs all the rays that can affect it, but the chloride of silver, spread upon paper, reflects many of the active rays. The former, when placed in the camera, gives rise to no reflexions that can be injurious; the latter fills it with active light, and stains the proof all over. Hence the Daguerreotype has a sharpness and mathematical accuracy about its lines, and a depth in its shadows, which is unapproachable by the other. Moreover, the translucency of the white chloride of silver, as well as its high reflecting power, permits of particles lying out of the lines of light being affected, the luminous material becoming diffused in the paper.

The fact, therefore, that a given compound remains unchanged even in the direct rays of the sun, is no proof that light cannot decompose it; it may reflect or transmit the active rays as fast as it receives them. It results from this, that *optical forces* can control and even check the play of *chemical affinities.* Whilst thus it appears that there are points of analogy between this chemical agent and radiant heat, we must not too hastily infer that the laws which regulate the one obtain exclusively also with the other. As is well known, there are striking analogies between radiant heat and light, but there are also points of difference, the convertibility of heat of one degree of refrangibility to another does not occur with light; there are also dissimilitudes in the phænomena of radiation and its consequences. I do not doubt, that what has been communicated in this memoir, will, by the researches of others, be greatly extended; but it is not to be expected that a complete parallel can be run between radiant heat and the chemical rays, any more than between radiant heat and light.

From the phænomena of the interference of these rays, of the sensitiveness or non-sensitiveness of the *same* chemical compound being determined merely by the fact of its thickness or thinness, these, and many other similar results, *obviously* depending upon mechanical principles, it seems to me that very powerful evidence may be drawn against the materiality of light, and its entering into chemical union with ponderable atoms. Those philosophers who have endeavoured to prove the undulatory theory, will probably find in studying these subjects cogent evidence in favour of their doctrines.

XXXI. *On the Atomic Weight of Carbon. By Professors* REDTENBACHER *of Prague, and* LIEBIG *of Giessen**.

IN the analysis by combustion of organic substances which contain carbon and hydrogen, the observation has frequently been made of late years, that the weight of the elements separately found by experiment, actually exceeds the original weight of the matter submitted to combustion. In the analyses we possess of naphthalin by Mitscherlich, by Dumas, and by Woskresensky, this is particularly remarkable. One hundred parts of naphthalin gave to Mitscherlich,—

---

* Translated from the original German by Dr. J. H. Gilbert; and communicated by the Chemical Society, having been read before the Society, May 8th, 1841.

An abstract of Dr. Marchand's paper on the same subject will be found in the Proceedings of the Society, to be given in our next Number.

	1.	2.
Carbon	94·34	94·440
Hydrogen	6·26	6·225
	100·60	100·665

One hundred parts of naphthalin gave to Dumas,—

	1.	2.	3.	4.	5.
Carbon ...	94·2	94·22	94·27	94·9	94·9
Hydrogen	6·3	6·30	6·26	6·2	6·1
	100·5	100·52	100·53	101·1	101·0

And to Woskresensky, 100 parts of the same substance yielded—

	1.	2.	3.	4.	5.	6.
Carbon	94·625	94·598	95·0268	93·668	94·395	94·494
Hydrogen	6·528	6·289	5·3830	6·142	6·206	6·526
	101·153	100·897	100·4098	99·810	101·601	101·020

This constant occurrence in so many carefully conducted experiments, indicates a common source of error upon which it is dependent; it can only be attributed to two causes. One of these may be sought in the defects of the method of analysis, the other in the supposition that the products of the combustion (water and carbonic acid) have different compositions from those usually assigned to them. If indeed either water or carbonic acid contains somewhat less of hydrogen or of carbon than we at present suppose, then as we calculate from the quantities found of the former bodies, the excess in the analyses is diminished in the same proportion.

Let us suppose, for example, that carbonic acid contains only 76 carbon instead of 76·437 to 200 of oxygen, and water only 12 hydrogen instead of 12·4795 to 100 oxygen, and then we shall have no excess in any of the analyses quoted, whilst the experimental come to agree perfectly with the calculated results. Are we then entitled to make such changes in the atomic weights, proceeding as we do upon the assumed accuracy of experiments, which, from the complex nature of the apparatus, can make no claim to absolute precision; or ought we not first to compare these with other experiments, in which this source of error is entirely avoided?

A question also arises, whether *naphthalin*, a substance the atomic weight of which cannot be determined with certainty, as it enters undecomposed into no combination, is a proper substance to select as the means of determining the atomic weight of carbon or of hydrogen? That body must indeed be rejected on this account, for it is not in our power to control our analytical results from a knowledge of the weight of its atom, that is, the sum of the atomic weights of the elements composing it.

When we also consider, that the naphthalin, which in the above experiments was submitted to combustion in a glass tube with oxide of copper, is a volatile body, that it cannot be introduced into the combustion tube, with oxide of copper that is absolutely free from moisture; and bear in mind also, that, owing to the volatility of the substance, this moisture cannot previously to combustion be removed by means of exhaustion, we cannot doubt the existence of a source of error, which must increase the per-centage of hydrogen beyond that which actually existed in the substance; for, however small the quantity of this hygroscopic moisture may be, it is nevertheless always present; it is weighed with the chloride of calcium tube, and its hydrogen added to that contained in the substance.

In all analyses hitherto conducted, even those in which the whole of the hygroscopic water had been removed as nearly as possible before combustion, by means of exhaustion, it is observed that the experiment invariably gives rather more hydrogen than is indicated by calculation. This excess amounts in good analyses to from 0·1 to 0·2 per cent. It is found, however, that this, in reference to the quantity of substance employed in analysis, is not sufficient to affect the proportion of the elements, to the extent observed in the analyses of naphthalin; the excess in those analyses is however diminished, when allowance is made in the calculation for this error.

There exists therefore some other cause affecting the determination of the equal quantity of the elementary constituents of an organic substance, in such a manner, that one of them, namely, the *carbon*, when calculated from the quantity of carbonic acid obtained by combustion, amounts to more than the weight of the carbon which is contained in the matter analysed. On this account a new determination of the *atomic weight of carbon* appears to be indispensable, and we have united, in order conjointly to submit the atomic weight of carbon, as at present received, to a severe and accurate scrutiny.

It is known that two of the most distinguished natural philosophers, Biot and Arago, have, by means of the direct weighing of carbonic acid, fixed upon the number 1·519 for the specific gravity of that gas. Their experiments were repeated by Dulong and Berzelius, with whom, as regards skill and talent, conscientiousness and accuracy, no others can be compared. The two last observers found the number 1·524 for the specific gravity of carbonic acid; that obtained by De Saussure is 1·5269.

The atomic weight of carbon, as calculated from the first of these, is 75·530, and from the other, 76·437. There is no known gas more easily obtained in a pure state, or which can more easily be distinguished from a foreign body, than carbonic acid. Any admixture of atmospheric air, or of other gases, can only lower its specific gravity.

Experiments have lately been conducted by Rudberg on the dilatation of gases under the influence of heat*, from which he calculates that the coefficient of dilatation is somewhat less than was previously supposed; should these experiments be correct, the proof of which still remains to be made known, they do not influence the specific gravities of two gases as determined at the same temperature, even supposing the reduction to the normal temperature be made according to the coefficient of dilatation as hitherto received; if weighed at unequal temperatures, however, a difference is observed.

In the experiments of Dulong and Berzelius, atmospheric air was weighed at 20½° C., and carbonic acid at 18° C.; consequently the reduction of the gas to 0° C., according to the former coefficient of dilatation, gives the weight of air somewhat too high, and since this, in an equal volume, represents the *divisor*, the specific gravity of carbonic acid is estimated rather too low; in all cases, however, the differences fall within the limits of the errors of observation.

When we remember that the determinations of the specific gravities of these gases were conducted with the same balloon, the same scales and weights, and at temperatures varying very little from each other, we ought not to call in question their correctness without the strongest and most convincing reasons.

During the last twelve years, a great number of weighings of the vapours of volatile bodies very rich in carbon, have been undertaken in reference to this point by Gay-Lussac, and also by Dumas; as, for instance, those of alcohol, æther, and acetone, the results of which agree either perfectly with the specific gravity of carbon vapour, as deduced from that of carbonic acid and of oxygen, or indicate it to be somewhat higher; thus the specific gravity of æther vapour is 2·586 by experiment, and 2·580 by calculation; and that of the vapour of alcohol is 1·6133 by experiment, and 1·600 by calculation.

---

* See Taylor's Scientific Memoirs, vol. ii. pp. 507, 514, 543.

In most of the observations of Dumas, the observed specific gravity of bodies very rich in carbon, is far higher than that obtained by calculation. Thus, according to the formula $C_5\ H_4$, the specific gravity of the vapour of naphthalin is 4·4882; the experiment of Dumas, however, gives 4·528, and that of Woskresensky 4·672, from which it may with great probability be concluded, that the specific gravity of carbon vapour is rather higher than 0·42139, or 0·84279.

The above-mentioned atomic weights of carbon, which have resulted from direct experiments, are contradicted by one, which, however, we may say is quite fictitious; its adoption is based on the hypothesis that the atomic weights of simple bodies are multiples of that of hydrogen by whole numbers. The atomic weight of hydrogen is in the abstract very small, and it would be strange indeed, if this, when multiplied by whole numbers, did not in many cases give a number, within certain limits, approaching (suppose we say of one-fourth or one-eighth of its own atom) those of bodies having higher atomic weights, so that a multiple of the atomic weight of hydrogen could, without introducing an important error, be substituted for that found for other bodies. For instance, by dividing 1351·61, the atomic weight of silver, by 6·2394, the atomic weight of hydrogen, we obtain the number 216$\frac{48}{62}$, that is to say, the atomic weight of hydrogen is contained about 216·5 times in that of silver; but even if one-half the atomic weight of the former is added to, or subtracted from, that of a compound of silver, so small a variation is made in the per-centage of that metal, that in many cases 216 may be taken instead of 216·5, the difference only affecting the fourth figure. The error is so small, because the atomic weight of hydrogen is itself very small. An entire misconception of the nature of the investigation respecting chemical equivalents, has led some chemists to permit themselves the license of adding to, or retrenching from, their results so much as the amount of one-half or one-fourth of an equivalent of hydrogen, seeing that it altered very little the relation of the numbers to each other, provided it led to whole numbers, which were multiples of the equivalent of hydrogen. It is in this manner that the number 75 is arrived at as the atomic weight of carbon, that number being very nearly the multiple of the atomic weight of hydrogen, by the whole number 12.

This is doubtless an unusual mode of controlling the accuracy of an experiment, yet the numbers thus modified have been admitted by many chemists.

The fact that the atomic weights of simple bodies are very nearly the multiples of that of hydrogen by whole numbers, is in itself nothing remarkable; there are other numerical relations of this kind, which appear far more extraordinary, on the strength of which, however, no one would think of making a change in atomic weights.

Thus, if we add to the atomic weight of potassium 489·92
the atomic weight of lithium, which is . . . . 80·33

we obtain the number . . . . 570·25

and this divided by 2, gives 285·12; now this last is as near the atomic weight of sodium, which in chemical properties is a link between the two former bodies, as is the atomic weight of that body obtained by multiplying that of hydrogen by a whole number.

Again, the sum of the atomic weights of barium and of calcium, divided by 2, gives very nearly that of strontium; the sum of those of chlorine and of iodine, divided by 2, gives nearly that of bromine; and those of iron and cobalt, divided by 2, give that of manganese.

There are evidently hidden connexions in these numerical relations, with which we are not acquainted, and to take them as criteria, before they are understood, is obviously inconsistent with the true spirit of philosophy; the same must be admitted with respect to the hypothetical atomic weight of carbon, 75, for the correctness of which as yet no experience speaks. The early determination of the atomic weight of lead by Berzelius, entirely confirmed as it is by his later experiments in 1830, is in complete contradiction with the correctness of atomic weights as multiples of that of hydrogen.

His memoir on that subject is indeed highly important in reference to our own investigation, and should not be forgotten by chemists. The following is an extract *verbatim* from his memoir*.

"*Further experiments on the Atomic Weight of Lead and its Oxides.*—The reduction of a metallic oxide by means of hydrogen gas, appears so simple an experiment for determining the atomic weight of a metal, that it might be supposed the results obtained would enable us to settle the question, whether or not the atomic weight of a metal is a multiple of that of hydrogen; but the nearer we approach to absolute accuracy, the greater are the obstacles we have to overcome in arriving at such a point. Besides, the circumstance that very few bodies submitted to analysis are absolutely free from all impurity, or from the substances from the compounds of which they are separated, introduces a difficulty which is often not less than that of accurately conducting the analysis itself.

---

* Poggendorff's *Annalen*, B. xix. S. 310–315.

"I was of opinion that crystallized nitrate of lead, which is ignited in a platinum crucible until the nitric acid is entirely decomposed, would give perfectly pure oxide of lead; but when this oxide is reduced by means of hydrogen gas, and the metallic lead dissolved in nitric acid, lead-coloured scales, which prove to be metallic platinum, remain behind. It is true the quantity of this residue is very small, but if the result be depended upon up to the last figure, then even the smallest impurity should be avoided. Crucibles of gold and of silver were also employed, but these are oxidized, and combine with the oxide of lead, even when the salt is introduced in small quantities into the crucible previously heated to redness; indeed oxide of lead thus obtained, is impregnated with the foreign metal to a greater extent than when the calcination takes place in a crucible of platinum.

"This induced me to employ carbonate of lead prepared by precipitation, partly from the acetate and partly from the nitrate of lead, by means of the carbonate of soda; and in order to avoid all admixture of the carbonate of soda with the precipitate, an excess of the salt of lead was employed; but notwithstanding this precaution, and the perfect washing of the precipitate, the atomic weight obtained by means of reduction, oscillated in both cases between 1303·5 and 1306·0; and when the metallic lead was heated with pure water, the latter was found to contain carbonate of soda. Carbonate of ammonia, employed as the precipitant, is not objectionable on this ground; but, partly because, if special care be not taken, it is difficult to obtain that reagent perfectly free from all traces of the hydrochlorate and of the sulphate of ammonia; and also because, during the calcination, the hydrogen of the ammonia may reduce a small quantity of the oxide to the condition of suboxide, which, though not perceptible, nevertheless notably alters the result.

"By the following method I think I succeeded in obtaining perfectly pure oxide of lead. Nitrate of lead is heated to redness in a platinum crucible, until nothing but oxide of lead remains; this is rubbed to powder, and digested in water with twice its weight of neutral nitrate of lead for some hours; the liquid is then poured off. The oxide is by this means converted into the basic nitrate ($2\,PbO + NO_5$), which is dissolved in boiling water, the solution filtered whilst boiling hot, and then allowed to cool, when fine scaly crystals are deposited. These were collected, washed, pressed into a consistent mass, and dried. The mother liquor of these crystals contains a still more basic salt, which precipitates on mixing it with a solution of the neutral nitrate. The liquid decanted at the commencement of the process, was therefore treated in this manner in these experiments. The last precipitate, which is pulverulent, was thrown upon a filter, and allowed to drain, and, whilst yet moist, the inside of a platinum crucible was coated with it to the extent of half a line in thickness. This coating, when dry, adheres strongly to the crucible. Since this basic salt does not fuse at the temperature required completely to decompose it, there is formed in this way a quantity of oxide of lead, which it is true contains platinum when in contact with the vessel, but the inner portions are quite free from that metal. In the crucible thus lined, the basic salt is laid in single pieces, so that after the calcination is completed, the oxide can be removed without any admixture of that lining the vessel; for the crucible containing the salt is, previous to the calcination, put into a larger one, having a cover, and the whole is then placed in charcoal, and heated to redness, at which temperature the oxide does not fuse. It is very easily known when the salt is entirely decomposed, for it is first converted into minium, which appears almost black when red-hot; the smallest particle of it can therefore be plainly distinguished. After this change is completed, the heat is continued for a full half-hour, and then the crucible is removed. The oxide thus obtained is of a beautiful lemon-yellow colour, and it does not in the least adhere to the coating. It still possesses the glistening appearance of the decomposed scaly crystals. It dissolves in dilute acetic acid without changing the colour of that liquid in the smallest degree, or leaving any residue, which proves that it contains no minium. The solution, moreover, is not rendered turbid by the addition of nitrate of silver.

" When this oxide was dissolved in nitric acid, precipitated by sulphuric acid, the filtered acidulous liquid concentrated, and the excess of sulphuric acid expelled, sulphate of lead remained behind, from which water did not extract any traces of a copper salt, and it was neither coloured nor rendered turbid by caustic ammonia. The lead obtained by reducing this oxide by means of hydrogen, dissolved in nitric acid, without leaving any residue. The oxide is therefore pure.

" The oxide in masses, not in powder, is introduced into a glass bulb blown upon a barometer tube, and in this it is weighed. In order to expel all moisture, the bulb was heated over a spirit-lamp, until the oxide assumed a dark orange red colour, and a stream of dry air was then passed through it, after which it was allowed to cool. The oxide regains its lemon-yellow colour by this treatment, proving that no minium is found, for which indeed the temperature is not sufficiently high. The oxide prepared in this manner is very little hygroscopic, so that 13 to 14 grammes contain at the utmost from 1·5 to 2 millegr. of moisture.

" The hydrogen gas was evolved from distilled zinc and sulphuric acid, and was, previous to entering the bulb, conducted through a solution of oxide of lead in caustic potass, and also through a tube containing coarsely powdered hydrate of potass. At the commencement of the operation, and until about two-thirds of the oxide were reduced, the temperature was not raised so high as to make the bottom of the bulb red-hot; when this precaution is neglected beyond certain limits, a portion of the oxide of lead combines with the glass, and is not subsequently reduced. It was on this account that the oxide was introduced in masses, which only touched the glass at a few points; free access between the pieces was moreover by this method afforded to the hydrogen. The first effect of the hydrogen gas is to convert the oxide into suboxide, owing to which the masses become of a dark gray colour; their form and size are not otherwise changed, although the temperature is higher than is required for their fusion, supposing them to consist of metallic lead. As soon as the glass begins to be red-hot at the bottom, small globules of lead are seen to form, and the whole is gradually converted into fused metallic lead. Of the portions of oxide of lead which were afterwards analysed, only two were obtained by one and the same operation; all the others were separately prepared, so that a fault in the preparation of the oxide cannot introduce a constant error into all the analyses; this, however, might easily occur if the same specimen of oxide had been employed in all the different analyses.

Nos.	Ox. of Lead in grs.	Lead.	Oxygen.	Atomic Weight of Lead.	Lead.	Oxygen.
					In 100 parts.	
1.	6·6155	6·1410	0·4745	1294·202	92·8275	7·1725
2.	8·0450	7·4675	0·5775	1293·074	92·8222	7·1778
3.	13·1465	12·2045	0·9420	1295·695	92·8346	7·1654
4.	14·1830	13·1650	1·0180	1293·222	92·8224	7·1776
5.	14·4870	13·4480	1·0390	1294·315	92·8201	7·1779
6.	14·6260	13·5775	1·0485	1294·946	92·8314	7·1686
			Mean	1294·259	92·8277	7·1723

"These results, which range about between 1293 and 1296, appear to prove that the atomic weight of lead lies between those two numbers. The mean of these experiments differs so little from the number arrived at in my former ones, namely, 1294·489, that I consider it unnecessary to alter the latter.

"If the atomic weight of hydrogen is 12·5, then the atomic weight of lead, supposing it to be a multiple of that number, is exactly either 1287·5, or 1300; and if either of these numbers be the true one, it appears to me that my results must have oscillated about one of them; instead of which, they, as we see, oscillate about a number which lies precisely midway between the above-named."

It may therefore be concluded, that the fact of an atomic number being a multiple of the equivalent of hydrogen is no proof of its exactness.

There are other means of controlling and estimating anew the atomic weight of carbon. The direct method, viz. that of burning a known quantity of pure carbon, and ascertaining the quantity of carbonic acid formed, is but little fitted for the solution of the point in question, since a complex apparatus is required for collecting the gas, a circumstance which lessens the dependence to be placed in such determinations. If, indeed, it be remembered, that even in operating on several grammes of carbon, the variations generally amount to from 8 to 10 millegrammes, it will be evident that a complex apparatus offers no security for absolute accuracy. We have therefore selected a different method; an ordinary one, it is true, but which has been hitherto generally acknowledged to be the most certain and the most free from error in analytical chemistry.

We can, for instance, estimate with great certainty the atomic weights of many organic compounds, namely, numerous organic acids, by determining the proportions in which they combine with oxide of silver, or, what is the same thing, with metallic silver. These organic acids contain several atoms of carbon, combined with oxygen and hydrogen in certain proportions, which can be ascertained with great facility. It is evident, that if the formulæ of these acids are known with certainty, we obtain the sum of the weights of the atoms of carbon, by subtracting the sum of the atoms of oxygen and hydrogen which they contain from the atomic weights of their compounds, as accurately determined by means of their silver salts. The sum of the atoms of carbon thus ascertained, must, if the formerly received number for carbon be correct, be a multiple of it by a whole number, or it must indicate how far that number differs from the true one. There is no substance, the atomic weight of which we believe to be known with greater certainty, and more precisely determined, than that of silver; the continued and important applications of it by Gay-Lussac, in his assay of silver in the wet way; indeed we may say, every one of his experiments on that subject is a fresh proof of its correctness.

With respect to the atomic weight of hydrogen, there is strong reason for thinking it to be rather less than was formerly supposed. In the three last analyses of water by Berzelius and Dulong, the following numbers were obtained:—

	1.	2.	3.
Oxygen......	88·942	88·809	88·954
Hydrogen...	11·058	11·191	11·046
	100·000	100·000	100·000

Supposing the atomic weight of hydrogen to be 6·2398, then the limits of error lie between the numbers 6·3055 and 6·2085. The difference between these numbers is 0·0970, which would either increase the atomic weight of carbon from 76·437 to 76·534, or reduce it to 76·340. If we take the mean, which is 6·2398, for hydrogen, then the limits of error in the determination of the atom of carbon fall in the second decimal place. Such variations result, as we know, from errors of observation.

The method which we have selected, enables us to estimate the atomic weight of carbon in the condition in which it is contained in organic compounds; and it requires only three weighings:—first, that of the vessel in which the salt is burned; secondly, that of the silver salt; and, thirdly, that of the residual silver. In these three weighings no apparatus is changed, and they take place in one and the same porcelain vessel, of which the weight is constant. The height of the barometer and the proportion of humidity in the atmosphere, exert no influence on this experiment, and the silver which remains behind is not hygroscopic.

The only precaution in these experiments, to which the greatest attention must be directed, is, of course, the rigid purity of the salt, and consequently the entire absence of all hygroscopic water.

It is at all times a difficult task to obtain a chemical compound in a state of absolute purity; to do this in our experiments, was, however, very important, as even the most minute admixture of a foreign substance must increase the found weight of the atom of carbon.

We were soon convinced that there are but few silver salts available in determinations of this kind; most of them are obtained in the form of caseous, or pulverulent precipitates, which enter into combination with a part of the precipitant. The quantity of foreign matter which adheres to these precipitates is so small, that it does not generally render the estimations of atomic weights incorrect, but, as before observed, we are here obliged to avoid every source of fallacy. We selected from those silver salts which are perfectly crystalline, which retain no water when dried at the ordinary temperature, which are not hygroscopic, and which (and this is a character of great importance) do not sputter when heated. The salts employed should moreover leave behind no carburet of silver after calcination.

There are, as stated above, but few silver salts which are not liable to one or other of these sources of error.

The cyanide of silver, for instance, is easily obtained in a pure state, by precipitating the nitrate of that metal by means of hydrocyanic acid; it can even be obtained in large shining tables, by allowing to cool slowly a hot mixture of hydrocyanic acid, with a dilute ammoniacal solution of silver; carefully washing the crystals, thus obtained, with solution of ammonia, and drying them at 120° C. By the latter treatment, and even at the ordinary temperature, all the ammonia is got rid of, and the crystals become opake milk-white, without suffering any other change; they are nevertheless in practice not fitted to be employed in our experiment, for they do not appear to be entirely decomposed by heat.

When the cyanide of silver is first heated, it melts without evolving any gas; by increasing the temperature cyanogen is given off, and a basic cyanide is formed; at a certain point beyond this, flame is developed, nitrogen is then evolved, the flame being at the same time as it were extinguished; and fused carburet of silver, of a dull white colour, remains behind, from which the carbon cannot be expelled by subsequent calcination. It is true, the carbon burns at the surface of the carburet, leaving a stratum of pure silver, but this protects the inner part from the contact of oxygen; and when this residue is dissolved in dilute nitric acid, a net-work of pure carbon remains.

The behaviour of the benzoate of silver is very similar to that of the cyanide; it is easily obtained perfectly pure, in beautiful shining crystals, which are not hygroscopic; when this salt, to the amount of 7 to 12 grammes, is heated, it melts and is decomposed; but even after being kept at a red heat for twelve hours, a very considerable quantity of carbon is found in the residue of silver.

The oxalate of silver appeared at first to be the most applicable of any in these determinations, but it is almost impossible to obtain it anhydrous, besides which, when heated in large masses, it deflagrates in the same manner as the fulminating mercury. We could, it is true, ascertain its composition by decomposing it, by means of hydrochloric acid, but we consider it important to bring no foreign element into these determinations. In the following experiments, the *acetate*, the *tartrate*, the *racemate*, and the *malate* of silver were employed.

Acetic acid, owing to its volatility, is easily obtained quite pure. We prepared it by decomposing some brilliantly white sugar of lead, which had been frequently recrystallised, in the usual manner, by means of sulphuric acid. The acetic acid thus obtained, which was free from sulphurous acid, was once more rectified over binoxide of manganese in excess. The pure acid was then partly saturated with ammonia, and precipitated by nitrate of silver in the warm. A shining white precipitate was obtained, possessing the form of small laminæ, having a silvery lustre; this was perfectly washed on a filter, dissolved in hot water, the solution filtered, and left to cool in a glass vessel, when at the bottom, at the surface, and on the sides, broad shining needles of acetate of silver, of an inch in length, were formed; these were again washed with pure water, dried in the air, finely powdered in an agate mortar, and exposed to a stream of dry air at 102° C., until the weight remained unaltered.

In each individual analysis, the weighed salt was once more heated in a water-bath for an hour, and then allowed to cool under a bell-glass with concentrated sulphuric acid; after which it was again weighed. It never occurred in our experiments, that the weight was perceptibly altered by this treatment; this precaution was nevertheless uniformly adopted.

The weighings as well as the burnings were conducted in a thin crucible of Meissner porcelain, which was covered by a platinum lid. All our experiments were fortunately performed with the same crucible, the weight of which therefore was controlled whenever weighings were made.

The calcination of the acetate of silver proceeds very easily, without either swelling up or spirting. The salt at first becomes gray, and, on cautiously heating acetic acid, distils off, and then the salt assumes a brown colour; at length, when the odour of the acid is no longer observable, there remains a gray skeleton of silver, which retains the form of the salt burnt. If the heat be now increased, and the lid raised, so as to admit a current of air, a visible glowing is observed throughout the whole mass, and there remains a spongy mass of shining white metallic silver.

After cooling, the crucible with the silver is weighed, heated to redness anew, weighed again, and so on, until not the slightest change of weight is exhibited. The absence of carburet of silver was always particularly demonstrated by dissolving the silver in dilute nitric acid.

The weighings were effected with a balance, which indicated quite distinctly half a millegramme, even when loaded with 20 grammes. The weights (made by Oerthing of Berlin) were carefully compared previously to employment, and were found to exhibit no appreciable variation in their subdivisions. Lastly, we adopted the precaution of conducting one-half of the experiments with newly prepared salt, by which means the fact, that the numbers yielded by salts prepared by different operations agree, is rendered very obvious.

The tartrate of silver is not easily obtained in a crystalline form. When the nitrate of silver is precipitated in the cold by a solution of pure Rochelle salt, from which, by the addition of a little nitric acid, the weak alkaline reaction which it generally possesses has been removed, a caseous and not a crystalline precipitate is formed. On the other hand, if the precipitation is effected by mixing boiling hot and dilute solutions, the liquid becomes brownish, but not turbid, and on cooling, metallic silver in brown laminæ falls down. Again, when a dilute solution of nitrate of silver is heated to from 80° to 85° C., and to this a hot concentrated solution of the tartrate of potass and soda is added, a precipitate falls, which, by agitating, at first disappears; if the addition of the tartrate of potass and soda be discontinued as soon as the precipitate is permanent, and is not redissolved, fine scales of the tartrate of silver separate on cooling, which, after being well washed and dried, are very white, and have a metallic lustre, resembling that of polished silver. In this process the liquid must always contain a slight excess of the nitrate of silver.

The pure tartrate of silver was dried, and its silver determined by means of calcination, observing the same rules of precaution as in the case of the acetate. By gently heating the salt pyrotartaric and carbonic acids distil off, and there remains, without spirting or swelling, a spongy mass of bright metallic silver, which, when washed with water, yield to it no trace of alkali. Of four determinations which we made, two were with salts prepared by different operations; that employed in the fifth, was prepared with very special care by a gentleman in the laboratory at Giessen, in the course of his investigation on the constitution of organic acids, and with a view to discover, if possible, some difference between the atomic weights of the tartaric and racemic acids. For the preparation of the racemate of silver, very pure racemic acid was half neutralized by ammonia, the resulting sparingly soluble acidulous salt was washed with water, redissolved in water containing ammonia, and again thrown down by means of nitric acid. The acid racemate of ammonia thus obtained was brilliantly white, and perfectly pure. This was employed in the preparation of crystallized racemate of silver, exactly in the manner described for the tartrate of that metal. These two salts do not differ from one another in appearance, but the racemate is less soluble in hot water than the tartrate.

We prepared the malate of silver by means of the acid malate of lime, and nitrate of silver. The acid malate of lime is, by virtue of its very unequal solubility in hot and cold water, easily obtained perfectly pure. Nitrate of silver is mixed with a warm solution of this acid malate, when immediately a granular crystalline, very heavy precipitate falls. Since, after frequently washing this silver salt with water, the fluid still contained traces of lime, the whole precipitate was dissolved in dilute nitric acid, and to this solution ammonia was added drop by drop, taking care that free acid should always be present in excess. The malate of silver is in this case free from lime and ammonia, and is, after continued washing, quite pure.

The malate of silver is decomposed when heated; it fuses, at the same time evolving fumaric acid, carbonic acid, and water, and there remains behind a porous cake of silver, which is free from carbon.

The following are the results of our experiments, represented in a tabular form.

Nos.	Weight of the Salt in Grammes.	Weight of Silver in Grammes.	In 100 Parts.			Atomic Weight of the Salt.	Sum of Four Atoms of Carbon.	Atom of Carbon.
			Silver.	Oxide of Silver.	Acid.			
				ACETATE OF SILVER.				
1.	4·8735	3·1490	64·615	69·396	30·604	2091·790	302·745	75·686
2.	7·5870	4·9030	64·624	69·402	30·598	2091·504	302·458	75·615
3.	6·4520	4·6950	64·623	69·405	30·595	2091·511	302·465	75·616
4.	5·7905	4·7415	64·614	69·395	30·605	2091·804	302·758	75·689
5.	4·1000	2·6490	64·610	69·390	30·610	2091·951	302·905	75·726
	28·803	18·612	64·618	69·399	30·601	2091·680	302·634	75·658
				TARTRATE OF SILVER.				
1.	3·8400	2·2770	59·297	63·684	36·316	2279·390	302·824	75·706
2.	2·7597	1·6365	59·299	63·688	36·312	2279·270	302·704	75·676
3.	3·2356	1·9183	59·287	63·674	36·326	2279·751	303·185	75·799
4.	5·4217	3·2147	59·293	63·682	36·318	2279·530	302·964	75·741
5.	0·9630	0·5710	59·293	63·681	36·319	2279·505	302·939	75·735
	16·220	9·6175	59·294	63·682	36·318	2279·491	302·925	75·731
				RACEMATE OF SILVER.				
1.	5·2640	3·1210	59·290	63·676	36·324	2279·670	303·104	75·776
2.	9·2668	5·4945	59·292	63·679	36·321	2279·561	302·994	75·749
3.	4·6730	2·7705	59·287	63·674	36·326	2279·751	303·184	75·796
4.	1·6320	0·9675	59·283	63·670	36·330	2279·920	303·354	75·838
5.	6·5976	3·9113	59·284	63·671	36·329	2279·890	303·325	75·831
	27·4334	16·2648	59·287	63·675	36·325	2279·711	304·145	75·786
				MALATE OF SILVER.				
1.	6·8730	4·2610	61·996	66·583	33·417	2180·141	303·575	75·894
2.	4·2635	2·6440	62·015	66·604	33·396	2179·490	302·924	75·731
3.	4·4305	2·7495	62·059	66·651	33·349	2177·951	301·385	75·346
4.	5·6490	3·5030	62·011	66·599	33·301	2179·621	303·054	75·764
5.	4·6820	2·9015	61·972	66·557	33·443	2181·011	304·444	76·111
	25·898	16·059	62·009	66·597	33·403	2179·707	303·141	75·785

It will be observed on first sight, with respect to the proportion of oxide and of acid in each particular salt, that our results yield the same number up to the third, and in many cases to the fourth figure; and, in our opinion, a better selection of salts could scarcely be made than that which was adopted, for the proportion of oxygen in these four salts is just as unequal as that of the hydrogen; hence if an error had been occasioned by the hydrogen, it could not possibly escape observation.

In the *acetic*, *malic*, *racemic*, and *tartaric* acids, the oxygen is as the numbers 3, 4, 5, and the hydrogen as 4 : 6.

With such coincidence as is exhibited in our analyses, the existence of any foreign admixture in these salts, which could have increased the atomic weight of carbon, cannot be supposed. We have, however, still another direct proof of their correctness, viz. in the results which Berzelius obtained in his analyses of the *tartrate*, and of the *racemate* of lead. These were as follows:—

Tartrate of Lead.

Nos.	Salt.	Oxide of Lead.	In 100 Parts.	
			Oxide of Lead.	Acid.
1.	2·000	1·25449	62·7245	37·2755
2	2·000	1·25434	62·7170	37·2830
3.	2·000	1·25522	62·7610	37·2390
4.	2·8873	1·81212	62·7618	37·2382
Mean	8·8873	5·57617	62·7431	37·2569

Nos.	Atomic Weight of Salt.	Sum of Four Atoms of Carbon.	Atomic Weight of Carbon.
1.	2223·191	303·743	75·936
2.	2223·460	304·012	76·003
3.	2221·904	302·456	75·614
4.	2221·871	302·423	75·606
Mean	2222·531	303·082	75·771

Racemate of Lead.

(Mean of several experiments.)

2·000 of the salt gave 1·2550 oxide of lead, from which we obtain 2222·290 as the atomic weight of the salt, 302·842 as the sum of the four atoms of carbon, and 75·711 as the atomic weight of carbon.

The results of our analyses of the acetates, tartrates, racemates, and malates of silver, give as the mean of five experiments with each, the following proportions:—

	Salt.	Silver.	Atomic Weights	
			of the Salt.	of Carbon.
Acetate of silver . .	28·803	18·612	2091·680	75·658
Tartrate	16·220	9·6175	2279·491	75·731
Racemate	27·4334	16·2648	2279·711	75·786
Malate	25·898	16·0590	2179·707	75·785

If we compare our experiments separately with one another, that is to say, the atomic weight of carbon as deduced from the one salt, with that from the others, we observe that the numbers obtained in the analyses of the same salt, agree more perfectly with one another, than do the mean numbers resulting from the analyses of the different silver salts.

If the experiments themselves be assumed to be perfectly exact, this discrepancy must, from the very nature of things, result from some cause. This cause can be no other than the difference between the specific gravities of the different salts. They were weighed, not *in vacuo*, but in air, and in the proportion of their unequal densities, they must displace unequal volumes of air. All these salts in the above weighings must lose weight, and those which are specifically lighter more than the specifically heavier salts. Dr. Clark, at the meeting of the British Association held at Birmingham in 1839, called attention to the influence which these relations exert on determinations by weight, and corrected the experiments of Berzelius accordingly. It is clear, that in weighing of from one to two grammes of substance, these differences will not affect the relation of numbers, but when 20 or more grammes are weighed, the correction ought not to be neglected.

Such a correction must result from a knowledge of the specific gravities of salts. We determined those of the four salts analysed by us, by taking a known weight of the salt in a saturated solution, and comparing the specific gravity of this liquid with that of pure water.

3·1281 gr. acetate of silver displaced 1 gr. = 1 c. c. water at 15° C. But 1 c. c. of air at that temperature weighs 0·00123 gr. (log. 0·0905137 − 2); 28·800 gr. acetate of silver displaces therefore 0·0113 gr. atmospheric air. These 28·803 gr. of salt weigh consequently 28·814 gr., but the weight was estimated by means of a brass weight of 28·803 gr., which having the sp. gr. 7·8, displaced 0·00455 gr. of air; that is to say, it lost that weight in air.

The above 28·814 gr. of acetate of silver, weighed therefore 28·8098 gr., and the 18·612 gr. of metallic silver which remains behind after the calcination weighed only 18·6113 grammes.

According to these corrections, 28·8098 gr. of the silver salt, yield 18·6113 gr. of metallic silver.

If we correct in this manner the atomic weights of the tartrate, racemate, and malate of silver, the specific gravities of which are *in vacuo*, respectively = 3·4321, 3·7752, and 4·0016, we obtain the following results:—

	Salt.	Silver.	Atom of Carbon.	Difference from the mean.
Acetate of silver..	28·8098	18·6113	75·804	— 0·050
Tartrate of silver .	16·223	9·6171	75·861	+ 0·007
Racemate of silver	27·438	16·2641	75·908	+ 0·054
Malate of silver...	25·9019	16·0596	75·843	— 0·011
	Mean atomic weight of Carbon 75·854			

The discrepancies in the atomic numbers for carbon lie therefore in the fourth figure, or, what is the same thing, in the second decimal place. The atomic weight of carbon is therefore 75·854.

If we multiply the atomic weight of carbon as here given, viz. 75·854, by 1·1026, the specific gravity of oxygen, we obtain as the hypothetical weight of a volume of carbon vapour 0·83636; and if to this we add 2·20520, the weight of 2 volumes of oxygen, and divide the sum of these by 2, we obtain the number 1·521 for the specific gravity of carbonic acid.

The numbers which have been found for the specific gravity of carbonic acid, by means of the direct weighing of the gas, are—

	Sp. gr. of carb. acid.	Calculated at. weight.
By Biot and Arago	1·519	75·530
By Berzelius and Dulong	1·524	76·437
Calculated from our own analyses	1·521	75·854

We consider it as a further proof of the correctness of the atomic weight of carbon as determined by us, that our result lies intermediate between those of the observations of four such distinguished experimenters; and indeed all doubt must be dissipated, when we reflect that with our number the differences observed in organic analyses are legitimately accounted for*.

---

* Extract of a letter from Berzelius to Professor Wöhler:—

"April 2, 1841.

"Baron Wrede is engaged with experiments in reference to the specific gravity of gases. He has devised peculiar methods of obtaining a balloon of gas in a pure state, which supersede those employed by Dulong and myself. The weights of carbonic acid gas, as hitherto determined by him, nearly agree with those of Dulong and myself. He has found, however, that the specific gravity of that gas is not the same under unequal pressures, but that it diminishes with the decreased pressure until one-third of an atmosphere; below this point it is constant. Its specific gravity at the ordinary pressure

XXXII. *A new Method of Investigating the Resistance of the Air to an Oscillating Sphere. By the* Rev. J. CHALLIS, *M.A., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge**.

I SHOULD have been unwilling to advert again to the subject of discussion between Mr. Airy and myself, unless it had appeared to me, after reading Mr. Airy's Reply in the August Number (p. 143), that something may yet be said in elucidation of this confessedly difficult question. Mr. Airy may feel assured that I have no fault to find with the manner in which he supports his own views, and that equally with him I am persuaded of the necessity of adhering to strictly legitimate principles in treating questions of this nature. By placing my reasoning under another point of view, I hope to show that it is not defective in this respect.

But first I must remark, that the argument by which Mr. Airy concludes that Poisson's solution is possible and mine impossible, is nothing to the purpose. I was fully aware that my solution would not satisfy the equation by which Mr. Airy has tried it; and it was scarcely necessary to prove that an integral obtained by so experienced a mathematician as Poisson satisfied the equation from which it was derived. The question really at issue is, whether that equation, without any limitation annexed, is the one which the conditions of the problem require. The reasoning I am about to adduce will, I think, prove that it is not, and at the same time will be an answer to all that Mr. Airy has said in the latter part of his Reply.

The following general proposition will be necessary for my purpose:—

If N *be a factor which makes* $u\,dx + v\,dy + w\,dz$ *an exact differential, then will*

$$N(u\,dx + v\,dy + w\,dz) = 0$$

be the differential equation of a surface which cuts at right angles the directions of the motion of the particles through which it passes.

The proof that follows is taken from that which Mr. Earn-

---

is therefore somewhat too high, and it contains more than one volume of oxygen; thus it is clear that the atomic weight of carbon, as calculated from this, is also too high. That, calculated from his specific gravity, at the pressure of one-third of an atmosphere, is 75·7. He has as yet, however, only made three weighings, which he considers as little more than introductory experiments of practice. I shall be present tomorrow at the fourth determination."

* Communicated by the Author.

shaw has given of a very similar proposition in a memoir on Fluid Motion, contained in the Cambridge Philosophical Transactions (vol. vi. part ii. p. 204).

Let u, v, w be the resolved parts in the directions of the axes of rectangular coordinates, of the velocity V at a point of the surface whose coordinates are x, y, z; and let $x + dx$, $y + dy$, $z + dz$ be the coordinates of another point of the surface distant by ds from the former. Let the line ds joining the two points make angles α, β, γ with the axes of coordinates, and let the direction of the velocity V at the point xyz make the angles α', β', γ' with the same axes. Then

$$0 = \mathrm{N}(u\,dx + v\,dy + w\,dz)$$

$$= \mathrm{N}\,\mathrm{V}\,ds\left(\frac{u}{\mathrm{V}}\cdot\frac{dx}{ds} + \frac{v}{\mathrm{V}}\cdot\frac{dy}{ds} + \frac{w}{\mathrm{V}}\cdot\frac{dz}{ds}\right)$$

$$= \mathrm{N}\,\mathrm{V}\,ds\,(\cos\alpha\cos\alpha' + \cos\beta\cos\beta' + \cos\gamma\cos\gamma').$$

Consequently, as the factors without the brackets do not vanish, the quantity within the brackets, which is the cosine of the angle that the direction of motion makes with a tangent to the surface, must be equal to zero. Hence the motion is in the direction of a normal to the surface.

As the differential equation $\mathrm{N}(u\,dx + v\,dy + w\,dz) = 0$, may contain the time t in any manner, its integral will be of the form $\mathrm{F}(x, y, z, t) = \phi(t)$; or, differently expressed, $f(x, y, z, t) = 0$. For the sake of brevity I will call the surfaces given by this equation surfaces of *displacement*. In any proposed instance of motion there will be an unlimited number of such surfaces, each of which will in general be continually varying its form and position, as well by the change of t as by the change of form of the function f depending on the arbitrary circumstances of the motion. It will be a restricted case, but sufficient for our purpose, to consider the change of a given surface of displacement to depend only on the change of t, the function f retaining its form. Then the coordinates of the particle which at the time t were x, y, z, will at the time $t + dt$ be $x + u\,dt$, $y + v\,dt$, $z + w\,dt$. Consequently, as these are coordinates of the surface of displacement in its new position,

$$f(x + u\,dt,\ y + v\,dt,\ z + w\,dt,\ t + dt) = 0.$$

Hence taking the letter f to represent $f(x, y, z, t)$, we shall have

$$f + \frac{df}{dx}u\,dt + \frac{df}{dy}v\,dt + \frac{df}{dz}w\,dt + \frac{df}{dt}dt = 0.$$

But

$$f = 0;\quad \frac{df}{dx} = \mathrm{N}u;\quad \frac{df}{dy} = \mathrm{N}v;\quad \frac{df}{dz} = \mathrm{N}w.$$

Hence

$$N(u^2+v^2+w^2)+\frac{df}{dt}=0; \quad \text{or} \quad N=-\frac{1}{V^2}\cdot\frac{df}{dt}.$$

This determination of the arbitrary factor N is the analytical proof that the assumed kind of motion is possible.

Let us suppose, for example, that the surface of displacement is a spherical surface, whose equation is

$$(x-a)^2+(y-b)^2+(z-c)^2=R^2,$$

R being its radius, and a, b, c the coordinates of its centre. And first, let a, b, c be constant, and R be a function of the time. Then

$$\frac{df}{dt}=-2R\cdot\frac{dR}{dt}=-2RV.$$

Hence $N=\frac{2R}{V}$, and consequently N is a function of the time.

Next let a, b and R be constant, and c variable. This is to suppose that the surface of displacement is a spherical surface of given radius, the centre of which is moving parallel to the axis of z. For this case we have

$$\frac{df}{dt}=-2(z-c)\frac{dc}{dt}; \text{ and } V=\frac{z-c}{R}\cdot\frac{dc}{dt}.$$

Hence $$N=\frac{2R^2}{(z-c)\frac{dc}{dt}}.$$

The supposed surface of displacement being thus shown to be possible, it necessarily follows, that if a smooth solid sphere move rectilinearly in the fluid, its surface coincides with a surface of displacement. Here then we have the case of the oscillating sphere, and as $z-c$ occurs in the expression for N, we have come to the important conclusion that $u\,dx+v\,dy+w\,dz$ is not an exact differential in this instance, when the variation of the coordinates is from one point to another of the surface of the sphere, and therefore not an exact differential for *every* variation of the coordinates. It becomes so when multiplied by $\frac{R}{z-c}$, if R be supposed to vary with x, y, z.

Let us, therefore, suppose in general that

$$N(u\,dx+v\,dy+w\,dz)=d\phi.$$

Then

$$u=\frac{1}{N}\cdot\frac{d\phi}{dx}; \quad v=\frac{1}{N}\cdot\frac{d\phi}{dy}; \quad w=\frac{1}{N}\cdot\frac{d\phi}{dz}.$$

Hence by substituting these values of u, v, and w in the equations (a), (b), (c), given in page 64 of the July Number, viz.

$$\frac{d\,\mathrm{P}}{d\,x} + k\,\frac{d\,u}{d\,t} = 0; \quad \frac{d\,\mathrm{P}}{d\,y} + k\,\frac{d\,v}{d\,t} = 0; \quad \frac{d\,\mathrm{P}}{d\,z} + k\,\frac{d\,w}{d\,t} = 0;$$

and neglecting quantities such as $\frac{d\,\mathrm{N}}{d\,t} \times \frac{d\,\phi}{d\,x}$, which will be of the order of the square of the velocity, it will be found that

$$\frac{d\,\mathrm{P}}{d\,t} + \frac{k}{\mathrm{N}} \cdot \frac{d\,\phi}{d\,t} = 0.$$

Hence from the equation

$$\frac{d\,\mathrm{P}}{d\,t} + \frac{d\,u}{d\,x} + \frac{d\,v}{d\,y} + \frac{d\,w}{d\,z} = 0,$$

it follows (since $k\,a^2 = 1$) that

$$\frac{d^2\,\phi}{d\,t^2} = a^2 \cdot \left\{\frac{d^2\,\phi}{d\,x^2} + \frac{d^2\,\phi}{d\,y^2} + \frac{d^2\,\phi}{d\,z^2}\right\}$$

$$- \frac{a^2}{\mathrm{N}} \cdot \left\{\frac{d\,\mathrm{N}}{d\,x} \cdot \frac{d\,\phi}{d\,x} + \frac{d\,\mathrm{N}}{d\,y} \cdot \frac{d\,\phi}{d\,y} + \frac{d\,\mathrm{N}}{d\,z} \cdot \frac{d\,\phi}{d\,z}\right\}.$$

Let us now transform this equation into one of polar coordinates r, θ, η, the angle θ being that which the radius vector r makes with the axis of z, and η that which the plane of these two lines makes with a vertical plane. And, guided by the foregoing result, let us assume the factor N to be $\frac{r}{z - c}$, or $\frac{1}{\cos\theta}$. By the usual rules of transformation I find that

$$\frac{d^2 \cdot r\,\phi}{d\,t^2} = a^2 \left\{\frac{d^2 . r\,\phi}{d\,r^2} + \frac{d . \left(\sin 2\,\theta\,\frac{d\,.\,r\,\phi}{d\,\theta}\right)}{r^2 \sin 2\,\theta\,d\,\theta} + \frac{d^2 . r\,\phi}{r^2 \sin^2\theta\,d\,\eta^2}\right\},$$

an equation differing from Poisson's in having $\sin 2\,\theta$ in the place of $\sin\theta$ in the second term of the right-hand side, and also in the signification of ϕ. This equation, applied to the instance of the oscillating sphere, remains the same (small quantities being neglected) if the origin of the polar coordinates be the centre of the sphere. The resolved parts of the velocity in the directions of the three rectangular axes being

$$\frac{d\,\phi}{d\,x}\cos\theta, \qquad \frac{d\,\phi}{d\,y}\cos\theta, \qquad \frac{d\,\phi}{d\,z}\cos\theta,$$

the velocity in the direction of r will be $\frac{d\,\phi}{d\,r}\cos\theta$. Now the conditions of the proposed problem require that when R is substituted for r in $\frac{d\phi}{dr}$, the equation

$$\frac{d\phi}{dr}\cos\theta = \frac{dc}{dt}\cos\theta$$

should be true, whatever be θ and η. Hence it follows that ϕ is a function of r and t only, and that $\frac{d\phi}{d\theta} = 0$, $\frac{d\phi}{d\eta} = 0$. The above equation consequently becomes

$$\frac{d^2 . r\phi}{dt^2} = a^2 . \frac{d^2 . r\phi}{dr^2},$$

according with what precedes both in giving for ϕ a function of r and t only, and in making $\sec\theta\,(u\,dx + v\,dy + w\,dz)$ an exact differential. Hence the value of the factor N has been correctly assumed. Since $\frac{d\phi}{d\theta} = 0$, and $\frac{d\phi}{d\eta} = 0$, the velocity is wholly in the direction of r, and $V = \frac{d\phi}{dr}\cos\theta$. But by integration, $r\phi = f(r - at)$. Hence

$$V = \left\{\frac{f'(r-at)}{r} - \frac{f(r-at)}{r^2}\right\}\cos\theta.$$

This is the value of V which I am contending for, and from it the result I originally obtained respecting the resistance of the air to an oscillating sphere necessarily follows.

All the above reasoning, excepting the introduction of the factor N, is according to the principles of the method adopted by Poisson. The sole reason of the difference of result is, that Poisson has argued as if $u\,dx + v\,dy + w\,dz$ were an exact differential in an instance, where, if my reasoning be good, that quantity is not an exact differential unless it be multiplied by a factor.

The investigation I have now gone through is in perfect agreement with the principles I advocated in my communication to the June Number of this Journal (S. 3. vol. xviii. p. 477), and may serve to place in a clear light the position I have there maintained, viz. that $u\,dx + v\,dy + w\,dz$ cannot in general be an exact differential unless the variation of the coordinates at a given instant be in the direction of a normal to a surface of displacement. This limitation is equivalent to the introduction of the factor N.

Being aware that I am arguing for a principle, which, if true, must have a very important bearing in the treatment of hydrodynamical problems, I feel it incumbent on me to support my views by an accumulation of evidence, and will therefore add another argument. The equations (*a*), (*b*), (*c*), quoted above, are accurately true if the complete differential coefficients $\left(\frac{du}{dt}\right)$, $\left(\frac{dv}{dt}\right)$, $\left(\frac{dw}{dt}\right)$ be substituted for the partial differential coefficients $\frac{du}{dt}$, $\frac{dv}{dt}$, $\frac{dw}{dt}$, and become by this substitution

$$\frac{dP}{dx}+k\left(\frac{du}{dt}\right)=0;\quad \frac{dP}{dy}+k\left(\frac{dv}{dt}\right)=0;\quad \frac{dP}{dz}+k\left(\frac{dw}{dt}\right)=0.$$

Hence at the same time that $\frac{dP}{dx}dx+\frac{dP}{dy}dy+\frac{dP}{dz}dz$ is an exact differential, $\left(\frac{du}{dt}\right)dx+\left(\frac{dv}{dt}\right)dy+\left(\frac{dw}{dt}\right)dz$ is an exact differential. Let therefore this latter quantity be the differential with respect to x, y, z, of a function ψ of x, y, z and t. Then

$$\left(\frac{du}{dt}\right)=\frac{d\psi}{dx};\quad \left(\frac{dv}{dt}\right)=\frac{d\psi}{dy};\quad \left(\frac{dw}{dt}\right)=\frac{d\psi}{dz}.$$

Hence, by integration,

$$u=\int\frac{d\psi}{dx}dt;\quad v=\int\frac{d\psi}{dy}dt;\quad w=\int\frac{d\psi}{dz}dt.$$

Consequently, differentiating independently of the time,

$$\frac{du}{dy}=\int\frac{d^2\psi}{dy\,dx}dt=\int\frac{d^2\psi}{dx\,dy}dt=\frac{dv}{dx}.$$

So $\frac{du}{dz}=\frac{dw}{dx}$, and $\frac{dv}{dz}=\frac{dw}{dy}$.

Hence $u\,dx+v\,dy+w\,dz$ and $\frac{dP}{dx}dx+\frac{dP}{dy}dy+\frac{dP}{dz}dz$ are also exact differentials at the same time. Now any one arguing according to the views maintained by Mr. Airy would say that the latter quantity *is* unconditionally an exact differential; and how the conclusion could be avoided that $u\,dx+v\,dy+w\,dz$ is without limitation an exact differential in every instance of fluid motion whatever, I am unable to see. But this conclusion is certainly untrue. Poisson has written a long memoir on the propagation of motion in elastic fluids, on the supposition that that condition is not fulfilled. (See *Memoirs of the Paris Academy*, tom. x. 1831.) The explanation of this apparent contradiction I believe to be as follows:— When the complete differential ($d\,$P) is substituted for the sum of the partial differentials $\frac{d\,\mathrm{P}}{d\,x}\,d\,x$, $\frac{d\,\mathrm{P}}{d\,y}\,d\,y$, $\frac{d\,\mathrm{P}}{d\,z}\,d\,z$, it is tacitly assumed that P is a function of x, y, z, which varies *continuously*, and not arbitrarily, with the variation of the coordinates, and so far as the variation of the function is such, the substitution is legitimate. Now in any instance of fluid motion it is only in the direction of the *displacement* of the fluid that the variation of the velocity and density at a given time is in any respect necessarily continuous and not altogether dependent on the arbitrary displacement. In this direction it depends in part on the *propagation* of the motion, and on the mutual inclination to each other of the normals to the surface of displacement, in a manner which it is the peculiar province of the integration of a partial differential equation to make known, and so as to leave the particular form of the function expressing the arbitrary displacement indeterminate. But the variation of velocity and density at a given instant from one point to another *along* a surface of displacement altogether depends on the arbitrary disturbance, and cannot therefore be given by a similar process of integration. With the above-mentioned limitation respecting the direction of variation of the coordinates, both $\frac{d\,\mathrm{P}}{d\,x}\,d\,x + \frac{d\,\mathrm{P}}{d\,y}\,d\,y + \frac{d\,\mathrm{P}}{d\,y}\,d\,z$, and $u\,d\,x + v\,d\,y + w\,d\,z$ are exact differentials in every instance of fluid motion, excepting where the motion is not essentially different from that of a solid.

After this communication I shall not again enter upon the consideration of the problem which has given rise (not, I hope, without some advantage to the cause of science) to so prolonged a discussion. But the general principle on which my solution rests is too important to be hastily dismissed, and I shall probably take other opportunities of exemplifying it.

Cambridge Observatory, August 19, 1841.

XXXIII. *Note on the Production of Sulphuretted Hydrogen by the Action of decomposing Animal Matter upon Sulphates.* *By* ROBERT MALLET, *Ph. D., M.R.I.A.*

To Richard Phillips, Esq., &c. &c.

SIR,

THE valuable communication in the Number of the Philosophical Magazine for this month (July) by Mr. Daniell, on the spontaneous evolution of sulphuretted hydrogen in the waters of the African Coast, &c., has been read by me with peculiar interest, from its connexion with a subject to which

I have for some time devoted a good deal of attention, namely, the action of "air and water on iron," and other metals.

To Mr. Daniell must be awarded the merit of having first pointed out the very extended limits in which sulphuretted hydrogen thus occurs, and of indicating its highly probable connexion with pestilential diseases; but the claim which is apparently made in this paper (see page 11 *et passim*) by its author, to be the first who has pointed out the undoubtedly true source of this gas in the decomposition of sulphates by decomposing organic matter, cannot, I think, be sustained.

In the second report on the action of air and water, (whether fresh or salt) on iron, read at the last meeting of the British Association, and since published, section 171—172, the production of sulphuretted hydrogen in this way will be found fully pointed out, and its theory in the case of the iron salts even much more extended than had before been done; while the mere fact that the sulphates are deoxidized by organic matter in certain states has been observed years since, and even the formation of pyrites by the action of animal matter in putrefaction on the sulphates of iron; a case of which is, I think, recorded in Thompson's Annals of Philosophy. I have remarked that even insoluble sulphates, such as gypsum, can be decomposed in this way; but the power of decomposing either soluble or insoluble sulphates depends upon the *peculiar state* of decomposition in which the vegetable organic matter is found, and does not seem to me to take place, unless when carbonic acid is also present on the water, which it generally then is as a product.

About nine years ago the Directors of the Dublin and London Steam Marine Company requested me to examine the copper sheathing of their steam ship "Thames," since lost off "The Lizard," which had in a very short time corroded into holes, and on the ground of which they were about taking proceedings against the parties who supplied the copper. I found that the inside of much of the copper was lined with black sulphurates, such as Mr. Daniell describes; and on examining the "berth" where the vessel lay when in this port, I found it was over a bank of soft silt, and opposite a large sewer which constantly discharged putrid matter: to this I then attributed the decay of the copper, and advised the removal of the vessel to another berth, which was done after recoppering, and with the result desired: this, therefore, confirms the views on this subject Mr. Daniell has so well stated; but I feel myself compelled to differ with this gentleman as to the power of zinc *permanently* to protect copper from corrosion in sea-water thus charged with sulphuretted hydrogen. The results of experiments made for very extended periods have shown me that unless the surface of the zinc be at intervals kept *clean* mechanically by scouring, &c., it soon ceases to protect, to any considerable amount, iron *or copper* in foul sea-water: its surface gets gradually covered with an impervious coating of microscopic crystals of calc spar, which diminish, and at length destroy all reaction upon it.

No experiments on the mutual protection of metals in water are worthy of dependence unless continued for a very long time, by which reactions are brought into evidence which would never have been perhaps suspected in experiments of much more limited duration.

Should you consider these observations suitable, I shall feel obliged by their insertion in the Philosophical Magazine, and am, Sir, your most obedient,

ROBERT MALLET.

94, Capel Street, Dublin,
July 4, 1841.

XXXIV. *Notices respecting New Books.*

Elements of Chemistry, including the most recent discoveries and applications of the Science to Medicine and Pharmacy, and to the Arts. By ROBERT KANE, M.D., M.R.I.A., Professor of Natural Philosophy to the Royal Dublin Society; Professor of Chemistry to the Apothecaries' Hall of Ireland, &c. Part II. Dublin, 1841, 8vo, pp. 357 to 676, with 49 wood-cuts.

HAVING in the preceding volume (S. 3. xviii.) of the Philosophical Magazine, p. 304, given an analysis of the first part of Dr. Kane's work, we now present our readers with the contents of the second part, in a similar form:—

Chapter X.—OF THE RELATIONS OF CHEMICAL CONSTITUTION TO THE MOLECULAR STRUCTURE OF BODIES.

Sect. I. *Of the Atomic Theory* (continued.)—Physical and chemical atoms. Various orders of molecular groups.

Sect. II. *Of Isomorphism.*—Relation of constitution to form. Various hypotheses on which the isomorphism of compound bodies has been explained. Crystalline forms of the simple bodies. Isomorphous groups. Principle of Plesiomorphism. Similarity of form does not prove necessary similarity of chemical constitution. Principles of isomorphous replacement. Of dimorphism and isomerism.

Sect. III. *Of Dimorphism and Isomerism, and of the Theory of Types.*—List of dimorphous bodies. Difference in structure of dimorphous bodies. Change of physical characters indicating an approach to dimorphism. Principle of isomerism. Connexion of dimorphism and isomerism. Possible isomerism of the metals. Nature of compound radicals. Constitution of organic bodies. Theory of organic types. Of actions by contact.

Sect. IV. *Of Catalysis.*—Of catalysis and analysis. Catalytic effects of heat. Catalytic effects ascribable to the communication of motion.

Chapter XI.—On the Classification of the Elementary Bodies. —Classification of bodies. Classification of elements.

Chapter XII.—Of the simple Non-metallic Bodies, and their Compounds with each other.—Of oxygen. Gasometers. Preparation of Oxygen. Its properties.

Of Hydrogen.—Its preparation. Its properties. Hydroxygen blowpipe. Hydrogen harmonicon. Chemical relation of hydrogen. Constitution of water. Solution of gases in water. Chemical relations of water. Various functions of water, in chemical combination. Mineral waters. Peroxide of hydrogen. Preparation and properties of peroxide of hydrogen.

Of Nitrogen.—Preparation and properties of nitrogen gas. Atmospheric air. Analysis of air. Composition of the atmosphere. Atmospheric air a mixture, and not a chemical compound. Constitution of the atmosphere. Law of diffusion of gases. Permanency of constitution of the atmosphere. Atmospheric pressure. Extent of the atmosphere. Poisson's atmospheric theory. Production of clouds, dew, &c. Compounds of nitrogen and oxygen. Nitrous oxide, its properties and composition. Preparation and properties of nitric oxide. Hyponitrous acid. Nitrous acid. Nitric acid. Natural and artificial production of nitric acid. Preparation of nitric acid. Properties and constitution of liquid nitric acid. Detection of nitric acid. Composition of nitric acid.

Sulphur.—Sources, properties and preparation of sulphur. Relations of oxygen to sulphur. Preparation and properties of sulphurous acid. Sulphurous acid. Sulphuric acid. Formation of sulphuric acid. Manufacture of oil of vitriol. Anhydrous sulphuric acid. Hydrates of sulphuric acid. Hyposulphurous acid. Hyposulphuric acid. Real constitution of the compounds of sulphur and oxygen. Preparation and properties of sulphuretted hydrogen.

Of Selenium, and its compounds with oxygen, hydrogen, &c.—Preparation and properties of *Phosphorus.* Compounds of phosphorus and oxygen. Phosphorous acid. Phosphoric acid. Phosphates of water. Salts of phosphoric acid. Preparation and properties of phosphuretted hydrogen.

Of Chlorine.—Preparation of chlorine. Bleaching power of chlorine. Oxidizing power of chlorine. Compounds of chlorine and oxygen. Hypochlorous acid. Chloric acid. Chlorous acid. Perchloric acid. Chloride of hydrogen. Composition of muriatic acid. Muriatic acid. Liquid muriatic acid. Nitro-muriatic acid. Chloride of sulphur. Chloride of phosphorus.

Iodine.—Preparation of iodine upon the great scale. Properties of iodine. Iodic acid. Feriodic acid. Hydriodic acid. Iodo-phosphuret of hydrogen.

Bromine.—Properties of bromine. Hydrobromic acid. Compounds of bromine.

Of Fluorine.—Hydrofluoric acid.

Of Silicon.—Of silica. Of silicic acid. Chloride of silicon. Fluoride of silicon. Hydrofluosilicic acid.

Boron and boracic acid.—Chloride of boron. Fluoride of boron. General characters of the metals.

Chaper XII. [XIII.]—Of the General Characters of the Metals, and of their Compounds with the Non-metallic Bodies.—Malleability and ductility. Compounds of the metals with oxygen. Classification of the metals. Degrees of oxidation. Relation of the metals to chlorine. State of the metals in nature. General principles of the reduction of the metals.

Chapter XIII. [XIV.]—OF THE INDIVIDUAL METALS, AND OF THEIR COMPOUNDS WITH OXYGEN, SULPHUR, SELENIUM, AND PHOSPHORUS.

Sect. I.—*Metals of the First Class.*

Of Potassium.—Preparation of potassium. Properties of potassium. Preparation of potash. Properties of potash. Sulphurets of potassium.

Of Sodium and soda.

Lithium and lithia.

Barium.—Compounds of barium. Preparation of barytes. Sulphuret of barium.

Strontium and strontia.

Of Calcium.—Compounds of calcium. State of lime in nature. Properties of lime. Sulphurets of calcium.

Of Magnesium.—Properties of magnesium. Properties of magnesia.

Of Aluminum.—Preparation of aluminum. Condition of aluminum in natural properties of alumina.

Sect. II.—*Metals of the Second Class.*

Of Glucinum and glucina.

Of Yttrium, Thorium, Zirconium, Cerium, Lanthanum.

Of Manganese.—Preparation of manganese. Oxides of manganese. Protoxide, sesquioxide, and peroxide of manganese. Determination of the real value of peroxide of manganese. Manganic and permanganic acids. Detection of manganese.

Sect. III.—*Metals of the Third Class.*

Of Iron.—State of iron in nature. Smelting of iron ores. Preparation of malleable iron. Manufacture of steel. Relations of iron to oxygen. Passive condition of iron. Various oxides of iron. Sulphurets of iron. Detection of iron.

Of Nickel.—Preparation of nickel. Nickel and its oxides.

Of Cobalt.—Preparation of cobalt. Cobalt and its oxides. Uses of cobalt in the arts.

Of Zinc.—Preparation and properties of zinc. Of the oxide and sulphuret of zinc.

Of Cadmium and its compounds.

Of Tin.—Extraction and properties of tin. Protoxide and peroxide of tin. Isomeric forms of stannic acid. Preparation of mosaic gold.

Of Chromium and its compounds.—Oxide and acid of chrome.

Of Vanadium.

Sect. IV.—*Metals of the Fourth Class..*

Tungsten and its oxides.—Compounds of tungstin.

Molybdenum and its compounds.

Osmium and its compounds.

Columbium and *Titanium.*—Titanic acid and titanic oxide.

Of Arsenic.—Properties of arsenic. Arsenious acid. Arseniuret of hydrogen. Sulphurets of arsenic. Detection of arsenic. Liquid tests of arsenic. Arseniuretted hydrogen. Marsh's tests for arsenic. Sources of fallacy. Arsenic naturally in the body. Antidote to arsenious acid.

Antimony and its oxides.—Oxygen acids of antimony. Sulphurets of antimony. Preparation of kermes mineral. Forms of sulphuret of antimony employed in pharmacy. Antimoniuret of hydrogen.

Tellurium and its compounds.

Uranium and its compounds.

Sect. V.—*Metals of the Fifth Class.*

Of Copper.—Reduction of copper from its ore. Properties of copper. Oxides of copper. Sulphurets of copper. Detection of copper. Useful alloys of copper.

Of Lead.—State of lead in nature. Oxides of lead. Sulphuret of lead.

Of Bismuth.—State of bismuth in nature. Atomic weight of bismuth. Compounds of bismuth.

Sect. VI.—*Metals of the Sixth Class.*

Of Silver.—Extraction of silver from its ores. Properties of silver. Oxides of silver. Detection of silver.

Of Mercury or Quicksilver.—State of mercury in nature. Properties of mercury. Oxides of mercury. Sulphurets of mercury. Detection of mercury.

Of Gold.—Extraction and properties of gold. Detection of gold.

Of Palladium.

Of Platinum.—State of platinum in nature. Different forms of platinum. Compounds of platinum. Detection of platinum.

Iridium and its compounds.

Rhodium and its compounds.

Chapter XIV. [XV.] ON THE GENERAL PROPERTIES AND CONSTITUTION OF SALTS.—General characters of salts.

XXXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xviii. p. 141 *.]

November 26 (*Continued*), 1840. THE following papers were read:— Description of a Percussion Shell to explode at the bottom of the Sea. By Captain J. Norton. Communicated by S. Hunter Christie, Esq., M.A., Sec. R.S., &c.

An iron tube, like the barrel of a musket, is screwed into a shell of any size, water-tight. A rod of iron, about half a pound in weight and a foot in length, is suspended within the tube, by means of a split quill passing through a hole in the upper end of the rod, the other end being armed with a percussion-cap. The mouth of the tube is closed with a screw lid also water-tight. Tin or brass wings being attached to the upper end of the tube will keep it in a vertical position during its descent to the bottom of the sea; and the shock on its striking the bottom will cause the bar of iron within the tube to fall, and produce the percussion and explosion.

Should it be found difficult to make the shell water-proof, I am satisfied that percussion powder made from silver will explode by friction or percussion even when *mixed with water*.

Memorandum addressed to the Royal Society. By T. Wharton Jones, F.R.S.

The following is the memorandum in the words of the author:—

On the 18th of June, 1835, a memoir, entitled, "On the Ova of Man and Mammiferous Animals, as they exist in the Ovaries before Impregnation, and on the discovery in them of a Vesicle analogous to that described by Professor Purkinje in the Immature Egg of the Bird †," was laid before the Royal Society.

[* The two abstracts of papers now inserted were accidentally omitted from their proper place; they are the notices referred to in vol. xviii., p. 547; and are followed in order of reading by those given at p. 307 of that volume.]

[† An Abstract of this memoir appeared in L. and E. Phil. Mag., vol. vii. p. 209.]

At the time I wrote, I believed myself the first who had observed the vesicle alluded to; but by a reference to the manuscript in the archives of the Society, it will be seen, from a postscript, that before sending it to be communicated, I had become aware that M. Coste of Paris had some time before announced that he had made a similar observation, as far as concerns the rabbit. Those who are conversant in such matters are doubtless aware that I was anticipated also by Professor Valentin: but of this circumstance I was not informed till some considerable time after.

It thus appears that, though I was an independent discoverer of the germinal vesicle of the mammiferous ovum, all the share in the discovery I can lay claim to *historically* is that of being the first who pointed it out in this country.

There is one point, however, in the anatomy of the germinal vesicle of the mammiferous ovum, of which I feel myself entitled to be recognized, especially by the Royal Society, as contemporaneous discoverer, and that is, the spot on the side of the vesicle. Feeling this, and having heard at the last meeting of the Royal Society the discovery of this spot attributed solely to the distinguished German physiologist, Professor Rudolph Wagner, I consider it due to the Royal Society and to myself to call to the Society's remembrance the fact, that, in the memoir above referred to as having been laid before them in 1835, the spot in question is not only pointed out and particularly delineated, but its physiological importance hinted at.

The laying of a paper before a Society is an act of publication. With the communication of my paper to the Royal Society in 1835, the publication of Professor Wagner's paper in Müller's *Archiv* was *contemporaneous* merely.

It is true, that though Professor Wagner's observations were only first published in Müller's *Archiv* for 1835, there is a note by the editor, saying that the paper was received by him in 1834; but it is also true,—and of this, were it necessary, proof could be easily adduced,—that my paper was written also in 1834.

In conclusion, I beg to apologize to the Royal Society for obtruding on their notice what may appear matter rather of personal than general interest.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from vol. xviii. p. 604.]

Nov. 13, 1840.—The following communications were read:—

A Letter from Mr. Dawes on the subject of a new Binary Star recently observed.

"I beg to call the attention of the Society to the star registered by Sir William Herschel as the 16th of his third class of double stars. Its Æ is $20^h\ 23^m{\cdot}6$, and N.P.D. $79^\circ\ 17'$. This star was measured by Herschel and South in 1822 with the five-foot achromatic. It was again observed by Struve on two nights in 1829, and also on two in 1832; and though powers of 320 and 480 were employed in the measurements, nothing remarkable was noticed by him in either of the stars. On turning upon it Mr. Bishop's equatorially-mounted achromatic telescope, having an aperture of seven inches, and focus of nearly eleven feet, armed with a power of 320, though the character of the night (October 27) was very indifferent, I was immediately struck with the elongated appearance of the smaller of the two stars; and having applied higher powers, I procured measures of the direction of the elongation. I have since obtained two other sets with power 420, with which, in best moments, the elongated disc was slightly notched. The results of the three nights' observations are:—

October 27,	Position = 208°40′	Weight = 12
— 31,	... = 208 25	... = 11
November 4,	... = 208 44	... = 18
Mean......	= 208 38	

The estimated central distance = 0″·6 or 0″·7.

"It is extremely improbable that so acute an observer as Struve should have failed to recognise an appearance which is now so obvious and measurable with a much smaller instrument, if the star had then presented the same aspect as at present—the Dorpat refractor being capable of *distinctly separating* stars of the 8th or 9th magnitude whose central distance does not exceed 0″·4. Most likely, therefore, this star constitutes a new binary system; and it is highly desirable that, during the remainder of its present apparition, observations should be obtained by different individuals possessing instruments of sufficient optical power. To the notice of such, I beg earnestly to commend it.

"I may embrace this opportunity of announcing that the observations made during the last sixteen months at Mr. Bishop's Observatory afford satisfactory proof of binary character in several instances where it had been only suspected to exist, and of a very large amount of orbital motion in some binary systems previously known as such. The close pair of *ε Equulei* are decidedly more distant than when observed by Struve in 1835 and 1836. Within the last four years, 4 *Aquarii* has advanced 20° in its orbit; in which interval the star H i. 39 (Σ 3062) has changed its position to the extent of about 40°, and *η Coronæ* nearly 50°, with a central distance of scarcely 0″·5. The alteration is also striking in *ζ Herculis* (now measurable with a five-foot achromatic) and in Σ 2107; while *τ Ophiuchi*, which for years defied the power of the Dorpat telescope even to elongate it, has now opened out to the extent of nearly a second between the centres of its component stars. Supposing Sir W. Herschel's measure of the close pair of *ζ Cancri*, taken in 1781, to be exact, that remarkable binary system will now have *completed* a whole revolution since that date, that is, in 59 years.

"W. R. Dawes."

"Mr. Bishop's Observatory, Regent's Park,
Nov. 12, 1840."

A Supplemental Catalogue of the Right Ascensions of Fifty-five

Stars contained in the Royal Astronomical Society's Catalogue. By the Honourable John Wrottesley[*].

Of the stars contained in this catalogue, seventeen had been wholly, or in part, observed at the time Mr. Wrottesley's former catalogue of 1318 stars[†], for which the gold medal of the Society was awarded in 1839, was in course of observation, but are not included in that catalogue. The remaining thirty-eight are selected from the first list of stars accompanying Mr. Baily's "Address to Astronomical Observers," May 1837; and such were chosen as had not been previously observed at the author's observatory, or had not been observed at all since Piazzi's time, or presented discrepancies the clearing up of which seemed most likely to prove interesting. The observations of these thirty-eight were begun in May 1837, and carried on until August in the same year, by Mr. Hartnup; they were resumed by Mr. Wrottesley himself in December 1839, and concluded in August 1840. In every case the observations were continued until six or more of each star had been procured. In observing this catalogue all possible care was exercised that the mean places of the stars comprised in it should be entirely unaffected by any errors arising from imperfect instrumental adjustment. With respect to the standard stars employed, in 1837 only a few were used, sometimes only one, but care was taken that it should be situated very near the parallel of declination of the catalogue star. In 1840 all the Greenwich stars were used indifferently, that pass the meridian to the south of the zenith, and are comprised in the list of those with which the former catalogue was compared; and usually from five to six, and sometimes as many as from eight to ten, were observed on the same day, and the mean clock-error resulting therefrom used in deducing the catalogue stars. Bessel's right ascensions of these standard stars, and Mr. Wrottesley's own place of Fomalhaut, have been invariably used in reducing the catalogue stars; so that this catalogue, as well as the preceding, is founded on Bessel's mean places of the Greenwich stars. In an introduction to the catalogue, the author has explained in detail the different methods which he employed for determining the errors of level, collimation, and azimuth, and the other corrections applied in the reductions. The performance of the transit-clock during the progress of the observations was satisfactory; for though the rate was at times considerable in amount, it was always uniform. As a test of the extent to which the catalogue may be relied on for accuracy, Mr. Wrottesley states, that out of the forty-three stars contained in it, which have been observed by Mr. Airy, in no one case does the difference of the results, for a star more than 25° from the pole, exceed $0^s{\cdot}17$.

Postscript to Mr. Baily's Report on Mr. Maclear's Pendulum Experiments[‡]. By Mr. Baily.

---

[*] Now Lord Wrottesley, and President of the Society.

[†] See the Monthly Notices for November 1836; [or Phil. Mag., Third Series, vol. x. p. 227.]

[‡] Read at the meeting of the Society in June 1840. [See vol. xviii. p. 602.]

The author states that the Admiralty having left to his decision the form and construction of a new pendulum, which they had resolved on sending out to the Cape, for the purpose of being swung by Mr. Maclear at the several stations of the trigonometrical survey now in progress in that colony, he had not hesitated in adopting the *bar*-pendulum, as by far the best and most convenient for a travelling instrument. The pendulum which has been accordingly constructed is a brass bar, sixty inches long, two inches wide, and about half an inch thick. It was formed of several thin plates which were pressed together by a rolling machine, and is, consequently, very compact and hard. Its specific gravity was 8·60, and its rate of expansion for one degree of Fahrenheit's thermometer, ·00001034. It is furnished with four knife-edges, thereby affording the advantages of four distinct pendulums on one and the same bar, and which thus serve as a check on each other. As the construction of the pendulum did not allow of much filing away at the ends without cutting into the knee-pieces, the vibrations on the several knife-edges were rendered nearly isochronous (for absolute isochronism can hardly be obtained), by fastening a circular piece of brass weighing 3000 grains, about an inch and a half from the centre of the bar; the weight and position having been determined by repeated preliminary experiments. After every thing was finished, seven sets of experiments were made on each knife-edge, the mean results of which were respectively as follows:—knife-edge A, 85906·322 vibrations; B, 85905·725; C, 85904·107; D, 85903·427; in a mean solar day. The computations and corrections were made in the usual manner, with the exception of the correction for the height of the barometer, which can only be determined accurately by swinging the pendulum *in vacuo*. For this there was not time before the pendulum was sent off, and the correction was assumed to be the *double* of that which is given by the formula which was usually employed prior to the experiments of M. Bessel. The agate planes, which were made expressly for this pendulum, are attached to a solid frame of brass, three-quarters of an inch thick, and having three foot-screws for the purpose of levelling the planes.

Observations of the Second Comet of 1840, made at the Observatory at Hamburgh. By Mr. Rumker.

The observations give the apparent right ascension and declination from January 29 to March 24, 1840.

Dec. 11.—The following communications were read:—

On a large Achromatic Object-Glass of a Telescope worked by Mr. Dollond, the flint glass of which was prepared by the late Dr. Ritchie. By the Rev. Samuel King, M.A., F.R.A.S.

In a paper by Mr. Simms, "On the Optical-Glass prepared by the late Dr. Ritchie," which was read to the Society on the 14th of June, 1839, and is now printed in vol. xi. of the Memoirs, reference was made to an object-glass of 7⅜ inches aperture, the flint glass of which was worked by Mr. Dollond out of a disc prepared by Dr. Ritchie; and it was intimated that Mr. King, who had the object-glass at that time under trial, would probably report to the

Society upon its performance. Mr. King now states that the result of numerous observations on a variety of objects leads him to consider this glass as one of much excellence, though not faultless. There is scarcely any spherical aberration, and the light is very white and free from colour; but when the central portion is covered up, there is a good deal of irradiation, indicating a want of homogeneity near the edge of the lens, where the glass is very thick. For many objects, however, especially very faint nebulæ, the whole aperture may be used with great advantage; but for most purposes a contraction to six inches, or a little less, causes it to perform much better, and enables it to carry high powers with much distinctness. The powers tried with it vary from 40 to 700, and no doubt it would satisfactorily bear a considerable increase in this respect. It is not saying much of such an object-glass, that the small stars accompanying *Polaris*, *α Lyræ*, and *Rigel*, are readily seen; but it shows also with the greatest distinctness and completely separates the close stars of *ζ Cancri*, *ι* and *ζ Boötis*, *ξ Ursæ*, &c., and also *δ Cygni*, which affords one of the best tests of distinctness and perfection in a telescope. As a planetary glass, Mr. King remarks that he cannot speak of it so decidedly, not having had a fair opportunity of trying it. *Jupiter* and *Saturn* have both for a long time been at very low altitudes, and, with respect to his observatory, the entire range of each is through the smoke of London. But with the moon it performs magnificently, penetrating, as it would seem, into her very structure, when high magnifying powers are used. Upon the whole, he is of opinion that this object-glass will bear comparison with most others of the same size worked from the foreign material.

The object-glass is fitted into a brass cell, admitting of accurate adjustment by means of three screws with rods reaching to the eye end. The telescope, twelve feet in length, is mounted upon the rotative roof of a small observatory, in a manner which combines great ease in observing with freedom and steadiness of motion.

LONDON ELECTRICAL SOCIETY.

July 20, 1841.—The communications before the Society were,—1st, "On the Perforation of Non-conducting Substances by the Mechanical Action of the Electric Fluid." By Mr. Crosse.

Two wires were laid end to end and tied firmly on a piece of window glass, and a quick succession of sparks was passed between them by connexion with the respective conductors of a powerful machine; the result was a perforation. The same was varied in several ways; and was afterwards successfully tried with a crystal of quartz. The author concludes that even diamonds may be thus drilled.

2nd. "The Effects of Vegetable Points on Free Electricity." By Mr. Pine.

This was a continuation of the subject of a former paper, and contained some striking illustrations of the functions of leaves of growing plants, &c.

3rd. "An Account of Experiments undertaken to investigate the Nature of the Change of Colour of Bodies by Heat, and their conducting Power." By Mr. Pollock.

It is the author's object to show that the imponderable agents are all one. He has this in view in all the communications which he lays before the Society, and is indefatigable in gathering from every source such facts as may tend to bear on the one great object. How far he has been successful may be judged from a perusal of his several papers.

4th. "On the Method of restoring to weakened Magnets their primitive strength." By Professor Muncke. (Translation.)

The principle is simply to furnish the magnet with keepers in proportion to its strength, and to add more as its power increases.

5th. A Translation of an account of "Experiments made with a powerful Grove's Battery. By M. De la Rive."

Among the experiments was that of the rotation of the voltaic flame in obedience to the magnet; and the Professor's opinion on the subject is thus expressed in his own words: —"That which the magnet attracts or repels is that conductor formed of the series of particles of carbon transported from one pole to the other, and traversed by the current."

6th. The Secretary then laid before the Society Mr. Weekes's "Monthly Register for June," containing data upon several important points. This register will form a portion of the Society's Quarterly Part of the Proceedings.

Aug. 17.—The papers read this evening were,—1st, "On a New Electro-magnetic Machine." By Mr. B. Hill.

The polarities of the rotating magnets were so adjusted as to make available both the attractive and repulsive results, thus producing a power equal to the *sum* of these, whereas in other machines the power is only equal to the difference.

2nd. "Description of a smaller Atmospheric Electrical Apparatus." By W. H. Weekes, Esq.

The insulating apparatus in this is similar to that in the larger apparatus already described in the Proceedings of the Society; the collecting arrangement is a *pointed wire*, in place of 365 yards of wire. The use of such an apparatus is very limited, and not to be depended on except for certain purposes. Mr. Weekes mentions, that a lighted candle, in a metal candlestick insulated on wax, will gather indications of electricity when other means fail.

3rd. "Further Observations on Electrotype Manipulation:—Fusible Metal." By Charles V. Walker, Esq., Hon. Sec.

The author here describes the composition of fusible alloy, and the mode of producing *Clichée* medals,—fusible casts from fusible moulds. The method is not so generally known here as on the Continent; it is very simple and effectual. Specimens were on the table obtained by these means, for which the author expressed himself as indebted to Mons. C. de Rheims of Calais.

4th. "Methods of giving more force and stability to the Currents of Galvanic Batteries formed by a single Liquid." By Professor Poggendorff. (Translation.)

The copper plates of batteries are submitted to one of four processes, which gives them a surface analogous to that of Mr. Smee's platinized platinum. One of the methods is to deposit copper on them by electrolysis. The action of such plates in combination with amalgamated zinc is both powerful and constant.

5th. "On a Voltaic Process for Etching Daguerréotype Plates." By W. R. Grove, Esq., M.A., F.R.S., Prof. Exp. Phil. Lond. Inst.

This discovery enables us to multiply in a durable material the fleeting and delicate traces of Daguerréotype. The plate to be etched is made a positive electrode, in an electrolyte of dilute hydrochloric acid; and the action is continued for a few seconds. The etchings are fit for the printer. Several prints obtained from plates thus prepared were placed before the Society, and were much admired as specimens of what may be done when the art has been further carried out. The author states that these prints are not so true to nature as the original picture, because in order to etch deep enough to receive the printing ink, some of the fine lines will blend. This will not practically be an objection to the process, because no lines are lost except those which, if present, could not be appreciated. One very important application of the art is to etch very delicately a picture, and to take from this *perfect* etching electrotype copies. These are so true that the author actually read on one, by microscopic aid, five lines of inscription on a surface 1-10th by 6-100dths of an inch. The following are the concluding words of this communication:—"I transmit with this paper some specimens of engravings of the etched plates, and of electrotypes taken from them; and in conclusion would call attention to the remarkable instance which these offer of the effects of the imponderable upon the ponderable. Thus, instead of a plate being inscribed as 'drawn by Landseer and engraved by Cousins,' it would be 'drawn by Light and engraved by Electricity.'"

The business of the evening terminated by the reading of Mr. Weekes's Monthly Journal of the electric state of the atmosphere.

ROYAL IRISH ACADEMY.

Abstract of Observations on the Origin of Audible Sounds. By ROBERT KANE, *M.D.*[*]

Nov. 30, 1840.—Dr. Kane read a paper "On the Production of Audible Sounds," of which the following is an abstract.

The sensation of sound is produced upon the ear by the tympanum being thrown into vibratory motion, isochronous with the vibrations transmitted from the sounding body.

---

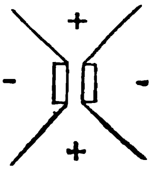
[*] From the Proceedings of the Royal Irish Academy.

Any body which vibrates as a single mass gives origin at the same moment to two waves, whose motions are in opposite directions, and of which one is rarefied and the other condensed.

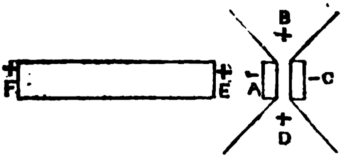
If these two arrive at the tympanum at the same moment and with equal power, perfect neutralization should result, and no sound be heard: hence, where a vibratory body produces upon the ear the sensation of sound, it arises from one wave of the two being either totally intercepted, or, at least, diminished in force, and the loudness of the sound is proportional to the difference of the intensity of the two waves when they affect the ear.

All instruments for increasing sound, and producing resonance, act upon this principle.

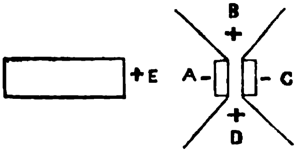
The following facts will illustrate these principles in detail. A tuning fork is a centre of four waves, two + and two —, but unless it be very close to the ear, no sound is heard from it; because the centre of all the four waves being very close, all act on the ear with equal force, and the difference is 0 (approximately).



Now, if an open tube, of the same length as a one-phase wave from the fork, be approached to one centre, as A, in the adjoining figure, the air in it commences to vibrate in unison with the fork, from being set in motion by the first wave which passes into it: the vibration of the tube is, however, a phase behind that of the fork, and hence, when a — wave passes from the centre A, it meets a + wave from the end of the tube E, and both are destroyed. The —centre, C, destroys also a + centre, as D, and there remain only the centres of + waves, B from the fork, and F from the tube, and these acting in concert on the tympanum produce the sound that we hear.

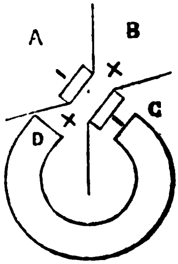


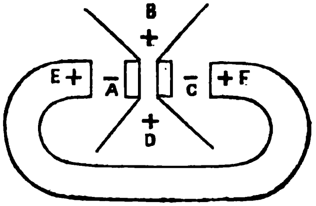
If the tube be closed, and of only one-half the length, the + wave, which emanates from the centre A, passes in, and being reflected from the bottom, issues again at the moment when the next — wave from A is about to enter; E and A then destroy each other, and C and D also interfering, there results only the + wave B, which acts unimpeded on the ear. The sound of an open tube is, therefore, *ceteris paribus*, much stronger than that of a closed tube, as there are two waves in place of one.

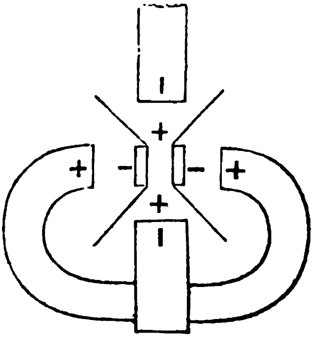


That the office of closed tubes, when resonant, is to destroy a portion of the sound of the original vibrating body, and of the open tubes to afford, in addition to that, a new centre of a wave of the same phase as that which remains, may be exhibited in many ways.

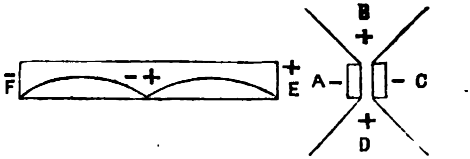
Thus, Mr. Adams showed long since, that when two closed tubes are placed at right angles to each other, they interfere when made to speak to a tuning fork, and for this effect no explanation has hitherto been given. But it is evident that the tubes being at right angles to each other, the waves destroyed are in opposite phases, and those which remain are in opposite phases also, so that the effect is the same as if no tubes were present at all. The same effect may be produced by a single tube, bent so that its apertures may be at right angles to each other; the − and + waves, D and C, meeting in the tube, produce neutralization, and the waves A and B, also + and −, which remain, interfere also, and hence no sound results. In an open tube bent into a circle, as in the figure, the two waves destroyed (A C) are of the same phase, and also those which remain (B D), and hence such a tube sounds with nearly double the power of an ordinary open tube. That it is the sound of the waves which do not go into the tube, and not that of the waves in the tube, we hear, may be shown by applying two closed tubes, as in the next figure. When the two − waves are absorbed by the circular open tube, each closed tube absorbs a + wave, and hence, notwithstanding that there is so much vibrating material, no sound is heard. But if the tubes A and B were open, then the vibrating centres should have been simply transferred to their further extremities, and the tubes would emit sound as the fork had done without them in the preceding figure.







If the open tube be double the length of a phase, then the neutralization occurs as in the figure, the residual waves being B and F, in opposite phases; but as their centres are separated so far, they interfere only in hyperboloidal planes, which are not detected unless when carefully sought for, but have been noticed to exist by Savart, although he did not suspect their cause.



All these principles have received very full verification from an instrument constructed for the purpose, and termed a *Chorizophone*. It consists of a square glass plate, which is placed above a set of closed tubes of such size, that when the plate vibrates in four pieces, with diagonal nodal lines, the length of each tube is half the length of the phase of the wave produced, and their form is triangular, of the magnitude of one of the four vibrating portions of the plate; when one of these tubes is presented to the plate, and this brought to vibrate by a violin bow applied to the centre of one of the sides, the tube resounds, and more loudly in proportion as the plate is brought nearer to its orifice. Now here the entire wave from the plate is caught by the tube, and the more perfectly its escape into the air is prevented, the louder is the sound produced, the sound must arise therefore from the waves which do not pass into the tube. Any one or more waves may thus be absorbed by the closed tubes, and a range of loudness of sound produced from the same plate with one or more of the four tubes, according as they are disposed as follows:—

The vibrating plate gives eight waves, four above and four below, 4 being + and 4 —.

With one tube, one wave is absorbed, and 3 + and 3 — destroying each other, a wave remains opposite in phase to that which is absorbed, and produces an audible sound.

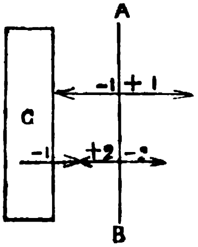
With two tubes, the waves absorbed may be either of opposite or of the same phases. If opposite, then the remaining waves are 3 + and 3 —, and no sound is produced; but if the waves absorbed be of the same phase as +, then there remains 4 — and 2 +, and hence the ear is doubly affected by 2 —. The two tubes may be either both above or both below, or one above and one below the plate.

With three tubes, the absorbed waves may be either all of the same phase, or two of one and one of the other. In the first instance, 3 + being absorbed, there remains 4 — and 1 —, and the ear receives the impulse of 3 —. In the other case 2 + and 1 — being absorbed, there remains 2 + and 3 —, and the impulse on the ear is only 1 —. The position of the tubes may vary in this as in the former case.

With four tubes, the absorption may be either all of the same phase, or 2 + and 2 —. In the former case, the remaining waves will be either 4 + or 4 —, in which case the greatest sound the plate can produce is heard, or else there remain 2 + and 2 —, in which case the plate gives no sound. These results prove fully that it is the residual sound that is heard, and not that which passes into the tube.

A vibrating plate gives some sound always, even without the tubes, for since there are at least eight waves, some one will always be more favourably disposed for acting on the ear than another: this difference will increase with the number of waves; and hence the independent sound of a plate increases in proportion as the vibrating portions into which it divides become more numerous.

A string vibrating in free space produces little or no sound; but if it be strung over, or in connexion with, an elastic board or box, a great resonance is produced. This arises from two sources; first, the string when by itself is the centre of two waves excessively close, and the action of which is therefore interfering. But if the string A B vibrate near a plane surface C, the wave − 1, which passes towards it, is reflected back, and meeting the wave + 2, which follows, it neutralizes it partly, and enables the wave − 2 to reach the ear without diminution. It is probable, however, that the great portion of the sound arises from the board or plate itself vibrating in parts, or as a whole. If in parts, these parts are variously situated, as regards the ear, and hence produce an effect upon it. Or if, as a whole, the plate C is so broad, or bounded, if a box, that one wave is lost by internal reflexion, and only the wave emanating from the outer surface can arrive at the ear.



When a tuning fork is placed on a table, one wave is lost by internal transmission and reflexions, whilst that directed from the outer surface reaches to the ear.

In the case of reed instruments, the reed produces two waves, which, if it vibrated freely, should neutralize each other on the ear; but in practice whilst an open passage is allowed to one by the mouth-piece, the other wave is lost within the cavities of the lips and mouth. In mouth-piece instruments, as bugles and trumpets, the cavity of the mouth serves also for the absorption of the one wave, leaving the other free to act.

XXXVI. *Intelligence and Miscellaneous Articles.*

ACTION OF SULPHURIC ACID ON NITRATE OF AMMONIA. BY M. PELOUZE.

WHEN sulphuric acid and nitrate of ammonia are mixed at common temperatures, no phænomena occur which might not be expected, whatever may be the properties employed, that is to say, reagents indicate the presence of ammonia, sulphuric acid and nitric acid.

If the mixture contain water and be submitted to distillation, nitric acid and the sulphate of ammonia indicated by theory are obtained. When, on the contrary, the nitric of ammonia is deprived of water, and it is heated in fifty times its weight of concentrated sulphuric acid, the mixture yields, at about 302° Fahrenheit, a very considerable quantity of nitrous oxide; water is formed, which combines with the sulphuric acid, and neither nitric acid nor ammonia exists in the product of this reaction. The nitrate of ammonia behaves under these circumstances as it does when merely heated, and it affords the only example of a nitrate which does not yield nitric acid by the action of sulphuric acid, and at the same time give up its base to this acid, more fixed and energetic than nitric acid.

When the proportion of sulphuric acid is most diminished, operating, for example, with ten parts to one of nitrate of ammonia, about three-fourths of the salt are decomposed into nitric acid and ammonia, and one-fourth into nitrous oxide and water. By gradually diminishing the proportion of sulphuric acid, but little nitrous oxide is obtained, so that with one equivalent of nitrate of ammonia and two equivalents of sulphuric acid, the phænomena coincide with those of the decomposition of a salt by a more fixed acid.

When a mixture of nitrate of ammonia and a large excess of sulphuric acid is heated only between about 200° and 250° Fahr. instead of 322° Fahr., nitric acid distils, but without the production of any nitrous oxide. When nitrite of ammonia is heated with a great proportion of concentrated sulphuric acid, it is converted as by the mere action of heat into azote and water.

It appears from the preceding statements, that very different results are obtained by varying the proportions of nitrate of ammonia, sulphuric acid, the quantity of water and the temperature employed. —*Journal de Pharmacie*, t. xxvii. p. 271.

PREPARATION OF AZOTIC GAS. BY M. PELOUZE.

M. A. Rose has observed that monohydrated sulphuric acid combines directly with nitric oxide, and absorbs very considerable quantities of it. Sulphate of ammonia being heated in this compound to about 322° Fahr., pure azote gas, perfectly unmixed nitrous or nitric oxide, was obtained. This experiment was varied by passing nitric oxide gas into concentrated sulphuric acid mixed with sulphate of ammonia, and heated from about 300° Fahr. to 392°. The nitric oxide was decomposed as in the preceding experiment, and pure azotic gas was obtained; it is mixed with nitric oxide only when the disengagement is too rapid. M. Pelouze is of opinion that this method of preparing azotic gas may be advantageously employed.

CONCENTRATION OF NITRIC ACID BY MEANS OF SULPHURIC ACID. BY M. PELOUZE.

It is stated in different chemical treatises, that concentrated sulphuric acid decomposes nitric acid into water, with which it combines, and hyponitrous acid. M. Pelouze doubted the accuracy of this statement, and was convinced that it was erroneous, by observing nitric acid distil from a mixture of nitrate of ammonia with great excess of sulphuric acid at 212°.

500 parts of concentrated sulphuric acid were mixed with 100 of nitric acid of specific gravity 1·448; the mixture was slowly distilled, and yielded 88 parts of nitric acid of specific gravity 1·520; this product, freed from red vapour by a gentle heat, was mixed with six and a half times its weight of concentrated sulphuric acid, un-

accompanied with any sensible increase of temperature. The mixture was colourless, and yielded very dense white vapours of nitric acid. When heated to a temperature which never exceeded 302°, and was kept as near as possible to 212°, 82 parts of nitric acid of specific gravity 1·520 were distilled; its density remained 1·520, and its boiling point was from 185° to 188° Fahrenheit.

A third rectification with sulphuric acid effected no change either in the properties, density or colour of the nitric acid.—*Journal de Pharmacie*, t. xxvii. p. 275.

COMPOSITION OF SUGAR OF GELATIN.

M. Boussingault observes, that the existence of sugar of gelatin discovered by M. Braconnot had been questioned by several chemists. It was obtained by him by acting on glue with sulphuric acid; and on following his directions M. Boussingault obtained the sugar in question, and leucin. The composition and constitution of this sugar, according to M. Boussingault, are,—

	By experiment.	By calculation.	
Hydrogen........	6·44	6·36	H^{36}
Carbon..........	33·85	34·	C^{32}
Oxygen..........	39·71	39·59	O^{14}
Azote	20·	20·05	N^{8}
	100·00	100·00	

Sugar of gelatin is readily combined with oxide of silver; the compound forms colourless crystals, which are but slightly soluble in cold water. They consist of

Hydrogen	1·21	
Carbon................	13·66	
Oxygen	12·31	
Azote	8·07	
Silver	63·95	——99·2

Sugar of gelatin combines also with the oxides of copper and of lead; these two compounds are very soluble in water; the cupreous compound is a crystalline mass of an azure blue colour; that of oxide of lead crystallizes in fine colourless needles; its solution is totally decomposable by carbonic acid. The composition of both these compounds is analogous to that of the silver compound.—*Journal de Pharmacie*, t. xxvii. p. 35.

LACTATE OF UREA IN URINE.

MM. Cap and Henry some time since expressed it as their opinion that urea existed in urine, combined with lactic acid, and probably also the phosphoric; the correctness of these opinions having been questioned by M. Lecanu, the authors have again investigated the subject, and they are of opinion that the following experiments demonstrate the accuracy of their statement.

When fresh human urine is evaporated to about five-sixths of its bulk, at a temperature not exceeding 248° Fahr., there is obtained, on cooling, a bright, brownish, very acid liquor, which must be carefully filtered to deprive it of a deposit of a dirty white colour, formed during concentration. The liquid is again evaporated, with a gentle heat, to the consistence of a syrup, and then nearly to dryness, *in vacuo*, over some substance powerfully attractive of water. The residue is to be put into a stoppered glass bottle, and there are to be added to it cold, ten or twelve times its weight, of a mixture of two parts of sulphuric æther and one part of rectified alcohol; it is to be often shaken, and, after some days, the liquid, which has acquired an amber colour, is to be poured off; this liquid is very acid, and is to be shaken in a bottle, with a slight excess either of the carbonate of lime, of zinc, or of barytes, or bicarbonate of potash. From their reaction effervescence results, and there are produced lactate and phosphate of lime, zinc, barytes, or potash, by the saturation of the *free* lactic and phosphoric acid, which the urine contains.

The liquid æthereal portion, again filtered and exposed to a very gentle heat, soon yields in both cases, very fine prismatic crystals of lactate of urea, perfectly similar to those artificially prepared. These crystals are long transparent hexagonal prisms, have a sharpish taste, are volatile at a moderate heat, entirely decomposed when heated to redness on platina foil, very soluble in water, alcohol, and alcoholized æther, but less so in sulphuric æther. They attract moisture from the air powerfully, dissolve entirely in it, and form a bright coloured solution, which, when gently heated, again furnishes crystals. Oxalic and nitric acids form, when added to the solution, either laminated crystalline precipitates, or pearly acicular crystals. Hydrate of lime does not disengage ammonia, as immediately occurs with ammoniacal salts. In order to determine the state of combination of the lactic acid with the urea, the following experiments were made:—A quantity of the crystals obtained by the spontaneous evaporation of alcoholized æthereal liquor, were pressed on filtering paper, and divided into three equal portions, A, B, C.

A. These crystals, dissolved in pure water, were slightly heated with an excess of hydrate of zinc, recently prepared and dried by exposure to the air. The mixture was carefully evaporated to dryness, and treated with hydrated sulphuric æther, which dissolved the urea, and yielded it, after evaporation in the air, in crystals which were not hygrometric. The residue, not acted upon by this, was treated with hot distilled water, filtered and evaporated on a sand bath. This operation yielded white acicular styptic crystals of lactate of zinc.

B. Another portion of the crystals was treated with a solution of barytes; it was evaporated to dryness, and treated successively with sulphuric æther and weak alcohol. The æther dissolved the pure urea, and the alcohol dissolved the dry lactate of barytes; it was very soluble, decomposable by heat, and precipitated sulphates like other barytic salts.

C. Lastly, to the third portion of the crystals, dissolved in a small quantity of water, oxalic acid was added till the crystalline precipitation of oxalate of urea ceased. The solution was carefully evaporated to dryness, and the product was subjected to the action of æther mixed with a fifth of rectified alcohol.

The liquid, treated with excess of carbonate of lime, was left in contact in the cold with alcohol. Hot distilled water was afterwards shaken with the undissolved residue, and after having been filtered, evaporation was carefully conducted. The product of this last operation was a soluble salt, which crystallized on standing, and possessed all the well-known characters of lactate of lime. If excess of nitric acid be used instead of oxalic acid, oxalate of lime is obtained instead of lactate, for the nitric acid converts the lactic into oxalic acid.

MM. Cap and Henry observe, that it appears to them evidently to result from the preceding experiments, that after having separated the microcosmic salt from the evaporated urine, and saturated the lactic and phosphoric acid in excess, and having obtained a crystallized product by the intervention of alcoholized æther, in which product M. Lecanu admitted the presence of lactic acid and urea; that these crystals have the form and characters very distinct from those which belong to pure urea, and that they perfectly resemble those procured by the direct action of lactic acid on urea; lastly, that the hydrates of zinc, barytes, lime and oxalic acid, occasion in these crystals the production of lactates of zinc, barytes and lime, and separate lactic acid; they observe that it is impossible to demonstrate in a more direct manner the existence of a compound of these two principles.—*Journal de Pharmacie*, xxvii. 356.

METEOROLOGICAL OBSERVATIONS FOR JULY 1841.

Chiswick.—July 1. Overcast. 2. Hazy: overcast and fine: slight rain. 3. Hazy and mild: very fine. 4, 5. Very fine. 6. Rain: fine. 7. Fine: rain. 8. Fine: clear. 9. Very fine. 10. Fine: rain. 11. Overcast. 12. Cloudy and mild. 13. Cloudy. 14. Showery. 15. Heavy thunder-showers: constant heavy rain. 16. Cloudy. 17, 18. Fine. 19. Very fine. 20. Heavy rain. 21. Overcast: rain. 22. Cloudy. 23. Cloudy and fine. 24, 25. Cloudy. 26, 27. Light haze: fine. 28. Cloudy. 29. Cold and dry: cloudy: slight rain. 30. Fine but cool. 31. Rain: cloudy.—The quantity of rain which fell on the 15th was unusually great, amounting to nearly an inch and a half in the course of the twenty-four hours.

Boston.—July 1. Rain. 2. Cloudy. 3. Fine. 4. Cloudy: rain P.M. 5. Cloudy. 6. Rain and stormy: rain early A.M.: rain P.M. 7. Cloudy: rain P.M. 8. Fine. 9. Cloudy: rain P.M. 10. Fine: rain early A.M. 11. Cloudy. 12. Fine: rain P.M. 13. Cloudy: rain P.M. 14. Fine. 15, 16. Fine: rain P.M. 17. Fine. 18. Cloudy: rain P.M. 19. Fine: rain P.M. 20. Rain: rainy day. 21. Cloudy: rain P.M. 22. Cloudy: rain P.M., with thunder and lightning. 23. Cloudy: rain P.M. 24—28. Cloudy. 29. Cloudy and stormy: rain A.M. and P.M. 30. Fine. 31. Cloudy: thunder and lightning P.M.

Applegarth Manse, Dumfries-shire.—July 1. Fine: one shower. 2. Fine and fair, but cloudy. 3. Sunshine and rain. 4. Slight showers. 5. Rain all day. 6. Fair and fine. 7. Cloudy A.M.: fine P.M. 8. Fine: one shower. 9. The same. 10. Wet afternoon. 11. Slight showers. 12. Slight showers, but heavier. 13. Heavy showers: thunder. 14. Incessant showers. 15. Heavy rain till noon. 16. Fair and fine. 17. Fair but threatening. 18. Fair and warm: thunder. 19. Fine with a few drops. 20, 21. Rain P.M. 22. Slight showers. 23. Cloudy but fair. 24. Fine all day. 25. Remarkably fine. 26. Showers. 27. Showers P.M. 28. One slight shower. 29. Fair throughout. 30. Fair but threatening. 31. Fair with a few drops.

Meteorological Observations made at the Apartments of the Royal Society by the Assistant Secretary, Mr. ROBERTON; *by* Mr. THOMPSON *at the Garden of the Horticultural Society at Chiswick, near London; by* Mr. VEALL *at Boston, and by* Mr. DUNBAR *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. July.	Barometer.						Thermometer.									Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.		30·080	30·049	29·47	29·76	29·84				63	59	62·5	64	51		SW.	W.	W.		·05	·22	...	
2.		30·123	30·086	29·52	29·98	30·01				69	60	63·5	65	53½		W.	calm	E.		·01	·06	...	
○ 3.		30·126	29·989	29·58	29·95	29·82				79	55	67	64	56		SW.	calm	SSW.		·04	...	...	
4.		29·999	29·963	29·37	29·80	29·95				69	53	66·5	64	55		SW.	calm	E.		...	...	...	
5.		30·064	29·929	29·55	29·97	29·65				75	57	61	59	48½		NE.	calm	SE & S.		·59	·03	...	
6.		29·805	29·635	29·03	29·58	29·68				72	52	60	64	52		W.	W.	NW.		·03	1·27	...	
7.		29·885	29·639	29·31	29·65	29·55				67	53	61	59	50		SW.	W.	W.		·08	·03	0·78	
8.		29·898	29·742	29·20	29·63	29·70				67	45	58	60½	47½		W.	NW.	W.		·01	·16	...	
9.		29·949	29·939	29·44	29·75	29·79				71	44	60	57	45½		W.	W.	WNW.		...	...	...	
10.		29·926	29·492	29·44	29·69	29·20				71	49	58	55½	45		SW.	calm	SW.		·36	·09	...	
☾ 11.		29·572	29·266	28·77	29·27	29·39				70	43	57·5	59½	47½		NW.	calm	NNE.		...	·26	...	
12.		29·594	29·578	29·05	29·43	29·46				64	44	58	40	45		W.	W.	NNE.		·02	...	...	
13.		29·719	29·630	29·13	29·49	29·55				68	42	58	59	48		W.	calm	NW.		·02	·06	...	
14.		29·726	29·570	29·25	29·56	29·57				66	45	60	57½	50		SW.	W.	NW.		·07	·04	1·80	
15.		29·742	29·646	29·16	29·58	29·71				68	51	50	61	48½		S.	calm	NE.		1·46	...	...	
16.		29·977	29·874	29·40	29·86	29·87				72	44	61	61	47½		N.	calm	NE.		·01	·03	...	
17.		30·013	29·820	29·53	29·85	29·75				75	49	63	63	40½		S.	calm	SSW.		...	·17	...	
● 18.		29·742	29·634	29·24	29·68	29·70				75	48	64	67	49		NW.	calm	W.		...	...	...	
19.		29·813	29·795	29·30	29·69	29·65				73	54	62	66	51½		W.	calm	SW.		·07	·01	...	
20.		29·699	29·493	29·15	29·49	29·30				63	55	59	64	52		SW.	W.	ESE.		·29	·09	...	
21.		29·665	29·469	28·92	29·33	29·43				67	54	63·5	64	53		SW.	W.	NW.		·10	·28	0·49	
22.		29·828	29·724	29·16	29·55	29·70				67	51	61	58½	48½		W.	W.	NNW.		·22	·14	...	
23.		29·999	29·915	29·39	29·82	29·96				64	53	56	62	49		NW.	W.	E.		·01	·65	...	
24.		30·133	30·085	29·66	30·05	30·08				60	50	55	62	51		NE.	NW.	NW.		...	·02	...	
☽ 25.		30·127	30·071	29·65	30·06	30·00				68	51	58	62	51		NE.	calm	W.		...	...	...	
26.		30·074	30·022	29·56	29·98	29·94				72	53	61	66	51		SW.	calm	SW.		...	...	...	
27.		29·990	29·943	29·45	29·85	29·62				70	49	58·5	55	47		NW.	NW.	SW.		...	...	...	
28.		29·839	29·774	29·20	29·53	29·55				72	47	61	60	51½		SW.	W.	W.		...	...	0·05	
29.		29·760	29·722	29·09	29·46	29·48				65	48	55	58½	48		W.	W.	W.		·01	...	...	
30.		29·695	29·621	29·10	29·49	29·40				63	48	58	56	45½		NW.	W.	WNW.		...	·13	...	
31.		29·622	29·576	29·03	29·35	29·54				62	44	54	60	48½		W.	W.	NW.		·11	...	...	
Mean.		29·877	29·764	29·29	29·681	29·682				68·61	50·00	60	61	49·1						3·50	3·74	3·32	

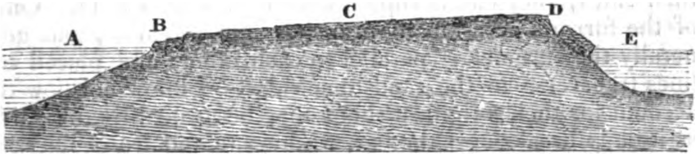
THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

OCTOBER 1841.

XXXVII. *On a remarkable Bar of Sandstone off Pernambuco, on the Coast of Braxil. By* C. Darwin, *Esq.*, *M.A.*, *F.R. & G.S.**

IN entering the harbour of Pernambuco, a vessel passes close round the point of a long reef, which, viewed at high water when the waves break heavily over it, would naturally be thought to be of coral-formation, but when beheld at low water it might be mistaken for an artificial breakwater, erected by cyclopean workmen. At low tide it shows itself as a smooth level-topped ridge, from thirty to sixty yards in width, with even sides, and extending in a *perfectly* straight line, for several miles, parallel to the shore. Off the town it includes a shallow lagoon or channel about half a mile in width, which further south decreases to scarcely more than a hundred yards. Close within the northern point ships lie moored alongside the reef to old guns let into it.



Transverse section: vertical heights considerably exaggerated.

A. Level of the sea at low water.
B. Subsided masses, thickly coated with Serpulæ, &c.
C. Summit of the bar, which generally slopes a little seaward; but the slope in the woodcut has been unintentionally somewhat increased.
D. Subsided masses of bare sandstone.
E. Surface of the harbour or lagoon.

---

* Communicated by the Author.

The accompanying woodcut represents, at low water spring tides, a transverse section of the northern part of the bar, where a section of about seven feet in height is exhibited on the inner side. It consists of a hard pale-coloured sandstone, breaking with a very smooth fracture, and formed of siliceous grains, cemented by calcareous matter. Well-rounded quartz pebbles, from the size of a bean, rarely to that of an apple, are imbedded in it, together with a very few fragments of shells. Traces of stratification are obscure, but there was an included layer in one spot of stalactitic limestone, an eighth of an inch in thickness. In another place some false strata, dipping landwards at an angle of 45°, were capped by a horizontal mass. On each side of the ridge quadrangular fragments have subsided, as shown in the woodcut; and the whole mass is in some places fissured, apparently from the washing out of some soft underlying bed. One day, at low water, I walked a full mile along this singular, smooth, and narrow causeway, with water on both sides of me, and could see that for nearly a mile further its form remained unaltered. In Baron Roussin's beautiful chart of Pernambuco (*Le Pilote du Brésil*) it is represented as stretching on, in an absolutely straight line, for several leagues; how far its composition remains the same, I know not; but from the accounts I received from intelligent native pilots, it seems to be replaced on some parts of the coast by true coral reefs.

The upper surface, though on a large scale it must be called smooth, yet presents, from unequal disintegration, numerous small irregularities. The larger imbedded pebbles stand out supported on short pedestals of sandstone. There are, also, many sinuous cavities, two or three inches in width and depth, and from six inches to two feet in length. The upper edges of these furrows sometimes slightly overhang their sides; they end abruptly, but in a rounded form. One of the furrows occasionally branches into two arms, but generally they are nearly parallel to each other, and placed in lines transverse to the sandstone ridge. I know not how to account for their origin, without they be formed by the surf, as it daily breaks over the bar, washing to and fro pebbles in depressions, originally only slight. Opposed to this notion is the fact, that some of them were lined with numerous small living *Actineæ*. I have copied this passage, as I at the time wrote it, because furrows of a somewhat similar nature on the surface of rocks have lately received much attention, and are supposed invariably to indicate the former action of a waterfall, over the edge of a moving glacier.

The exterior part of the bar is coated with a thin layer of calcareous matter; this, on the outer subsided masses, which can only be reached between the successively breaking waves at low water, is so thick, that I could seldom expose the sandstone with a heavy hammer. I procured, however, some fragments where the layer was between three and four inches in thickness; it consists chiefly of small *Serpulæ*, including some *Balani*, and a few very thin paper-like layers of a *Nullipora*. The surface alone is alive, and all within consists of the above organic bodies filled up with dirty white calcareous matter. The layer, though not hard, is tough, and from its rounded surface resists the breakers. Along the whole external margin of the bar, I only saw one very small point of sandstone which was exposed to the surf. In the Pacific and Indian Oceans the outer and upper margin of the coral reefs are protected, as will be described in a forthcoming work, by a very similar coating; but there it is almost exclusively formed of several species of *Nulliporæ*. Lieut. Nelson, in his excellent memoir on the Bermudas (Geol. Trans., vol. v. part 1. p. 117), has described reefs, formed, as he states, but I cannot avoid suspecting only coated, by similar masses of *Serpulæ*. I inquired from some old pilots, whether there was any tradition of change in the form and dimensions of this sandstone bar; but they were unanimous in answering me in the negative. It is astonishing to reflect, that although waves of turbid water, charged with sediment, are driven night and day, by the ceaseless trade-wind, against the abrupt edges of this natural breakwater, yet that it has lasted in its present perfect state for centuries, or more probably thousands of years. Seeing that the surface on the inner side does gradually wear away, as shown by the pebbles on the sandstone pedestals, this durability must be entirely owing to the protection afforded by the thin coating of *Serpulæ* and other organic beings: it is a fine example, how apparently inefficient, yet how effectual, are the means of preservation, like those of destruction, which nature employs.

I believe similar bars of rock occur in front of some of the other bays and rivers on the coast of Brazil: Baron Roussin states that at Porto Seguro there is a "*quay*" similar to that of Pernambuco. Spaces of several hundred miles in length on the shores of the Gulf of Mexico, the United States, and southern Brazil are formed by long narrow islands and spits of sand, including very extensive shallow lagoons, some of which are several leagues in width. The origin of these linear islets is rather obscure: Prof. Rogers (Report to British Association, vol. iii. p. 13.) gives some reasons for suspecting that they have been formed by the upheaval of shoals, deposited where currents met. These phænomena, it is very probable, are connected in their origin with the same causes which have produced the remarkable bar of sandstone off Pernambuco. The town of Pernambuco stands on a low narrow islet and on a long spit of sand, in front of a very low shore, which is bounded in the distance by a semicircle of hills. By digging at low water near the town the sand is found consolidated into a sandstone, similar to that of the breakwater, but containing many more shells. If, then, the interior of a long sandy beach in one part, and in another the nucleus of a bar or spit extending in front of a bay became consolidated, a small change, probably of level, but perhaps simply in the direction of the currents, might give rise, by washing away the loose sand, to a structure like that in front of the town of Pernambuco, and along the coast southward of it; but without the protection afforded by the successive growth of organic beings, its duration would be short, if indeed it were not destroyed before being completely exhibited.

XXXVIII. *On the Heat evolved by Metallic Conductors of Electricity, and in the Cells of a Battery during Electrolysis. By* JAMES PRESCOTT JOULE, *Esq.*[*]

1. THERE are few facts in science more interesting than those which establish a connexion between heat and electricity. Their value, indeed, cannot be estimated rightly, until we obtain a complete knowledge of the grand agents upon which they shed so much light. I have hoped, therefore, that the results of my careful investigation on the heat produced by voltaic action, are of sufficient interest to justify me in laying them before the Royal Society.

CHAP. I.—*Heat evolved by Metallic Conductors.*

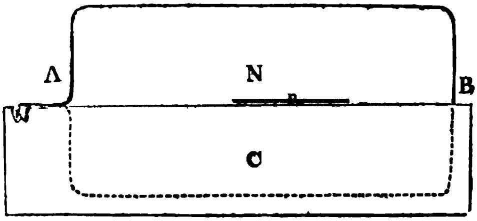
2. It is well known that the facility with which a metallic wire is heated by the voltaic current is in inverse proportion to its conducting power, and it is generally believed that this proportion is exact; nevertheless I wished to ascertain the fact for my own satisfaction, and especially as it was of the utmost importance to know whether resistance to conduction is the *sole* cause of the heating effects. The detail, therefore, of some experiments confirmatory of the law, in addition to those already recorded in the pages of science, will not, I hope, be deemed superfluous.

---

[*] Communicated by the Author.

3. It was absolutely essential to work with a *galvanometer*, the indications of which could be depended upon, as marking definite quantities of electricity. I bent a rod of copper into

Fig. 1.



the shape of a rectangle (A B, fig. 1.), twelve inches long, and six inches broad. This I secured in a vertical position by means of the block of wood C; N is the magnetic needle, 3¾ inches long, pointed at its extremities, and suspended upon a fine steel pivot over a graduated card placed a little before the centre of the instrument.

4. On account of the large relative size of the rectangular conductor of my galvanometer, the tangents of the deviations of the needle are very nearly proportional to the quantities of current electricity. The small correction which it is necessary to apply to the tangents, I obtained by means of the rigorous experimental process which I have some time ago described in the 'Annals of Electricity*'.

5. I have expressed my quantities of electricity on the basis of Faraday's great discovery of definite electrolysis, and I venture to suggest, that that quantity of current electricity which is able to electrolyze an atomic element expressed in grains in one hour of time, be called a *degree*. Now by a number of experiments I found that the needle of my galvanometer deviated 33°·5 of the graduated card, when a current was passing in sufficient quantity to decompose nine grains of water per hour; that deviation, therefore, indicates one *degree of current electricity* on the scale that I propose to be adopted. We shall see in the sequel some of the practical advantages which I have had by using this measure.

6. The thermometer which I used had its scale graduated on the glass stem. The divisions were wide, and accurate. In taking temperatures with it, I stir the liquid gently with a feather; and then, suspending the thermometer by the top of its stem, so as to cause it to assume a vertical position, I bring my eye to a level with the top of the mercury. In this

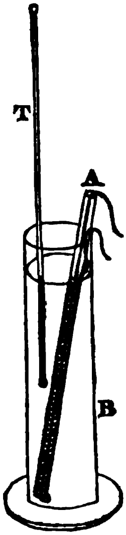
---

* Vol. iv. pp. 131–132, and 476.

way a little practice has enabled me to estimate temperatures to the tenth part of Fahrenheit's degree with certainty.

7. In order to ascertain the heating power of a given metallic wire, it was passed through a thin glass tube, and then closely coiled upon it. The extremities of the coil thus formed were then drawn asunder, so as to leave a small space between each convolution, and if this could not be well done, a piece of cotton thread was interposed. The apparatus thus prepared, when placed in a glass jar containing a given quantity of water, was ready for experiment. Fig. 2 will explain the dispositions: A is the coil of wire; B the glass jar partly filled with water; T represents the thermometer. When the voltaic electricity is transmitted through the wire, no appreciable quantity passes from it to take the shorter course through the water. No trace of such a current could be detected, either by the evolution of hydrogen, or the oxidation of metal.

Fig. 2.



8. Previous to each of the experiments, the necessary precaution was taken of bringing the water in the glass jar, and the air of the room to the same temperature. When this is accurately done, the results of the experiments bear the same proportions to one another as if no extraneous cooling agents, such as radiation, were present; for their effects in a given time are proportional to the difference of the temperatures of the cooling and cooled bodies; and hence, although towards the conclusion of some experiments this cooling effect is very considerable, the *absolute quantities* alone of heat are affected, not the *proportions* that are generated in the same time. [See the table of heats produced during half an hour and one hour, p. 264.]

9. Exp. 1.—I took two copper wires, each two yards long, one of them $\frac{1}{28}$th of an inch, the other $\frac{1}{30}$th of an inch thick, and arranged them in coils in the manner that I have described (7.). These were immersed in two glass jars, each of which contained nine ounces avoirdupois of water. A current of the mean quantity 1°·1 Q[*], was then passed consecutively through both coils, and at the close of one hour I observed that the water in which the thin wire was immersed had gained 3°·4, whilst the thick wire had produced only 1°·3.

---

[*] I place Q at the end of my *degrees*, to distinguish them from those of the graduated card.

10. Now by direct experiment, I found that three feet of the thin wire could conduct exactly as well as eight feet of the thick wire; and hence it is evident that the resistances of two yards of each were in the ratio of 3·4 to 1·27, which approximates very closely to the ratio of the heating effects exhibited by the experiment.

11. Exp. 2.—I now substituted a piece of iron wire $\frac{1}{27}$th of an inch thick, and two yards long, for the thick copper wire used in Exp. 1, and placed each coil in half a pound of water. A current of 1°·25 Q was passed through both during one hour, when the augmentation of temperature caused by the iron was 6°, whilst that produced by the copper wire was 5°·5. In this case the resistances of the iron and copper wires were found to be in the ratio of 6 to 5·51.

12. Exp. 3.—A coil of copper wire was then compared with one of mercury, which was accomplished by inclosing the latter in a bent glass tube. In this way I had immersed, each in half a pound of water, 11¼ feet of copper wire $\frac{1}{50}$th of an inch thick, and 22¾ inches of mercury 0·065 of an inch in diameter. At the close of one hour, during which the same current of electricity was passed through both, the former had caused a rise of temperature of 4°·4, the latter of 2°·9. The resistances were found by a careful experiment to be in the ratio of 4·4 to 3.

13. Other trials were made with results of precisely the same character; they all conspire to confirm the fact, that *when a given quantity of voltaic electricity is passed through a metallic conductor for a given length of time, the quantity of heat evolved by it is always proportional to the resistance*[*] *which it presents, whatever may be the length, thickness, shape, or kind of that metallic conductor.*

14. On considering the above law, I thought that the effect produced by the increase of the intensity of the electric current would be as the square of that element, for it is evident that in that case the resistance would be augmented in a double ratio, arising from the increase of the *quantity* of electricity passed in a given time, and also from the increase of the *velocity* of the same. We shall immediately see that this view is actually sustained by experiment.

15. I took the coil of copper wire used in Exp. 3, and have found the different quantities of heat gained by half a pound of water in which it was immersed, by the passage of electri-

---

[*] Mr. Harris, and others, have proved this law very satisfactorily, using common electricity.

cities of different degrees of tension. My results are arranged in the following table:—

Mean Deviations of the Needle of the Galvanometer.	Quantities of Current Electricity expressed in Degrees (5.)	Quantities of Heat produced in half an hour by the Intensities in Column 2.	Ratio of the Squares of the Intensities in Column 2.	Quantities of Heat produced in one hour by the Intensities in Column 2.	Ratio of the Squares of the Intensities in Column 2.
16°	0°·43 Q	°	......	1°·2	1
31½	0·92 Q	3	2·9	4·7	4·55
55	2·35 Q	19·4	18·8		
57⅔	2·61 Q	23	23·2		
58½	2·73 Q	25	25·4	39·6	40·

16. The differences between the numbers in columns three and five, and those in columns four and six, are very inconsiderable, taking into account the nature of the experiments, and are principally owing to the difficulty which exists in keeping the air of the room in the same state of quiet, of hygrometry, &c. during the different days on which the experiments were made. They are much less when a larger quantity of water is used, so as to reduce the cooling effects (28.).

17. We see, therefore, that *when a current of voltaic electricity is propagated along a metallic conductor, the heat evolved in a given time is proportional to the resistance of the conductor multiplied by the square* [*] *of the electric intensity.*

18. The above law is of great importance. It teaches us the right use of those instruments which are intended to measure electric currents by the quantities of heat which they evolve. If such instruments be employed (though in their present state they are far inferior in point of accuracy to many other forms of the galvanometer), it is obvious that the *square roots* of their indications are alone proportional to the intensities which they are intended to measure.

19. By another important application of the law, we are now enabled to compare the frictional [†] and voltaic electri-

---

[*] The experiments of De la Rive show that the calorific effect of the voltaic current increases in a much greater proportion than the simple ratio of the intensities.—*Ann. de Chimie*, 1836, part i. p. 193. See also Peltier's results, *Ann. de Chimie*, 1836, part ii. p. 249.

[†] The experiments of Brooke, Cuthbertson and others, prove that the quantity of wire melted by common electricity is as the square of the battery's charge. Harris, however, arrived at the conclusion, that the heating power of electricity is *simply* as the charge, Phil. Trans., 1834,

cities, in such a manner as to determine their elements by the quantity of heat which they evolve in passing along a given conductor; for if a certain quantity of voltaic electricity produce a certain degree of heat by passing along a given conductor, and if the same quantity of heat be generated by the discharge of a certain electrical battery along the same conductor, the product of the quantity and velocity of transfer of the *voltaic* electricity will be equal to the product of the quantity and velocity of the *frictional* electricity; or $Q\,V = q\,V$, whence $\frac{Q}{q} = \frac{v}{V}$.

CHAP. II.—*Heat evolved during Electrolysis.*

20. Under the above head, I shall now examine the heat produced in the cells of the battery, and when electrolytes are experiencing the action of the voltaic current. It has been my desire to render these experiments strictly comparable, both with themselves and with those of other philosophers. I have therefore taken care to apply the corrections which either specific heat, or other disturbing causes might require, and have by these means been able to express, in every case, the *total* amount of evolved heat.

21. The first of these corrections, which I call Cor. A, arises from the difference between the mean temperature of the liquid used in an experiment, and that of the surrounding atmosphere. Its amount is determined by ascertaining the rapidity with which the temperature of the liquid is reduced at the end of each experiment.

22. The second correction (Cor. B) is for the specific heat of the liquids, and the vessels which contain them; and when the necessary data could not be found in the tables of specific heat, I have supplied them from my own experiments. The *vessels* were white earthenware jars, 4½ inches deep, and 4½ inches in diameter: their caloric was one-twelfth of that contained by two pounds of water, *to which capacity I have reduced all my subsequent results.*

23. As resistance to conduction is the sole cause of the heat produced in the connecting wire of the voltaic battery, it was natural to expect that it would act an important part in this second class of phænomena also. It was important, therefore, to begin by determining the amount of heat evolved by that quantity of conducting metal which I found it convenient to adopt as a *standard of resistance.*

---

p. 225. Of course the remark in the text is made on the presumption, that when the proper limitations are observed, the calorific effect of electricity is as the square of the charge of any given battery.

24. Ten feet of copper wire, 0·024 of an inch thick, were formed into a coil in the manner described in (7.); its resistance to conduction was called *unity*. Three experiments were made in order to ascertain its heating power.

25. 1st. A jar was filled with two pounds of water, and a current which produced a mean deviation of the needle of the galvanometer (3.) equal to $57\frac{1}{4}^\circ = 2^\circ{\cdot}54$ Q of current electricity, was urged through the coil for twenty-seven minutes, by means of a zinc-iron[*] battery of ten pairs. The heat thus acquired by the water, after Cor. A, and that part of Cor. B which relates to the caloric of the jar, had been applied, was 6°·22.

26. 2nd. The battery was now charged with a weaker solution of sulphuric acid. In this case it passed the mean current 2°·085 Q during forty-five minutes. The heat thus produced, when corrected, was 7°·04.

27. 3rd, A battery of five pairs (three of which had platinized silver; one silver, and one copper, for their negative plates,) passed the mean current 1°·88 Q during one hour, in which time 7°·47 were generated.

28. When the first two experiments are reduced, in order to compare them with the third, we have, in accordance with the principles laid down in (17.), $\frac{(1{\cdot}88)^2}{(2{\cdot}54)^2} \times \frac{60'}{27'} \times 6^\circ{\cdot}22 = 7^\circ{\cdot}57$, and $\frac{(1{\cdot}88)^2}{(2{\cdot}085)^2} \times \frac{60'}{45'} \times 7^\circ{\cdot}04 = 7^\circ{\cdot}63$. Thus we have $\frac{7^\circ{\cdot}57 + 7^\circ{\cdot}63 + 7^\circ{\cdot}47}{3} = 7^\circ{\cdot}56$, the mean and total quantity of heat produced per hour by the passage of 1°·88 Q of current electricity, against the *unit* of resistance.

29. Before I proceed to give an account of some experiments on heat evolved in the cells of voltaic pairs, it is important to observe that every kind of action not essentially electrolytic must be eliminated. For instance, the dissolution of metallic oxides in acid menstrua, which has been proved by Dr. Faraday to be no cause of the current, is the occasion of a very considerable quantity of heat, which, if not accounted for in the experiments, would altogether disturb the results. I have taken the oxide of zinc, prepared either by igniting the nitrate, or by burning the metal, and have repeatedly dissolved it in sulphuric acid of various specific gravities; and on taking the mean of many experiments, none of which dif-

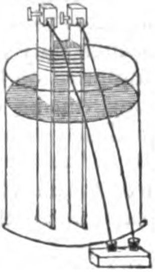
---

[*] Whenever an iron battery was used, it was of course placed at a distance from the galvanometer sufficiently great to render its action on the needle altogether inappreciable.

fered materially from the rest, I have found that the total corrected heat produced by the dissolution of 100 grains of the oxide of zinc in sulphuric acid, is able to raise two pounds of water 3°·44.

30. Exp. 1.—I constructed a single voltaic pair, consisting of thin plates of amalgamated zinc and platinized silver (Mr. Smee's arrangement): the plates were two inches broad, and were kept one inch asunder by means of a piece of wood, to the opposite sides of which they were bound with string: to the top of each plate, a thick copper wire formed a good metallic connexion, by means of a brass clamp. The voltaic pair, thus prepared, was immersed in two pounds of sulphuric acid, sp. gr. 1137, contained by one of the earthenware jars (22.). The arrangement is represented by fig. 3.

Fig. 3.



31. When the circuit was completed so as to present to the current the total metallic resistance 0·06, the galvanometer stood at $49\frac{1}{3}^\circ$ = 1°·84 Q; and at $17\frac{1}{2}^\circ$ = 0°·453 Q, when the total metallic resistance was increased to 1·16 by the addition to the circuit of ten feet of thin copper wire. Hence, according to the principles laid down by Ohm, $\frac{1{\cdot}84}{r+1{\cdot}16} = \frac{0{\cdot}453}{r+0{\cdot}06}$; from which r, the resistance of the voltaic pair, = 0·299. Immediately after this trial, the temperature of the liquid being exactly 49°, and that of the air 50°·2, the circuit was completed for one hour, during which the needle first advanced a little from 50°, and then declined to 46°, the average* deviation was 48° 44′ = 1°·8 Q. The temperature of the liquid was then 53°·7, indicating a rise of 4°·7. Another trial now gave $\frac{1{\cdot}59}{r'+1{\cdot}16} = \frac{0{\cdot}382}{r'+0{\cdot}06}$; whence r', the resistance of the pair at the close of the experiment, = 0·288: the mean resistance of the pair was therefore 0·293.

32. Now in order to obtain the total amount of heat evolved by the pair, reduced to the capacity of two pounds of water, we have 4°·7 + 0°·4 (on account of Cor. A (21.)) and − 0°·5 (on account of Cor. B (22.)) = 4°·6. The correction due to the dissolution of oxide of zinc is found by multiplying its

---

* During each experiment the deflections of the needle were noted at intervals of five minutes, or less. From thence I deduce my averages.

quantity by $\frac{3^\circ{\cdot}44}{100}$ (see (29.)); the quantity of the oxide being obtained by multiplying the equivalent of oxide of zinc by the mean quantity of current electricity. We have then

$$40{\cdot}3 \times 1{\cdot}8 \times \frac{3^\circ{\cdot}44}{100} = 2^\circ{\cdot}5:$$ this, when subtracted from $4^\circ{\cdot}6$, leaves $2^\circ{\cdot}1$, the *correct voltaic heat.*

33. Assuming in this case, as well as in that of a metallic conductor, that the heat evolved is proportional to the resistance multiplied by the square of the electric intensity, we have, from the data in (28.) and (31.), $\frac{(1{\cdot}8)^2}{(1{\cdot}88)^2} \times 0{\cdot}293 \times 7^\circ{\cdot}56 = 2^\circ{\cdot}03$, which is very near $2^\circ{\cdot}1$, the heat deduced from experiment.

34. Exp. 2.—I now constructed another pair, consisting of plates precisely similar to those used in Exp. 1, but half an inch only asunder: it was also immersed in two pounds of sulphuric acid, sp. gr. 1137. The circuit was closed for one hour, during which the needle of the galvanometer advanced gradually from $47\frac{1}{2}^\circ$ to $50\frac{1}{3}^\circ$, the mean deviation being $49^\circ\ 35' = 1^\circ{\cdot}84$ Q. The liquid had then gained $4^\circ{\cdot}8$: this, $+0^\circ{\cdot}1$ (for Cor. A) and $-0^\circ{\cdot}5$ (for Cor. B), $= 4^\circ{\cdot}4$. The heat due to the dissolution of oxide of zinc is in this case

$$40{\cdot}3 \times 1{\cdot}84 \times \frac{3^\circ{\cdot}44}{100} = 2^\circ{\cdot}55,$$ which, when subtracted from $4^\circ{\cdot}4$, leaves the correct voltaic heat $1^\circ{\cdot}85$.

35. The resistance of the pair was ascertained in this, as in every other instance, at the beginning and at the end of the experiment. The equations thus obtained were $\frac{1{\cdot}714}{r + 1{\cdot}16} = \frac{0{\cdot}432}{r + 0{\cdot}06}$, and $\frac{1{\cdot}91}{r' + 1{\cdot}16} = \frac{0{\cdot}446}{r' + 0{\cdot}06}$; whence $r = 0{\cdot}311$, and $r' = 0{\cdot}275$: the mean resistance was therefore $0{\cdot}293$. Now, calculating as before (33.), on the basis of the heat produced by the passage of electricity against the standard of resistance, we have $\frac{(1{\cdot}84)^2}{(1{\cdot}88)^2} \times 0{\cdot}293 \times 7^\circ{\cdot}56 = 2^\circ{\cdot}12$.

36. Exp. 3.—I formed another pair on Mr. Smee's plan; it was similar to the last, with the exception that the plates were only one inch broad. When the circuit was closed, a current of the mean intensity $1^\circ{\cdot}46$ Q passed through the apparatus during one half hour. The heat thereby produced,

when corrected, and reduced on account of the dissolution of oxide of zinc, was 0°·84.

37. In this instance the mean resistance was 0·32; whence, by a calculation precisely similar to those given under Exps. 1 and 2, we have the theoretical amount of heat = 0°·74.

38. The three instances above given, are specimens taken from a number of experiments with the platinized silver pairs. The mean of the eight unexceptionable experiments which I have made with them, gives 2°·08 of actual, and 2°·13 of theoretical heat, and not one of the individual experiments presented a greater difference between real and calculated heat, than Exp. 2.

39. Exp. 4.—A plate of copper, four inches broad, was bent about a plate of amalgamated zinc three inches and a half broad, so as to form a pair of Wollaston's double battery. It was placed in a jar containing two pounds of dilute sulphuric acid. In this instance, the total voltaic heat that was generated was 1°·2, the calculated result being 1°·0 only. Repeated experiments with the copper pairs gave similar results, the real heat being invariably somewhat superior to that which the doctrine of resistances would demand. The cause of this I have found in a slight local action, which it is almost impossible to avoid in the common copper battery.

40. Exp. 5.—I now constructed a single pair on Mr. Grove's plan. The platinum, two inches broad, was immersed in an ounce and a half of strong nitric acid contained by a 4-inch pipe-clay cell; the amalgamated zinc plate, also two inches broad, was immersed (at the distance of an inch and a half from the platinum) in thirty ounces of sulphuric acid, sp. gr. 1156. The whole was contained by one of the jars (22.).

41. A trial, made first as usual, in order to ascertain the resistance of the pair, gave $\frac{4\cdot4}{r+2\cdot26} = \frac{0\cdot816}{r+0\cdot06}$, whence $r = 0\cdot441$. As soon as the slight heat acquired during the above trial was equably diffused through the apparatus, the thermometer placed in the dilute sulphuric acid stood at 51°·95, the temperature of the air being 52°·4. The circuit was then immediately closed for ten minutes, during which time the needle of the galvanometer advanced steadily from 68° 40′ to 71° 20′; the mean deviation being 70° 9′ = 4°·77 Q. As soon as the heat thus generated was equably diffused*,

---

* By gently stirring the dilute sulphuric acid with a feather, so as to bring every part in successive contact with the porous cell during two minutes.

the thermometer immersed in the dilute sulphuric acid stood at 56°·7, indicating a rise of 4°·75. Another trial now gave $\frac{5{\cdot}14}{r'+2{\cdot}26} = \frac{0{\cdot}91}{r'+0{\cdot}06}$, whence $r' = 0{\cdot}413$. The mean resistance of the pair was therefore 0·427.

42. 4°·75 + 0°·1 (for Cor. A), and — 0°·4 (for Cor. B, which in this case includes the capacity for heat of the porous cell) = 4°·45. The heat generated by the dissolution of oxide of zinc was in this case $40{\cdot}3 \times 4{\cdot}77 \times \frac{3^\circ{\cdot}44}{100} \times \frac{10}{60'}$ $= 1^\circ{\cdot}1$, which, subtracted from 4°·45, leaves the correct voltaic heat 3°·35.

43. The *theoretical* result is $\frac{(4{\cdot}77)^2}{(1{\cdot}88)^2} \times 0{\cdot}427 \times 7^\circ{\cdot}56 \times \frac{10'}{60'}$ $= 3^\circ{\cdot}46$.

44. Exp. 6 was made with a pair in every respect similar to the last: the circuit, however, was completed by means of a thin copper wire, in order to reduce the intensity of the current. At the end of one hour, during which the needle of the galvanometer advanced gradually from 41° to 42°, the correct voltaic heat that was generated was 1°·7. The *theoretical* result was 1°·82.

45. I was desirous of knowing how far the same principles would apply to the heat generated in Prof. Daniell's constant battery. But in this battery considerable *cold* is produced, in consequence of the separation of oxide of copper from the sulphuric acid to which it is combined. This is altogether a secondary effect, and should be eliminated as decidedly as the *heat* produced by the dissolution of oxide of zinc. I have not yet been able to obtain accurate data for the correction thus needed, and shall therefore content myself with remarking, that my results with Mr. Daniell's arrangements are, as far as they go, quite consistent with the theory of resistances.

46. Experiments, such as I have related, were varied in many ways; and sometimes a number of pairs were arranged so as to form a battery. Still the results were similar, and established the fact, that *the heat which is generated in a given time in any pair, by true voltaic action, is proportional to the resistance to conduction of that pair, multiplied by the square of the intensity of the current.*

---

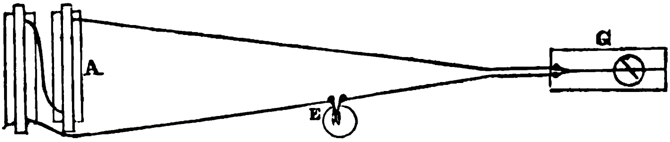
47. I now made some experiments on the heat consequent to the passage of voltaic electricity through electrolytes.

48. Exp. 7.—Two pieces of platinum foil, each of which was an inch long, and a quarter of an inch broad, were her-

metically sealed into the ends of two pieces of glass tubing: within these tubes, pieces of copper wire were metallically connected with the platinum; these, when the apparatus was in action, terminated in mercury cups. The tubes thus prepared were bound together by thread, so as to keep the pieces of platinum foil at the constant distance of half an inch asunder. This apparatus was immersed in two pounds of dilute sulphuric acid, sp. gr. 1154, contained in one of the jars (22.).

49. A battery of twenty-four inch, double iron-zinc plates, was then placed, with its divided troughs (which were charged with a pretty strong solution of sulphuric acid), at a distance from the galvanometer sufficiently great to obviate any disturbing effect on the needle. To the electrodes of this battery thick copper wires were secured, so that by means of one of them connexion could be made to the galvanometer, and by means of the other, to the decomposing cell. In fig. 4, A represents the battery, G the galvanometer, and E the decomposing apparatus (48.).

Fig. 4.



50. In order to ascertain the resistances of the battery or of the cell, I provided several coils* of silked copper wire, the resistances of which had been determined by careful experiments. When these were traversed by the current, they were placed in such a position as to prevent any action on the needle, and at the same time they were kept under water, in order to prevent them from becoming hot, which would have had the effect of increasing their resistances.

51. When everything was duly prepared, the battery was placed in its troughs, and the current from it was urged through the galvanometer and each of three of the coils, which were placed in succession at E (the decomposing apparatus having been removed). The resistances of these coils were 4·4, 5·5, and 7·7, and the currents which they allowed to pass were 1°·88 Q, 1°·65 Q, and 1°·29 Q.

52. The decomposing apparatus was now replaced, and the proper connexions being made, electrolytic decomposition

---

* Two of these coils had been previously employed (31. 41.), &c. in ascertaining the resistances of the voltaic pairs: the resistance 0·06 was that of the galvanometer and connecting wires.

was allowed to proceed during twenty minutes, in which time the needle of the galvanometer gradually declined from 55° to 48½°, the mean current being 1°·9 Q. The temperature of the liquid had now advanced from 46°·6 to 53°·95, indicating an increase of 7°·35. The temperature of the surrounding atmosphere was 46°·4.

53. The decomposing cell was now removed again, and the several coils, of which the resistances were, as before, 4·4, 5·5, and 7·7, were successively put in its place. The battery now urged through them, 1°·73 Q, 1°·48 Q, and 1°·22 Q.

54. In this case 7°·35 + 0°·55 (for Cor. A) and − 0°·64 (for Cor. B) = 7°·26, the heat which was generated in the decomposing jar.

55. The mean intensity of the current when passing through the coil of which the resistance was 4·4, was $\frac{1^\circ\cdot 88 + 1^\circ\cdot 73}{2} = 1^\circ\cdot 805$ Q, but 1°·9 Q when it passed through the decomposing cell. Hence $(4\cdot 4 + 3\cdot 15^*)\frac{1\cdot 805}{1\cdot 9} = 7\cdot 17$, this, $-3\cdot 15^*$, leaves 4·02, the amount of *obstruction* presented by the decomposing cell.

56. Now we must remember, that when the electric current was passing through the coils, it was urged by the whole intensity of the battery; but that in the case of the decomposing cell, a part of the intensity of the zinc-iron battery, equal (as I have found by experiment) to 3⅓ pairs, or to one sixth part of the whole, is occupied solely in overcoming the *resistance to electrolyzation* † of water in the decomposing cell. In order therefore to deduce the true *resistance to conduction*, we must subtract $\frac{4\cdot 02 + 3\cdot 15}{6}$ from the *obstruction* 4·02; and thus we have 2·83, the true *resistance to conduction* of the decomposing cell.

57. The latter part of this process is difficult to express clearly, I have therefore drawn a figure to illustrate it. Suppose that in fig. 5, 6 represents the intensity of the battery; the line R 3·15, the resistance of the battery and the connecting wires; and the remainder of the line A B, or 4·02 W,

---

* From (51. 53.) we have the equations $\frac{1\cdot 88}{R+7\cdot 7} = \frac{1\cdot 29}{R+4\cdot 4}$ and $\frac{1\cdot 73}{R'+7\cdot 7} = \frac{1\cdot 22}{R'+4\cdot 4}$, whence R = 2·81 and R′ = 3·49 : the mean resistance of the battery and connecting wires was therefore 3·15.

† Faraday's Experimental Researches, (1007).

the resistance of wire. I have shown, (55.) that the current 1°·9 Q would pass against the resistance A B. But we know that 1°·9 Q was also passed when the cell and the battery formed the sole opposition (52.), and that on account of the resistance to electrolyzation, the virtual battery intensity was then one-sixth less, and hence that only five-sixths of the resistance represented by A B could have been opposed in this case, in order to the passage of the same current. Draw, therefore, another line, C D, one-sixth less than A B, and it will represent this resistance; from which, on subtracting R 3·15, we have *r* 2·83, the true resistance to conduction of the decomposing apparatus.

Fig. 6.

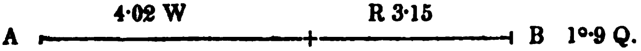
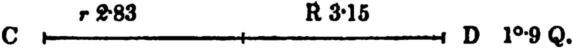


Fig. 5.



58. From (28.), and the data above given, we have

$$\frac{(1\cdot9)^2}{(1\cdot88)^2} \times 2\cdot83 \times 7^\circ\cdot56 \times \frac{20'}{60'} = 7^\circ\cdot29,$$

the *theoretical* result.

59. I made three other experiments with the same electrodes, and with the same battery. The results of these with those of the experiment just given at length, are as follows:—

	Experimental.		Theoretical.
	°		°
Exp. 7.	7·26	....	7·29
Exp. 8.	8·12	....	8·32
Exp. 9.	10·2	....	10·2
Exp. 10.	9·64	....	9·75 (Refitted battery.)
Mean	8·8	....	8·89

60. Exp. 11.—The mean current 0°·846 Q from a battery of ten zinc-iron pairs, was, by means of the same electrodes, sent through two pounds of dilute sulphuric acid for half an hour, during which the correct heat that was generated was 3°·09.

61. In order to find the true resistance to conduction of the decomposing cell, it was necessary to remember that in this instance one-third of the intensity of the ten pairs was expended in overcoming the resistance to electrolyzation of the water. With this exception the calculations were made precisely as before, and gave 3·76, the resistance of the cell; whence we have the theoretical heat 2°·88.

62. I now dismissed the narrow electrodes, and substituted for them two pieces of platinum foil, dipping to the bottom of the liquid; they were one inch apart, and each presented to the dilute sulphuric acid a surface of seven square inches. In this case I used twenty pairs of zinc-iron plates arranged in a series of ten.

63. The mean of six experiments with this apparatus gave 4°·42 of real, and 4°·13 of theoretical heat. I have no doubt that the difference is principally occasioned by the formation of the deutoxide of hydrogen, which is known to occur to a considerable extent when oxygen is evolved from an extended surface. Of this we have another instance in the following experiment.

64. Exp. 12.—Using the same electrodes, and a battery of ten zinc-iron pairs, I now passed a current of the mean intensity 1°·08 Q through two pounds of dilute nitric acid, sp. gr. 1047, for half an hour. The heat that was thus generated, when properly corrected, was 3°.

65. This experiment was, as the others, conducted in the manner described at length under Exp. 7. Water chiefly* was decomposed; and I ascertained, experimentally, that about $\frac{1}{3{\cdot}5}$ of the intensity of the battery was expended in overcoming resistance to electrolysis. Thus I had $3{\cdot}52 - \frac{3{\cdot}52 + 1{\cdot}68}{3{\cdot}5} = 2{\cdot}03$, the resistance to conduction; and hence $\frac{(1{\cdot}08)^2}{(1{\cdot}88)^2} \times 2{\cdot}03 \times 7^\circ{\cdot}56 \times \frac{30'}{60'} = 2^\circ{\cdot}53$, the theoretical heat.

66. Exp. 13.—Two plates of copper, each of which was two inches broad, were secured at the distance of one inch apart, and immersed in two pounds of a saturated solution of sulphate of copper. Through this apparatus, a battery of ten zinc-iron pairs passed the mean current 1° Q during half an hour. The heat thus produced, when properly corrected, was 5°·8.

67. In this case there was no *resistance to electrolysis*, and the action may be regarded simply as a transfer of copper from the positive to the negative electrode. All the *obstruction*, therefore, that was presented to the current, was *resistance to conduction*. Its mean was 5·5, whence we have $\frac{1}{(1{\cdot}88)^2} \times 5{\cdot}5 \times 7^\circ{\cdot}56 \times \frac{30'}{60'} = 5^\circ{\cdot}88$, the theoretical heat.

---

* See Faraday on the Electrolysis of Nitric Acid, 'Experimental Researches,' (752.).

68. We have thus arrived at the general conclusion, that *the heat which is evolved by the proper action of any voltaic current is proportional to the square of the intensity of that current, multiplied by the resistance to conduction which it experiences.* From this law the following conclusions are directly deduced:—

69. 1st. That *if the electrodes of a galvanic pair of given intensity be connected by any simply conducting body, the total voltaic heat generated by the entire circuit (provided always that no local action occurs in the pair) will, whatever may be the resistance to conduction, be proportional to the number of atoms (whether of water or of zinc) concerned in generating the current.* For if the resistance to conduction be diminished, the quantity of current will be increased in the same ratio, and hence, according to the law (68.), the quantity of heat which would thus be generated in a given time will be also proportionally increased; whilst of course the number of atoms which would be electrolyzed in the pair will be increased in the same proportion.

70. 2nd. That *the total voltaic heat which is produced by any pair, is directly proportional to its intensity, and the number of atoms which are electrolyzed in it.* For the quantity of current is proportional to the intensity of the pair, and consequently the quantity of heat evolved in a given time is proportional to the square of the intensity of the pair, but the number of atoms electrolyzed is proportional, in the same time, to the simple ratio only of the current, or of the intensity of the pair.

71. And 3rd. That *when any voltaic arrangement, whether simple or compound, passes a current of electricity through any substance, whether an electrolyte or not, the total voltaic heat which is generated in any time, is proportional to the number of atoms which are electrolyzed in each cell of the circuit, multiplied by the virtual* intensity of the battery.*

72. Berzelius thinks that the light and heat produced by combustion are occasioned by the discharge of electricity between the combustible and the oxygen with which it is in the act of combination; and I am of opinion that the heat arising from this, and some other chemical processes, is the consequence of resistance to electric conduction. My experiments on the heat produced by the combustion of zinc turnings in oxygen, (which, when sufficiently complete, I shall make public) strongly confirm this view; and the quantity of heat which Crawford produced by exploding a mixture of hydrogen and

---

* If a decomposing cell be in the circuit, the *virtual* intensity of the battery is reduced in proportion to its resistance to electrolyzation.

oxygen may be considered almost decisive of the question. In his unexceptionable experiments, one grain of hydrogen produced heat sufficient to raise one pound of water 9°·6. Now we know from Exp. 5, that the heat generated in one of Mr. Grove's pairs by the electrolysis of $\frac{4{\cdot}77 \times 32{\cdot}3}{6} = 25{\cdot}7$ grains of zinc, is theoretically 3°·46; and the heat which must in the same time have been generated by the *metallic* part of the circuit, which presented the resistance 0·06, is $\frac{0{\cdot}06}{0{\cdot}427} \times 3^{\circ}{\cdot}46 = 0^{\circ}{\cdot}48$: the total voltaic heat was therefore 3°·94. Hence the total heat which would have been evolved by the electrolysis of an equivalent, or 32·3 grains of zinc, is $\frac{32{\cdot}3}{25{\cdot}7} \times 3^{\circ}{\cdot}94 = 4^{\circ}{\cdot}95$; which, when reduced to the capacity of one pound of water, is 9°·9. But from the table of the intensities of voltaic arrangements (74.), the intensity of Mr. Grove's pair, compared with the affinity of hydrogen for oxygen, is $\frac{1}{0{\cdot}93}$; whence, from (70.), we have $9^{\circ}{\cdot}9 \times 0{\cdot}93 = 9^{\circ}{\cdot}2$, the heat which should be generated by the combustion of one grain of hydrogen, according to the doctrine of resistances: the result of Crawford is only 0°·4 more.

73. I am aware that there are some anomalous conditions of the current which seem to militate against the general law (68.), particularly when in the hands of Peltier it actually produces *cold**. I have little doubt, however, that the explanations of these will be ultimately found in actions of a secondary character.

Note on Voltaic Batteries.

74. In the foregoing investigation I have had occasion to work very extensively with different voltaic arrangements, and have repeatedly ascertained their relative intensities by the mathematical theory of Ohm. It will not, therefore, I hope, be deemed out of place to subjoin a table, in which the intensities of the batteries which are most generally used, are inversely as the number of pairs which would be just requisite in order to overcome the resistance of water to electrolyzation.

---

* If antimony and bismuth be soldered together, cold will be produced at the point of junction by the passage of the current from the bismuth to the antimony. Peltier, *Annales de Chimie*, vol. lvi. p. 371. In his paper, however, a misprint has inverted the direction of the current.

Mr. Grove's.	Platinum Nitric acid	Amalgam. zinc Dilute sulp. acid	$\frac{1}{0\cdot93}$	Constant Intensities.
Prof. Daniell's.	Copper Sulphate of oxide of copper	Amalgam. zinc Dilute sulphuric acid	$\frac{1}{1\cdot54}$	
Mr. Sturgeon's.	Iron. Amalgamated zinc Dilute sulphuric acid		$\frac{1}{3\cdot33}$	
Mr. Smee's.	Platinized silver. Amalg. zinc Dilute sulphuric acid		$\frac{1}{3\cdot58}$	
	Copper. Amalgamated zinc Dilute sulphuric acid		$\frac{1}{5\cdot40}$	

75. Without entering particularly into the respective merits of these arrangements, I may observe that each of the first four may be used advantageously, according to the circumstances in which the experimenter is placed, or the particular experiments which he wishes to execute. The zinc-iron battery is somewhat inconvenient on account of local action on the iron; but then it presents great mechanical facilities in its construction. Mr. Smee's and Mr. Grove's are also very good arrangements; but the battery of Daniell is the best instrument for general use, and is, moreover, unquestionably the most œconomical.

Broom Hill, Pendlebury near Manchester,
March 25, 1841.

P.S. In the above table of galvanic intensities, that of zinc-iron immersed in dilute sulphuric acid is somewhat overstated. Recent experiments convince me that when the iron is in its best condition it possesses the same powers as the platinized silver. I attributed the iron battery to Mr. Sturgeon, who constructed one of these excellent instruments early in 1839*. It consisted of twelve cast-iron tubes, furnished with strips of amalgamated zinc. But I find that the experiments of this gentleman were not published as early as those of Mr. Roberts. Prof. Daniell (Phil. Trans. 1836, p. 114) observed that iron is sometimes more efficient than platinum in voltaic association with amalgamated zinc.

August 11, 1841. J. P. J.

[* A paper by Dr. A. Fyfe, on the employment of iron in the construction of voltaic batteries, appeared in Phil. Mag. for August 1837: S. 3, vol. xi. p. 145.—EDIT.]

XXXIX. *Notices of the Results of the Labours of Continental Chemists.* *By Messrs.* W. Francis *and* H. Croft.

[Continued from p. 195.]

New Method of determining Nitrogen.

IN a letter addressed by Berzelius to Prof. Erdmann, and published in the *Journ. f. Prakt. Chem.* for June 15th, 1841, we find the following important notice:—"On preparing the interesting memoir of Dumas on the decomposition of organic substances, by the action of the hydrate of potash, for my Annual Report, it occurred to me that nitrogenous bodies should, when so treated, give off the whole amount of their nitrogen as ammonia, which may be received in muriatic acid just in the same manner as carbonic acid in a solution of potash, and weighed as ammonio-chloride of platinum. I am following up this idea with M. Plantamour. To guard against the formation of cyanogen, we have commenced with compounds of cyanogen, which give ammonia just as well as sal-ammoniac and lime. If, indeed, all should succeed according to the present appearances, what a valuable and easy control in determinations of nitrogen we shall thus obtain! We make the experiments in the same way as the usual organic analyses, and pass the vapours over a strongly-heated mixture of the hydrate of potash and hydrate of lime, to decompose such bodies as, for instance, anil $(C^{12} H^8 + \underline{N} \, \underline{H}^3)$."

Nitrurets.

On exposing the white precipitate to a gradually increasing heat, a vast quantity of ammonia first passes over without any trace of nitrogen gas, after which perchloride of mercury-ammonia, and ammonia are evolved; and a red body remains in the retort, which decomposes at a temperature above 360° into protochloride of mercury, mercury and nitrogen. This red body is obtained purest when the heating is continued in a metallic bath until some protochloride of mercury has formed. It consists of crystalline scales, and has quite the appearance of the crystalline peroxide of mercury. It is insoluble in water, not affected by aqueous alkalies, even at the boiling point of the liquid. The compound may be boiled with dilute and concentrated nitric acid, and with rather concentrated sulphuric acid, without being decomposed or dissolved, which takes place, however, when it is boiled with concentrated sulphuric acid, or with hydrochloric acid: no gas is evolved. Heated beyond the boiling point of mercury, nitrogen escapes; mercury and protochloride of mercury are sublimed. Several experiments were made to determine these three bodies; according to which the compound consists of perchloride of mercury with nitruret of mercury, $2\ Hg\ \underline{Cl} + Hg^3\ \underline{N}$. Prof. Mitscherlich, by whom the above researches have been made, did not succeed in isolating the nitruret of mercury. This has been effected by Plantamour. It explodes violently, equalling, if not surpassing, in this respect the iodide of nitrogen, but is by far less sensitive. It forms a dark brown powder; decomposed with oxide of copper after the manner of an organic analysis, it gives no water, but only nitrogen and mercury; the true discoverer of nitrurets is, however, Schrötter, to whose experiments we shall presently return. Mitscherlich concludes, from the composition of the chloronitruret of mercury (*Quecksilberstickstoffchlorid*), that the white precipitate does not consist of one atom of the perchloride of mercury and one atom of amidide of mercury, $Hg\ \underline{Cl} + Hg\ \underline{N}\ \underline{H}^2$, but of three atoms of both substances, $3\ Hg\ \underline{Cl} + 3\ Hg\ \underline{N}\ \underline{H}^2$; as, on heating, two atoms of ammonia and one of perchloride of mercury are separated*. The perbromide of mercury acts in the same manner as the perchloride towards ammonia, giving off, on being heated, bromide of mercury-ammonia and ammonia, and bromo-nitruret of mercury remains behind. The mercury behaves in these compounds in the same way as potassium to ammonia; the olive-coloured body which is obtained by the action of potassium on dry gaseous ammonia is amidide of potassium, ($3\ K . N\ H^2$); and the graphite-like substance, which is formed by exposing the amidide of potassium to heat, whereby ammonia is set free, contains nitruret of potassium, $K^3\ \underline{N}$.

Schrötter, to whose experiments we have above alluded, has succeeded in preparing several nitrurets, of which he has given a description, and their mode of preparation, analysis and properties, from which we extract the following:—Metallic copper is not affected by being treated with dried ammoniacal gas; but, when well-dried gaseous ammonia is passed over oxide of copper, situated in a glass tube three feet long, and the anterior portion of the tube where the gas enters gently heated, formation of water and evolution of nitrogen take place, without any reduction of the oxide being perceptible. When the under portion of the tube is more strongly heated, reduction ensues with violent disengagements of nitrogen and formation of water. In the portion of the tube gently heated, the oxide of copper is found to be partially converted into a green powder; while, in that portion subjected to higher temperature, only reduced copper occurs. When the green powder is heated over a spirit-lamp, a beautiful combustion appears before red heat; at the same time it is converted with violent disengagement of nitrogen into metallic copper: this indicates that the green powder is a combination of nitrogen with copper, which is confirmed by numerous experiments. The formation of the nitruret of copper takes place long before the boiling point of linseed oil, and, in fact, at 250° C., while the reduction of the oxide of copper, by means of hydrogen, only takes place at the boiling point. The degree of division of the oxide is of much importance, and it is best to employ an oxide prepared by precipitating a hot solution of the nitrate of copper with caustic potash, and boiling. Even with this precaution it is difficult to prepare a nitruret of copper perfectly free from oxide; for, after the action of ammonia has continued for about eight hours, all formation of water ceases; the powder must then be well shaken, which must be often repeated, and towards the end of the operation triturated several times in a mortar. To produce twenty grammes of nitruret of copper, it is requisite to continue the action of ammonia during 120 hours. The hydrate or carbonate of copper cannot be employed with the same advantage as the oxide, as they become heated in contact with ammonia, and the temperature rises so high that the nascent nitruret is immediately decomposed. The nitruret of copper is a somewhat blackish, fine powder (the green powder contains still undecomposed oxide of copper), which, at a temperature of about 300°, becomes decomposed, whilst evolving a beautiful red light, into copper and nitrogen, which is mixed with nitric oxide, if the combination still contain some oxide. In carbonic acid, and in ammoniacal gas, it is decomposed at a higher, in oxygen, at a lower temperature, than in atmospheric air. It is not decomposed by pressure or friction. Acids decompose it; brought into contact with sulphuric acid, a violent disengagement of nitrogen ensues, and the copper remains behind in a metallic state. Acids which dissolve the copper, as nitric acid, act far more violently. It is impossible to free it entirely from adherent ammonia. The analysis was made according to the method generally adopted for organic nitrogenous substances. The amount of copper was determined by dissolving a portion of the combination in nitric acid, and converting the nitrate into oxide of copper. The results obtained agree exceedingly well with the formula $N\,Cu^3$*.

---

* See Dr. Kane's paper, Phil. Mag., August 1840, S. 3, vol. xvii. p. 125.

[* See Prof. Grove's paper on some electro nitrogurets, in the present volume, pp. 100, 103: see also, Phil. Mag. Second Series, vol. iv. p. 155; vol. vi. p. 147.—Edit.]

Liebig observed, that on heating the bichromate of the perchloride of chromium with ammonia, or the chloride of chromium in gaseous ammonia, metallic chromium is obtained; the metal obtained, according to these two methods, exhibits, however, very different properties; the first being a black powder, which, under the steel, acquires a metallic lustre, while the latter has a dull chocolate-brown colour. This difference in the properties induced M. Schrötter to examine them more accurately: he found, that on heating the brown powder in a combustion-tube, from which atmospheric air had been expelled by means of carbonic acid, a beautiful incandescence ensued, accompanied by a violent disengagement of gas, which proved to be nitrogen mixed with nitric oxide. Another experiment showed that the substance contained no hydrogen; it is, therefore, nitruret of chromium. Its composition is represented by the formula $N^5 Cr^2$. It is impossible to free it entirely from ammonia. The brown powder is a mixture of the nitruret with the oxide of chromium, to obtain it free from which it must be prepared in the following manner:—The chloride of chromium is heated in an atmosphere of hydrochloric gas, until it is anhydrous; it has then acquired a beautiful peach-red colour, and is deposited in crystalline laminæ in the upper part of the tube. On treating it with ammonia all contact with oxygen must be carefully avoided. Thus prepared, the nitruret of chromium is black, and has all the properties of the body prepared after the other method. (Mitscherlich in *Poggendorff's Annalen*, vol. xlix. p. 407; Berzelius in *Journ. für Prakt. Chem.* vol. xxiii. p. 230; and Schrötter in *Annal. der Chem. und Pharm.* vol. xxxvii. p. 129.)

Action of Ammonia on glowing Charcoal; formation of Prussic Acid, &c.

In a notice published in Liebig's *Ann. der Chem. und Pharm.* vol. xxxviii. p. 69, M. Kuhlmann advances the following positions, to which he has been led by experiments on the properties of spongy platinum:—1. All volatile nitrogenous compounds can, when mixed with air, oxygen, or an oxygenous gas, be converted by this means into nitric acid or hyponitric acid: 2, the same compounds produce with hydrogen, or an hydrogenous gas, ammonia: 3, mixed with hydrocarbons, or when the nitrogen compound contains hydrogen with carbonic oxide, they produce prussic acid or the prussiate of ammonia. In repeating the experiment described by Clouet, viz. the production of prussic acid by the action of ammonia on incandescent charcoal, M. Kuhlmann found it to succeed perfectly, the prussiate of ammonia is obtained however, the free acid being decomposed at the temperature at which the action takes place; *at the same time* quadricarburet of hydrogen is *evolved*. The decomposition may be expressed by the formula $3C + 2N^2H^6 = N^2C^2H^2, N^2H^6 + CH^4$. The energetic action of charcoal on ammonia may be turned to account in preparing anhydrous prussic acid. For this purpose dried ammonia is passed through a porcelain tube containing small pieces of red-hot charcoal; the escaping gases, containing much prussiate of ammonia, are conveyed into dilute sulphuric acid, warmed to about 50°; the ammonia is detained by the sulphuric acid, and only prussic acid escapes, which is condensed in a vessel surrounded by a frigorific mixture. The acid thus obtained is quite as pure as that from the decomposition of cyanide of mercury by means of hydrochloric acid. This action of carbon with ammonia may also be employed in the preparation of the ferrocyanide of potassium; the vapour of the prussiate of ammonia is passed into a solution of caustic potash, in which the hydrate of the protoxide of iron is suspended.

Similar experiments on the action of ammonia on red-hot charcoal have been published by M. Langlois in the *Annales de Chim. et de Phys. 3ième Sér.* t. i. p. 117, which agree with the above, with the exception that the gas which escapes at the formation of the prussiate of ammonia, is stated to contain neither nitrogen nor *carburetted hydrogen*, but to be pure hydrogen. To explain the decomposition M. Langlois adopts the notion, that one equiv. of ammonia, N^2H^6, becomes, on contact with incandescent charcoal, $N^2H^2 + H^4$; the two equiv. of hydrogen are then replaced by two equiv. carbon to form prussic acid, which immediately enters into combination with one equiv. of undecomposed ammonia; a portion of the hydrogen of the ammonia being replaced by carbon exactly as the oxygen of the metallic oxides by chlorine, when exposed with the latter to a high temperature. 0·114 of the salt gave 0·340 cyanide of silver, corresponding to 0·068 prussic acid, which gives the formula $N^2H^6 + C^2N^2H^2$.

Double Cyanurets of Zinc with Alkalies and Earths.

Samselius has found that these combinations may likewise be obtained, (besides in the usual moist way, by dissolving cyanide of zinc in the alkaline cyanides,) by mixing the alkaline cyanides with carbonate, or any other salt of the oxide of zinc, as long as the cyanide of zinc is redissolved, upon which the double salt is obtained on evaporation, at least, as far as may be concluded from the action with cyanide of potassium. Caustic ammonia dissolves cyanide of zinc, and soon deposits the double salt. The double salt of barytes cannot be obtained by precipitating a solution of cyanide of zinc-potassium with acetate of barytes. It gives a white precipitate, insoluble, or nearly so, which contains barytes, with evolution of a strong smell of prussic acid. A corresponding combination with oxide of lead was obtained on precipitating cyanide of zinc-potassium with acetate of lead. It forms a white powder. Acetic acid removes the lead, leaving cyanide of zinc behind. The compound was not analysed, but regarded as $Zn\,\underline{Cy} + \dot{P}b$. This kind of combination of a cyanide with an oxide of another metal will be new and remarkable, should it be confirmed. Cyanide of zinc-magnesium could not be prepared; the cyanogen is decomposed during evaporation, and a brown substance is deposited.—(*Berz. Jahresber.* vol. xx. part ii. p. 153.)

On the Stibio- and Arsenio-Sulphurets.

Persulphuret of antimony, $Sb^2 S^5$, combines with basic sulphurets to form very peculiar crystallizable salts, of which, however, only the sodium and potassium compounds have been examined. Dr. Rammelsberg has published a memoir on the subject. Some chemists consider $Sb^2 S^5$ as a mixture of $Sb^2 S^3 + S^2$, because it is decomposed into these elements by boiling with oil of turpentine; its power, however, of forming characteristic salts, speaks for its being a determinate compound. [A fact in favour of its being a determinate sulphuret is, that when boiled with caustic soda, stibio-persulphuret of sodium and antimoniate of soda are formed, but no hyposulphite; on the other hand, the two atoms of sulphur may be driven out at a temperature by which sulphur boils; they may also be extracted by sulphuret of carbon.] (Mitscherlich, *Poggendorff's Annalen*, xlix. p. 412.)

The stibio-persulphurets may be formed in several ways. Those with the bases of the alkaline and earthy metals are soluble in water, and are either colourless or yellow; the insoluble salts are of various colours. The soluble sulphurets are decomposed by carbonic acid; the insoluble ones are often very difficult of decomposition.

The stibio-persulphuret of potassium is deliquescent; its formula is $3\,K\,S + Sb^2 S^5 + 9\,H^2 O$, or $\overset{\prime}{K}{}^3\,\overset{\prime\prime\prime\prime\prime}{\underline{Sb}} + 9\,\underline{\dot{H}}$. When sulphuretted hydrogen is passed through a solution of the neutral antimoniate of potash, persulphuret of antimony is thrown down, and the above-mentioned stibio-sulphuret remains in solution. Persulphuret of antimony loses its colour when treated with a solution of caustic potash; it dissolves, and at the same time a heavy white powder is formed; this is bin-antimoniate of potassa, $K\,\underline{\overset{\cdot\cdot\cdot\cdot\cdot}{Sb}} + 6\,\underline{\dot{H}}$. If the solution be evaporated to a certain point, and then allowed to cool, a colourless salt separates in long acicular crystals; they are not deliquescent, but soon become covered with a brown substance. They are soluble in boiling water, but decomposed by cold water, by which part is dissolved, and antimoniate of potash remains behind. This is a compound of the stibio-persulphuret with antimoniate of potash. Its formula is $(3\,K\,S + Sb^2\,S^5 + 9\,H^2\,O) + (K\,O + Sb^2\,O^5 + H^2\,O)$, or $(\overset{,}{K}^3\,\underline{\overset{,,,,,}{Sb}} + 9\,\underline{\dot{H}}) + (\dot{K}\,\underline{\overset{\cdot\cdot\cdot\cdot\cdot}{Sb}} + \underline{\dot{H}})$. With soda no such salt can be obtained.

Stibio-sulphuret of Sodium.—Schlippe's salt crystallizes in tetrahedrons; subordinate are the faces of the second tetrahedron, dodecahedron, and tetratrishexahedron $(a : \frac{1}{2}a \infty a)$. The solution of this salt, by exposure to the air, deposits a reddish-brown powder: in the solution are contained carbonate and hyposulphite of soda. As to the formula of this salt there is some degree of uncertainty. Different formulæ have been given. Duflos's is $\overset{,}{Na}^2\,\underline{\overset{,,,,,}{Sb}} + 8\,\underline{\dot{H}}$; Liebig's, $\overset{,}{Na}\,\underline{\overset{,,,,,}{Sb}} + 12\,\underline{\dot{H}}$; Schlippe's, $\overset{,}{Na}^3\,\underline{\overset{,,,,,}{Sb}} + 20\,\underline{\dot{H}}$. Rammelsberg found $\overset{,}{Na}^3\,\underline{\overset{,,,,,}{Sb}} + 18\,\underline{\dot{H}}$.

The barium salt can be obtained crystallized; its formula is $\overset{,}{Ba}^3\,\underline{\overset{,,,,,}{Sb}} + 6\,\underline{\dot{H}}$.

The ammonium, strontium, calcium and magnesium salts do not crystallize; they all, however, contain three atoms of basic sulphuret.

To obtain the pure metallic stibio-sulphurets, the solutions of the metallic salts must be added gradually to an excess of the alkaline stibio-sulphuret.

The silver, lead and copper salts, obtained in this manner, have the formulæ $\overset{,}{Ag}^3\,\underline{\overset{,,,,,}{Sb}}$, $\overset{,}{Pb}^3\,\underline{\overset{,,,,,}{Sb}}$, $\overset{,}{Cu}^3\,\underline{\overset{,,,,,}{Sb}}$. When the silver salt is heated sulphur is given off, and a substance remains having the formula $\overset{,}{Ag}^3\,\underline{\overset{,,,}{Sb}}$, the same as red silver; by heating the lead compound, $\overset{,}{Pb}^3\,\underline{\overset{,,,}{Sb}}$ is obtained; this is the composition of Boulangerite. With corrosive sublimate the salt $\overset{,}{Hg}^3\,\underline{\overset{,,,,,}{Sb}}$ may also be obtained. With sulphate of zinc an orange-coloured body is formed; its composition is $\dot{\mathrm{Zn}}^3\,\underline{\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{Sb}}}$ $+\ \dot{\mathrm{Zn}}$.

When the alkaline stibio-sulphuret is added to an excess of the metallic salt, and the whole boiled for some time, a totally different kind of salt is produced. By caustic potash these salts are decomposed into metallic sulphurets and antimonic acid. The silver salt contains $\mathrm{Ag}^8\,\mathrm{S}^8\,\mathrm{Sb}^2\,\mathrm{O}^5$; it may be either $\dot{\mathrm{Ag}}^3\,\underline{\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{Sb}}} + 5\,\dot{\mathrm{Ag}}$, or $8\,\dot{\mathrm{Ag}} + \underline{\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{Sb}}}$. The lead and copper salts have the same constitution. With chloride of mercury a white precipitate is obtained; its composition is $\dot{\mathrm{Hg}}^3\,\underline{\overset{\cdot\cdot\cdot\cdot\cdot}{\mathrm{Sb}}} + 3\,\mathrm{Hg\,Cl}^2 + 3\,\dot{\mathrm{Hg}}$.

When arsenio-sulphuret of sodium is added to an excess of sulphate of copper, and the whole boiled, the black insoluble substance which is formed is sulphuret of copper, Cu S, and in the solution is arsenic acid, containing the whole of the employed arsenic. It is therefore probable that these above-mentioned salts are compounds of the metallic sulphurets with antimonic acid. (*Poggendorff's Annalen*, lii. p. 193–242.)

XL. *Meteorological Summary of the Weather at Montreal, Province of Canada, in Lat.* 45° 30′ *N., Long.* 73° 22′ *W., from Registers kept* (*by* J. S. McCord, *Associate Member of the London Meteorological Society, Member of the Natural History Society of Montreal, Corresponding Member of the Literary and Historical Society, Quebec, and Albany Institute, New York,*) *for five years, from* 1836 *to* 1840 *inclusive*[*].

TEMPERATURE.

ANNUAL TEMPERATURE.

	Mean Temp.	Max. Temp.	Min. Temp.	Range.
1836.	40·43	90·00	−19·00	109·00
1837.	41·22	90·00	−18·00	108·00
1838.	41·58	90·00	−13·00	103·00
1839.	44·07	89·00	−18·00	109·00
1840.	44·29	91·00	−14·50	105·50
Mean.	42·31	90·00	−16·50	106·90

---

[*] Reprinted from a table privately distributed by the author, communicated by Dr. Daubeny.

TEMPERATURE.

MEAN MONTHLY TEMPERATURE.

	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.
1836.	17·05	11·32	20·90	35·24	53·32	65·25	71·90	63·05	57·46	39·37	32·24	18·09
1837.	8·70	15·78	25·15	40·40	52·15	65·55	65·60	65·15	58·30	43·20	33·70	20·95
1838.	21·00	9·00	31·60	34·50	52·80	69·90	68·90	65·50	57·90	46·60	28·80	12·40
1839.	12·70	20·80	29·30	45·50	52·90	62·20	70·00	66·50	59·60	50·00	34·40	25·00
1840.	10·45	23·50	30·55	44·46	55·73	66·99	71·96	70·87	59·26	46·35	34·36	17·02
Mean.	13·98	16·08	27·50	40·02	53·38	65·97	69·67	66·21	58·50	45·10	32·70	18·69

WEATHER.

	Clear.	Cloudy.	Rain.	Snow.	Showers.	Fog.	Days observed.
1836.	151·50	134·79	34·00	25·75	10·00	1·00	357·04
1837.	135·25	132·75	34·25	13·00	9·50	2·25	327·00
1838.	146·75	112·50	32·50	16·50	13·25	0·50	322·00
1839.	167·00	118·50	43·00	13·75	10·00	6·75	359·00
1840.	141·25	138·50	38·50	21·00	11·25	5·50	356·00
Mean.	148·35	127·40	36·45	18·00	10·80	3·20	

Averaging per cent.—Clear days about 44; Cloudy, 35; Rain, 11; Snow, 5; Showery, 3; Fog, Hail, &c. 2.

PRESSURE. Corrected and reduced to 32° F.			RAIN.	WINDS.				
	Mean Ann. Pressure.	Annual Range.	Inches.	WESTERLY. N. W. W. S. W.	EASTERLY. N. E. E. S. E.	North.	South.	No. of observ
1836.	29·920	1·550	19.20	189·20	46·25	65·85	55·66	357·00
1837.	29·823	1·758	16·90	200·00	32·00	56·50	38·50	327·00
1838.	29·884	1·498	18·60	163·00	32·25	66·75	49·25	311·25
1839.	29·909	2·128	15.90	179·00	83·00	63·50	33·50	359·00
1840.	29·895	1·569	27·55.	188·50	71·50	47·50	45·50	353·00
Mean.	29·886	1·700	19·63	183·94	53·00	60·02	44·48	
	Or about . . .			54 per cent.	15 per cent.	18 p.c.	13 p. c.	

SNOW-GAUGE.

Winter of 1830—31	73·90 inches.
31—32	107·60 ...
32—33	60·60 ...
33—34	51·00 ...
34—35	84·95 ...
35—36	86·45 ...
36—37	65·50 ...
37—38	49·85 ...
38—39	47·70 ...
39—40	49·40 ...
Mean.	67·695

These observations are all made with instruments by first (British) artists, duly compared with standards, and every precaution taken in their position to ensure the most correct indications. The city of Montreal is about 46 feet above the tide waters of the St. Lawrence; the Barometer about 45 feet above the mean level of the St. Lawrence. Total elevation of Barometer above the tide waters of the St. Lawrence, 91 feet.

XLI. *Remarks on a Statement in the* Traité de Mécanique *of* Poisson. *By* JAMES BOOTH, *M.A., Principal of, and Professor of Mathematics in Bristol College**.

IN a work of great and deserved celebrity, to be found in the hands of every student of mechanical science—the *Traité de Mécanique* of Poisson—the author, in determining the three principal axes of rotation of a body, states correctly, that a principal axe passing through the centre of gravity is also a principal axe for any point assumed along this line, but seems to have committed an oversight in saying that the two remaining principal axes of the assumed point will vary in direction as the point shifts along the line. His words are, after giving the formula for determining the angle θ between the principal axe $O x'$ of the point O, and the axis of coordinates $O x$, "Les intégrales que cette équation renferme pourront changer de valeur avec la position du point O; en sorte que le long de l'axe $O z$, les deux autres axes principaux ne seront pas, en général, parallèles à eux-mêmes."—*Traité de Mécanique*, tom. ii. page 91.

The equation by which θ is determined, as given by Poisson, is

$$(\cos^2\theta - \sin^2\theta)\int x\,y\,d\,m + \sin\theta\cos\theta\,(\int x^2\,d\,m - y^2\,d\,m) = 0,$$

which may be reduced to

$$\tan 2\,\theta = \frac{2\int x\,y\,d\,m}{\int(y^2 - x^2)\,d\,m}.$$

Now of the three coordinates $x\;y\;z$ of the particle $d\,m$, the only one which varies by shifting the point O along the axis of Z is z; but the above expression for the value of $\tan 2\,\theta$ is independent of z, hence the angle θ is constant, or the principal axes are always parallel for any point along the line.

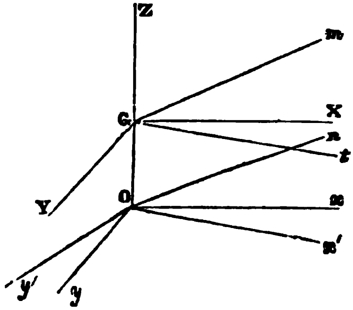
A direct demonstration from established principles of the parallelism of the principal axes in this case may be given as follows:—

Assuming then that a principal axe of rotation through the centre of gravity, is also a principal axe for any point taken on this line, let G X, G Y, G Z be the three principal axes of the body passing through the centre of gravity G, O any point assumed on the axis G Z; if the three principal axes through this point are not parallel to those, through the centre of gravity, let the principal axe $O\,x'$ make with the line $O\,x$, which is parallel to the line G X, the angle ω; let the moments of inertia round the principal axes G X, G Y, G Z be

---

* Communicated by the Author.

A, B, C, while the moments of inertia round the principal axes O x', O y', O Z are A$'$, B$'$, C; through the centre of gravity and the point O, let any two arbitrary parallel right lines be drawn, G m and O n; let the moments of inertia round these right lines be D and D$'$, and put the distance G O $= c$.



Let the angle Z G m or Z O n be θ, the angle between the planes Z G X and Z G $m = \phi$, then we shall have

$$D = A \sin^2 \theta \cos^2 \phi + B \sin^2 \theta \sin^2 \phi + C \cos^2 \theta \qquad \text{(I.)}$$

But as the line O n is parallel to G m and distant from it by the distance $c \sin \theta$, we shall have (*Traité de Mécanique*, tom. ii. page 53.), M being the mass of the body,

$$D' = D + M c^2 \sin^2 \theta,$$ or putting for D its value,

$$\left. \begin{array}{l} D' = A \sin^2 \theta \cos^2 \phi + B \sin^2 \theta \sin^2 \phi + C \cos^2 \theta \\ \quad + M c^2 \sin^2 \theta \;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;. \end{array} \right\} \text{(II.)}$$

The angle between the planes Z O n and Z O x' is $(\phi - \omega)$: referring then the moment of inertia D$'$ to the moments of inertia of the three principal axes passing through the point O, we find

$$\left. \begin{array}{l} D' = A' \sin^2 \theta \cos^2 (\phi - \omega) + B' \sin^2 \theta \sin^2 (\phi - \omega) \\ \quad + C \cos^2 \theta \;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;. \end{array} \right\} \text{(III.)}$$

Equating the values of D$'$ given by equations (II.) (III.), eliminating and dividing by $\sin^2 \theta$, there results

$$\left. \begin{array}{l} A \cos^2 \phi + B \sin^2 \phi + M c^2 = A' \cos^2 (\phi - \omega) \\ \quad + B' \sin^2 (\phi - \omega) \;.\;.\;.\;.\;.\;.\;.\;. \end{array} \right\} \text{(IV.)}$$

We have now to determine A$'$, B$'$.

Through G let a right line G t be drawn parallel to O x'; the moment of inertia round this line is $A \cos^2 \omega + B \sin^2 \omega$, and as this line passing through the centre of gravity is distant from O x' by c, the moment of inertia round O x' or A$'$ is equal to

$$A \cos^2 \omega + B \sin^2 \omega + M c^2. \text{ Similarly, .}$$

$$B' = A \sin^2 \omega + B \cos^2 \omega + M c^2.$$

Substituting these values of A$'$ and B$'$ in (IV.), we find

$$\left. \begin{array}{l} A \cos^2 \phi + B \sin^2 \phi = A \{ \cos^2 \omega \cos^2 (\phi - \omega) + \sin^2 \omega \\ \quad \sin^2 (\phi - \omega) \} + B \{ \sin^2 \omega \cos^2 (\phi - \omega) + \cos^2 \omega \\ \quad \sin^2 (\phi - \omega) \} \;.\;.\;.\;.\;.\;.\;.\;.\;.\;.\;. \end{array} \right\} \text{(V.)}$$

Now $\phi = \omega + \phi - \omega$, hence

$$\cos\phi = \cos\omega\cos(\phi-\omega) - \sin\omega\sin(\phi-\omega), \text{ or}$$

$$\cos^2\phi = \cos^2\omega\cos^2(\phi-\omega) + \sin^2\omega\sin^2(\phi-\omega) - 2\sin\omega\cos\omega\cos(\phi-\omega)\sin(\phi-\omega).$$ Similarly,

$$\sin^2\phi = \sin^2\omega\cos^2(\phi-\omega) + \cos^2\omega\sin^2(\phi-\omega) + 2\sin\omega\cos\omega\cos(\phi-\omega)\sin(\phi-\omega).$$

Putting these values of $\cos^2\phi$, $\sin^2\phi$ in the first member of (V.) and eliminating, we find

$$(A-B)\sin 2\omega\sin 2(\phi-\omega) = 0 \;\ldots\ldots\; \text{(VI.)}$$

Now as ϕ is quite arbitrary, we may always assume it differing in value from ω, hence we may consider $\sin 2(\phi-\omega)$ as always different from zero; dividing then by this factor, equation (VI.) is thus reduced to

$$(A-B)\sin 2\omega = 0 \;\ldots\ldots\ldots\; \text{(VII.)}$$

This equation is satisfied when $A = B$ by any values of ω, or, in other words, when the moments of inertia round two of the principal axes passing through the centre of gravity are equal; any pair of rectangular axes in a plane perpendicular to the third principal axe are also principal axes.

When A is not equal to B, equation (VII.) can only be satisfied by the values $\omega = 0$, or $\omega = \frac{\pi}{2}$, or the principal axe Ox' coincides with Ox, which is parallel to GX, hence we may deduce the following theorem:—

The principal axes of rotation of a body passing through any point assumed on one of the three principal axes through the centre of gravity, are always parallel to these axes.

XLII. *On the Focal Lengths and Aberrations of a thin Lens of Uniaxal Crystal, bounded by Surfaces which are of Revolution about its Axis.* By Sir WILLIAM ROWAN HAMILTON, *P.R.I.A., Member of several Scientific Societies at Home and Abroad, Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland*[*].

THE following short investigation may perhaps be not without interest to the students of mathematical optics, as serving to illustrate a general method, and to correct an important error into which an eminent writer has fallen.

1. Let a ray of ordinary light, *in vacuo*, and in the plane of xz, proceeding from or towards a given point on the axis of z, be incident nearly perpendicularly on a given surface of revolution, and there undergo extraordinary refraction at en-

[*] Communicated by the Author.

tering a given uniaxal crystal, of which the optical axis coincides with the axis of revolution and of z. It is required to determine the intersection of the refracted ray with the axis; the distance of the point of incidence from the vertex, or the semiaperture of the crystal, being given.

2. The law of this extraordinary refraction may (according to the general methods of my 'Theory of Systems of Rays,' published in the Transactions of the Royal Irish Academy,) be thus expressed:

$$(\sigma-\alpha')\,\delta\,\xi + (\upsilon-\gamma')\,\delta\,\zeta = 0. \quad \ldots\ldots \quad (1.)$$

In this formula, ξ, ζ are (in the plane of $x\,z$) the rectangular coordinates of incidence, and are connected by the equation of the meridional section of the given refracting surface of revolution, which equation we may suppose to be (at least nearly enough for our present purpose) developed under the form

$$\zeta = \frac{r}{2}\,\xi^2 + \frac{s}{4}\,\xi^4, \quad \ldots\ldots\ldots \quad (2.)$$

r being the curvature at the vertex, and s being another constant, which in the case of a spheric surface is half of the cube of that curvature; α', γ' are the cosines of the inclinations of the incident ray to the positive semiaxes of $x\,z$, so that they are connected by the relation

$$\alpha'^2 + \gamma'^2 = 1; \quad \ldots\ldots\ldots\ldots \quad (3.)$$

and σ, υ are the components of normal slowness of the extraordinary wave within the crystal, so that, if μ be the ordinary and ν the extraordinary index, these components are connected by the relation

$$\mu^2\,\sigma^2 + \nu^2\,\upsilon^2 = \mu^2\,\nu^2. \quad \ldots\ldots\ldots\ldots \quad (4.)$$

And these quantities, $\alpha'\ \gamma'\ \sigma\ \upsilon$, are such, that if $x'\ z'$ be the coordinates of any point on the incident ray, we have

$$(\xi - x')\,\delta\,\alpha' + (\zeta - z')\,\delta\,\gamma' = 0; \quad \ldots\ldots \quad (5.)$$

and if $x\,y$ be the coordinates of any point on the refracted ray, we have

$$(x - \xi)\,\delta\,\sigma + (z - \zeta)\,\delta\,\upsilon = 0. \quad \ldots\ldots\ldots \quad (6.)$$

3. Making then

$$\mathrm{T} = \xi\,(\sigma - \alpha') + \zeta(\upsilon - \gamma'), \quad \ldots\ldots\ldots \quad (7.)$$

we have first the equation

$$\frac{\delta\,\mathrm{T}}{\delta\,\xi} = 0, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (8.)$$

which contains the law of extraordinary refraction, and by which ξ can be eliminated from the expression of T, so as to leave that quantity T expressed as a function of α' and σ only; ζ being here treated as a known function of ξ, γ' of α', and υ of σ. And since, by (1.) (5.) (6.),

$$\delta \mathrm{T} = x\,\delta\sigma + z\,\delta\upsilon - x'\,\delta\alpha' - z'\,\delta\gamma', \quad \ldots\ldots \quad (9.)$$

we have, by (3.) and (4.), the following equations, for the incident and refracted rays respectively:

$$-x' + \frac{\alpha'}{\gamma'}\,z' = \frac{\delta \mathrm{T}}{\delta \alpha'}; \quad \ldots\ldots \quad (10.)$$

$$x - \frac{\mu^2 \sigma}{\nu^2 \upsilon}\,z = \frac{\delta \mathrm{T}}{\delta \sigma}. \quad \ldots\ldots \quad (11.)$$

4. The approximate equation (2.) of the section of the refracting surface, which gives ζ as an explicit function of ξ, is now to be combined with the following analogous expressions for the functions γ' and υ, deduced from the relations (3.) and (4.):

$$\gamma' = 1 - \frac{\alpha'^2}{2}; \quad \upsilon = \mu - \frac{\mu\sigma^2}{2\nu^2}; \quad \ldots\ldots \quad (12.)$$

and thus the expression (7.) for T becomes, if we neglect terms which are small of the sixth dimension with respect to α', σ, ξ:

$$\mathrm{T} \;\; = \mathrm{T}^{(2)} + \mathrm{T}^{(4)}; \quad \ldots\ldots\ldots\ldots \quad (13.)$$

$$\mathrm{T}^{(2)} = \xi(\sigma - \alpha') + \tfrac{1}{2}\, r\,\xi^2 (\mu - 1); \quad \ldots\ldots \quad (14.)$$

$$\mathrm{T}^{(4)} = \tfrac{1}{4}\, s\,\xi^4 (\mu - 1) - \tfrac{1}{4}\, r\,\xi^2 \left(\frac{\mu\sigma^2}{\nu^2} - \alpha'^2\right). \; . \; . \quad (15.)$$

And to eliminate ξ, it is sufficient to employ the equation (8.) under the approximate form

$$0 = \frac{\delta \mathrm{T}^{(2)}}{\delta \xi} = \sigma - \alpha' + r\,\xi\,(\mu - 1); \quad \ldots\ldots \quad (16.)$$

for although the complete expression for the abscissa ξ of incidence contains terms of the third and higher dimensions with respect to α' and σ, yet the introduction of these terms of ξ would only introduce terms of the sixth and higher dimensions, in the expression for the function T.

5. Retaining therefore ξ as an auxiliary symbol, of which the meaning is determined by the formula (16.), and making the abscissæ x' and x to vanish in the equations of the two rays, (10.) and (11.), in order to discover the relation between the ordinates z' and z of the intersections of those two rays with the axis, we find

$$+\alpha' z'\left(1+\frac{\alpha'^2}{2}\right)=-\xi+\frac{\delta T^{(4)}}{\delta\alpha'}; \quad \ldots\ldots \quad (17.)$$

$$-\frac{\mu\sigma z}{\nu^2}\left(1+\frac{\sigma^2}{2\nu^2}\right)=+\xi+\frac{\delta T^{(4)}}{\delta\sigma}; \quad \ldots \quad (18.)$$

and therefore, taking the reciprocals,

$$-\frac{1}{z'}=\frac{\alpha'}{\xi}\left(1+\frac{\alpha'^2}{2}\right)+\frac{\alpha'}{\xi^2}\frac{\delta T^{(4)}}{\delta\alpha'}; \quad \ldots\ldots \quad (19.)$$

$$\frac{\nu^2}{\mu z}=\frac{-\sigma}{\xi}\left(1+\frac{\sigma^2}{2\nu^2}\right)+\frac{\sigma}{\xi^2}\frac{\delta T^{(4)}}{\delta\sigma}. \quad \ldots \quad (20.)$$

Adding these last two equations, attending to the value (16.) of ξ, and observing that $T^{(4)}$, after substitution of that value, becomes a homogeneous function of the fourth dimension of α' and σ, so that

$$\alpha'\frac{\delta T^{(4)}}{\delta\alpha'}+\sigma\frac{\delta T^{(4)}}{\delta\sigma}=4\,T^{(4)}, \quad \ldots\ldots\ldots\ldots \quad (21.)$$

we find this relation:

$$\frac{\nu^2}{\mu z}-\frac{1}{z'}-(\mu-1)r=\frac{1}{2\xi}\left(\alpha'^3-\frac{\sigma^3}{\nu^2}\right)+\frac{4}{\xi^2}T^{(4)}. \quad (22.)$$

And changing, in the second member, α' and σ to their approximate values given by (19.) and (20.), namely,

$$\alpha'=-\frac{\xi}{z'}, \quad \sigma=-\frac{\nu^2\xi}{\mu z}, \quad \ldots\ldots\ldots \quad (23.)$$

we find

$$\left.\begin{aligned}&\frac{\nu^2}{\mu z}-\frac{1}{z'}-(\mu-1)r\\ &=\frac{\xi^2}{2}\left\{\frac{\nu^4}{\mu^3 z^3}-\frac{1}{z'^3}+2(\mu-1)s-2r\left(\frac{\nu^2}{\mu z^2}-\frac{1}{z'^2}\right)\right\}.\end{aligned}\right\}(24.)$$

By suppressing the second member, we get the intersection of the axis with a refracted ray infinitely near it, or the extraordinary focus of the central rays; by taking account of that member we get the longitudinal aberration.

6. As a verification, we may consider the case of an ordinary refraction at a spheric surface as being included in the foregoing, and the formula for that case must result from the equation (24.), by making therein

$$\nu=\mu, \quad s=\frac{1}{2}r^3. \quad \ldots\ldots\ldots\ldots\ldots \quad (25.)$$

Accordingly these values give

$$\frac{\mu}{z} - \frac{1}{z'} - (\mu - 1)\, r$$
$$= \frac{\xi^2}{2}\left\{\mu\left(\frac{1}{z^3} - \frac{2r}{z^2} + r^3\right) - \left(\frac{1}{z'^3} - \frac{2r}{z'^2} + r^3\right)\right\};$$

in the second member of which we have

$$\mu\left(\frac{1}{z^3} - \frac{2r}{z^2} + r^3\right) = \mu\left(\frac{1}{z} - r\right)\left(\frac{1}{z^2} - \frac{r}{z} - r^2\right),$$
$$\frac{1}{z'^3} - \frac{2r}{z'^2} + r^3 = \left(\frac{1}{z'} - r\right)\left(\frac{1}{z'^2} - \frac{r}{z'} - r^2\right),$$

and may write

$$\mu\left(\frac{1}{z} - r\right) = \frac{1}{z'} - r = \frac{\mu}{\mu - 1}\left(\frac{1}{z'} - \frac{1}{z}\right);$$

the formula for an ordinary spheric refraction is therefore thus found to be

$$\left.\begin{aligned}&\frac{\mu}{z} - \frac{1}{z'} - (\mu - 1)\, r\\ &\quad = \frac{\mu}{\mu - 1}\left(r - \frac{1}{z} - \frac{1}{z'}\right)\left(\frac{1}{z} - \frac{1}{z'}\right)^2 \frac{\xi^2}{2},\end{aligned}\right\} \ldots (26.)$$

in which it may be remarked that $\left(\frac{1}{z} - \frac{1}{z'}\right)^2 \xi^2$ is the square of the angular deviation, and which is easily seen to agree with known results.

7. Returning to the crystal, let it be bounded by a second surface of revolution, infinitely near to the former, and about the same axis; and let the light emerge at this second surface into a vacuum again. The equation of the second surface being

$$\zeta' = \frac{1}{2} r' \xi'^2 + \frac{1}{4} s' \xi'^4, \quad \ldots\ldots\ldots \quad (27.)$$

and the ordinate of the intersection of the emergent ray with the axis being z'', the formula (24.) will apply to this new case by merely changing r, s, z', z respectively to $-r'$, $-s'$, $-z''$, $-z$, without changing μ, ν, ξ; and we have

$$\left.\begin{aligned}&\frac{-\nu^2}{\mu z} + \frac{1}{z''} + (\mu - 1)\, r'\\ &\quad = \frac{\xi^2}{2}\left\{\frac{-\nu^4}{\mu^3 z^3} + \frac{1}{z''^3} - 2(\mu - 1)\, s' + 2r'\left(\frac{\nu^2}{\mu z^2} - \frac{1}{z''^2}\right)\right\}.\end{aligned}\right\} (28.)$$

And, adding the two equations (24.) and (28.), we find

$$\left.\begin{array}{l} \dfrac{1}{z''} - \dfrac{1}{z'} - (\mu - 1)(r - r') \\ \quad = \dfrac{\xi^2}{2}\left\{\begin{array}{l} \dfrac{1}{z''^3} - \dfrac{1}{z'^3} + 2(\mu - 1)(s - s') \\ + \dfrac{2(r' - r)\nu^2}{\mu z^2} - \dfrac{2r'}{z''^2} + \dfrac{2r}{z'^2} \end{array}\right\}; \end{array}\right\} \quad \ldots\ldots (29.)$$

a formula for the focal lengths and aberrations of a lens of uniaxal crystal bounded by any two infinitely near surfaces, which have the optical axis of the crystal for their common axis of revolution: all cases of cusps or other singular curvatures at the common vertex being here set aside.

8. We shall content ourselves at present with drawing two conclusions from this formula. First, that because the extraordinary index ν disappears from the part unaffected with the small factor ξ^2, the central focus of the extraordinary rays, after emerging from the thin crystalline lens, coincides with the central focus of the ordinary rays which emerge from the same lens, wherever in the axis the focus of the incident rays may be: whereas Malus [*], misled by an error of sign in a radical which he employed for expressing the law of extraordinary refraction in the case of an uniaxal crystal, thought that these foci, ordinary and extraordinary, might differ widely from each other. And second, that on account of the presence of ν in the term

$$\frac{(r' - r)\nu^2 \xi^2}{\mu z^2}, \quad \ldots\ldots\ldots\ldots \quad (30.)$$

the ordinary and extraordinary aberrations cannot be exactly the same (the power $(\mu - 1)(r - r')$ of the lens being supposed to be different from 0), unless the z in the denominator of this term become infinite; that is, unless, wherever the focus of incident rays may be, the lens is so placed as to allow the rays within the crystal to be exactly parallel to the axis. If then a lens of this sort be used for the object-glass of a telescope, it seems to be desirable that its anterior surface, or that on which the parallel rays fall, should be plane, and that the correction of the aberration of figure should be effected entirely by another lens, composed of an uncrystallized material.

W. R. H.

Observatory of Trinity College, Dublin,
September 14, 1841.

---

[*] Perhaps it may not be improper to mention here, as I am not aware that the correction of Malus's result, respecting the extraordinary focal length of a lens of uniaxal crystal, has been hitherto published by any other person, that this correction occurred to me many years ago, in treating the question by my own methods, which made it scarcely possible to fall into the same error. The discrepance between my conclusion and his was, however, so great, that I was at first perplexed to account for it, until I traced that error of sign in his calculations, to which allusion has been made above.

XLIII. *Experiments on the alleged Conversion of Carbon into Silicon.* *By* R. H. Brett, *Ph. D., and* J. Denham Smith, *Esq.*

To Richard Phillips, Esq.

Dear Sir,

In the Transactions of the Royal Society of Edinburgh of this year, some experiments have been detailed by Dr. S. M. Brown, announcing the startling discovery, that carbon in certain states of combination is susceptible of conversion into silicon, and inviting a repetition of the trials made by the author.

Anxious to satisfy ourselves respecting the accuracy of the statements made by Dr. Brown, we instituted the subjoined experiments for the purpose of verifying the results which he details.

In his experiments Dr. Brown has made use of paracyanogen, obtained by the decomposition of the bicyanide of mercury; the paracyanogen employed by us was produced by the decomposition of hydrocyanic acid; this was of a brownish black colour, and soluble in sulphuric acid, from which it was precipitated on the addition of water. It contained traces of sulphate of lime and iron.

We have deemed it unnecessary to repeat the whole of the numerous and, in many cases, prolonged experiments given in the paper, but have selected those which bore most directly on the stated transformation; the modes of procedure and the results arrived at by us, we shall describe without explanation or comment.

In page 231 of the Transactions we find it stated, that a tightly luted Berlin porcelain crucible, filled with paracyanogen, was imbedded in stucco paste and exposed to a white heat for an hour and a half. The residue of this experiment was a dark brown infusible substance like charcoal; this, when ignited with carbonate of potash, and the product treated with hydrochloric acid, is described as leaving a fine white gritty powder insoluble in acids and alkalies, infusible in microcosmic salt, and dissolving with effervescence in fused carbonate of potash, and then forming a solution in water, from which it was separable as a bulky gelatinous precipitate by acids, which precipitate was soluble both in acids and alkalies, and insoluble in either after ignition to redness. It is further stated, that "two grains of the dark ignited substance procured by the last process yielded 4·11 grs. of silicic acid; one grain, 2·06 grs., and 0·8 gr. 1·57."

This experiment was repeated in the following manner:—50 grs. of paracyanogen, obtained by the spontaneous decomposition of prussic acid, were stuffed into a small crucible of Berlin porcelain furnished with a cover; this covered crucible was completely imbedded in plaster of Paris made into a paste with water, in a Hessian crucible; a cover was then placed over the latter and luted with China clay.

This apparatus was placed in a sand-bath to dry, and afterwards submitted for three hours to a full red heat; when cold, the smaller crucible was carefully opened; there was found a perfectly black carbonaceous-looking residue, weighing 4 grs.; this black residue was ignited in a platinum capsule over a gas flame, and there remained a very inconsiderable light and grayish-coloured ash, which, when boiled in aqua regia, entirely dissolved. The ash in question consisted of sulphate of lime and traces of oxide of iron.—(R. H. B.)

Again, 50 grs. of paracyanogen were placed in a porcelain crucible under precisely similar circumstances to the last experiment. The crucibles in this case were exposed to a full white heat for two hours. When the smaller crucible was opened, a black residue, like that obtained in the former experiment, was found; this residue, when burnt in a platinum crucible with access of air, was consumed, with the exception of a trifling ash entirely soluble in aqua regia, and consisting of sulphate of lime and oxide of iron, as in the former experiment.—(R. H. B.)

Not content with the results of the two experiments just detailed, 50 grs. of paracyanogen were again subjected to an intense white heat in a furnace capable of fusing metallic iron and manganese, having previously rammed it tightly into a Berlin crucible, to which was adapted a suitable cover; these were luted together very carefully; when the luting, which then presented no cracks, was dry, this crucible was placed in an earthen one, and the vacant space filled up with a ferruginous sand; this again was covered and securely luted; after ignition for two hours it was withdrawn, when the residuum proved to be a perfectly black substance, exactly resembling that mentioned in the former experiments, weighing about 5 grs. The sand had agglomerated into a solid mass resembling soft white sandstone, and owing to a portion of this getting mixed with the black residue in the crucible, the exact weight could not be ascertained. The interior glaze of the crucible, to the level of the original bulk of the paracyanogen, had assumed a black colour with a shade of brown, which apparently penetrated to the depth of the glaze, but no further; on exposure to long-continued heat with free access of air, this black lining underwent but a very slight, if any, change of tint. Having carefully selected 3·5 grs. of the black residue so as to procure it free from the accidental admixture of sand, they were mixed with 20 grs. of carbonate of potash prepared from the bitartrate and free from silica; this mixture, which was perfectly black, was ignited for half an hour in a closed platinum crucible, which was itself placed in a covered earthen crucible carefully luted; the fused residue was black, and when treated with water left a black insoluble powder; this, washed, collected, dried, and ignited in a platinum capsule and in contact with air, glowed like ignited carbonaceous matter, and left an ash-coloured residue, which, treated with chlorate and carbonate of potash, gave a fused residuum entirely soluble in water; this solution, evaporated to dryness with the addition of hydrochloric acid in excess, and subsequently ignited, gave a slightly turbid solution with water, which became perfectly clear on the addition of hydrochloric acid; this solution contained traces of platinum. The first solution of carbonate of potash from the black residue treated in the manner described, gave not the least indication of silica.—(J. D. S.)

Having observed that the black lining of the crucible was precisely similar to the description given by Dr. Brown of the films of silicon mentioned by him as lining the porcelain crucible in which his experiment was made, and which process he recommends as likely "eventually to be an œconomical and convenient way of lining porcelain with silicon for experimental purposes," we were desirous of ascertaining whether a cheaper and more easily to be obtained substance containing carbon, could not be advantageously substituted for paracyanogen, so as to produce the desired object; we therefore placed some lamp-black, mixed with a trace of peroxide of iron, in a Berlin crucible, into which it was tightly rammed, and exposed this crucible, having previously put it into an exterior one, which was covered and luted, to a white heat for an hour and a half: when withdrawn it was found to be coated, as high as the lamp-black had reached, with a stain, differing only from that before alluded to as obtained from paracyanogen, in being quite black; this crucible was exposed to a red heat with free access of air for upwards of two hours, but the stain remained unchanged.—(J. D. S.)

We do not say that this is not silicon, but if it be, either lamp-black is as capable of lining crucibles with silicon as paracyanogen is, or at high temperatures the carbon is capable of reducing the silica of the glaze, and thus producing the same effect.

In page 235 Dr. Brown observes, that "before leaving the production of silicon from uncombined paracyanogen, there is another mode of operating to be mentioned, and it is equally remarkable for simplicity and freedom from any intelligible source of fallacy." "Triturate crude paracyanogen with an excess of carbonate of potash, and fuse the mixture two hours at a full white heat in a closed platinum crucible. Paracyanogen disappears; there is no free carbon in the white saline product; but it yields a conformable proportion of silicic acid when treated in the ordinary method of analysis for that compound." "This process is more striking when subborate of soda is substituted for potassa; for when the product is treated with acids, there is no effervescence of carbonic acid; and it must be remembered, once for all, that in every professed process of transformation, the disappearance of carbon has to be accounted for as well as the new formation of silicon." Dr. Brown further states, that his crude paracyanogen contains "nearly a third of its own weight of condensed cyanogen," and "that it yields a weight of silicon never less than an eleventh, and never more than a twelfth, under the calculable weight of the constituent carbon, the cyanogen of absorption being dissipated in the course of the processes;" by which we presume is meant, that from crude paracyanogen he always obtained between 27·5 and 28 per cent. of silicon, a proportion so large that, when converted into silicic acid, it is impossible it could escape observation, even when the quantities of paracyanogen subjected to experiment are very small.

As the two processes just quoted are strongly recommended by Dr. Brown for their "simplicity and freedom from any intelligible source of fallacy," and as they appeared to us to bear so directly upon the question of transformation as to be absolutely crucial experiments, we have followed the formula detailed by Dr. Brown with the greatest exactitude and care in the following experiments:—

Fifteen grs. of paracyanogen were mixed with 100 grs. of carbonate of potash prepared from the bitartrate: the mixture, placed in a closely covered platinum crucible, and this put into an earthen one, which was covered and carefully luted, was ignited at a full yellow heat for two hours; on examination when cold, a portion of the salt, probably cyanide of potassium, was found to have volatilized, cementing the cover to the platinum crucible, and the residual salt was white; this, treated with distilled water, gave a slightly opalescent solution, which, when a small portion was treated with a mixture of per- and protosulphate of iron and hydrochloric acid, gave an abundant precipitate of Prussian blue; to the solution, hydrochloric acid was added in excess, which occasioned the evolution of the characteristic odour of hydrocyanic acid; this acid solution, evaporated to dryness, ignited to redness, and treated with water, left a trace of reddish matter entirely soluble in hydrochloric acid.—(J. D. S.)

Five grs. of paracyanogen were then mixed with about 40 grs. of dried borax, and exposed to a white heat for full two hours in a platinum crucible, covered and placed in a Hessian-luted crucible, as in the preceding experiment; when withdrawn, the Hessian crucible was completely glazed exteriorly, and had evidently been softened by the heat; in the platinum crucible there remained a glassy substance of a light sea-green colour; this, treated with water, gave an alkaline solution, which *precipitated lime-water and effervesced with hydrochloric acid,* evaporated the acid solution to dryness and fused the residue; again treated the residue with hydrochloric acid, and after evaporation to dryness, fused; the fused mass, treated with distilled water and hydrochloric acid, left a slight sediment; when this sediment was fused with carbonate of potash and the residue treated with hydrochloric acid, evaporated to dryness and ignited, the ignited chloride of potassium was wholly soluble, without any sediment whatever, in water acidulated with hydrochloric acid; the solution contained traces of lime.—(J. D. S.)

This experiment was repeated upon rather a smaller scale, with precisely the same results.

Notwithstanding the decisive nature of the experiments just detailed, we determined, in order to avoid every objection, to subject paracyanogen prepared from bicyanide of mercury—the paracyanogen employed by Dr. Brown—to the process indicated by him as simple and free from fallacy. Some bicyanide of mercury was powdered and shaken down closely in a porcelain crucible, which was then covered and firmly luted; this was placed in another crucible, also covered and subjected to a full red heat for half an hour; the residue was black and coherent; a portion of this ignited left a trace of oxide of iron, and it was almost entirely soluble in sulphuric acid.

Twenty grs. of this paracyanogen were mixed with 120 grs. of carbonate of potash and ignited for one hour and a half at a full yellow heat, the same precautions being taken as in the experiment before cited; when withdrawn, the residual salt was white, soluble in distilled water, gave with mixed per- and protosulphate of iron and hydrochloric acid a precipitate of Prussian blue, and evolved much hydrocyanic acid on the addition of hydrochloric acid, which occasioned no precipi tate: this acid solution, evaporated to dryness, ignited, and again treated with hydrochloric acid and water, left no perceptible residue; whereas, from the statement of Dr. Brown (p. 236), if rightly understood by us, we ought to have obtained at least 11 grs. of silicic acid from this quantity of paracyanogen.—(J. D. S.)

We may here be allowed to remark, in explanation of the circumstance which seems to Dr. Brown so difficult to account for in the experiments just described, viz. "the disappearance of the carbon" of the paracyanogen when ignited with carbonate of potash, that it is probable that one portion of the paracyanogen decomposes the carbonate of potash forming carbonic oxide, mixed perhaps with carbonic acid, azote and potassium, and that the last-named substance combines with that portion of paracyanogen which is decomposed by heat into cyanogen, forming cyanide of potassium, a portion of which is volatilized, whilst, as we have seen, the remainder is found mixed with the excess of carbonate of potash used in the process: other explanations may be given, but this appears to us to be both the most feasible and the most in accordance with the facts we have noticed.

Although the foregoing experiments were as direct and, to us, as decisive as we could desire respecting the alleged transformation of carbon, we nevertheless determined to pursue the investigation further, so as to render it complete; we therefore turned our attention to the experiments of Dr. Brown (p. 236–7) on the formation of "compounds of silicon with copper, iron, and platinum, by the reaction of paracyanogen on these metals."

Three grains of paracyanogen from decomposed prussic acid were tightly wrapped up in platinum foil and placed in a porcelain crucible, the bottom and sides of which were lined with platinum foil, so as to prevent the foil containing the paracyanogen from coming in direct contact with the glazed lining of the crucible; a cover was placed over the crucible, and the whole imbedded in plaster of Paris paste contained in a Hessian crucible; when dry, the apparatus was exposed to a white heat for two hours. The plaster of Paris had become vitrified by heat, and a portion of this vitrified matter found entrance into the porcelain crucible; the latter, however, when broken up, displayed the platinum of a brilliant colour, and of softer texture than foil not previously exposed to so high a temperature. It could hardly therefore be supposed that the platinum had been acted upon by silicon, which renders it brittle, and does not render the metal more brilliant; it was obviously useless to act upon the platinum by aqua regia for the purpose of determining the quantity of silicon, if any, which it might contain, from the fact just noticed of vitrified matter having got accidentally into the crucible.—(R. H. B.)

Twenty grains of precipitated copper were mixed with five grains of paracyanogen, and placed in a covered and luted porcelain crucible; this was placed in another crucible, also covered and carefully luted, and the apparatus exposed to a white heat for one hour and a half; when opened, the contents consisted of numerous metallic globules, mixed with a black powder; these globules were malleable, and wholly dissolved in nitric acid, whilst the black powder was consumed when ignited in contact with atmospheric air.—(J. D. S.)

Another experiment was made by rolling up some very thin sheet-copper, so as to form a tube of considerable thickness, into which five grains of paracyanogen were tightly rammed, and each extremity of the tube secured by hammering; this was placed in an earthen crucible, covered and tightly luted, along with a platinum tube made of foil filled with paracyanogen, in the same way as the copper one above described, and exposed to a bright yellow heat for one hour and a half; when the crucible was withdrawn and opened, the contents were found to be fused into a button, which was only slightly malleable, breaking readily; having removed the exterior of this button, which adhered to the crucible, it was treated with nitric acid, which left a residue having the appearance of gray metallic particles mixed with black carbonaceous matter; by ignition with access of air the black powder burnt away, leaving a gray residue entirely soluble in nitro-muriatic acid.—(J. D. S.)

Having hitherto so completely failed in our endeavours to obtain results similar to those detailed by Dr. Brown in the paper under consideration, we resolved to conclude our experimental examination of it, by endeavouring to procure silicic acid, either with or without the admixture of carbonate of potash, from ferrocyanide of potassium. These experiments are not so satisfactory as the preceding ones, owing to the energetic action of the mixed ferrocyanide and carbonate upon wrought iron at the high temperatures, but are nevertheless useful as serving to confirm the results we had previously arrived at.

An experiment was made upon ferrocyanide of potassium, by drying it carefully, and pressing 700 grains of it into a covered porcelain crucible, which was then carefully luted, and when dry plunged into a paste of plaster of Paris contained in a Hessian crucible; this was well dried on a sand-bath, then covered, luted, and exposed to a white heat for five hours; when withdrawn from the fire and opened whilst still warm, some portions of the contents glowed like a pyrophorus; the sulphate of lime had fused down, and had acted on the exterior of the porcelain crucible; the contents consisted of a light sooty spongy mass, very friable, and containing small globules of a metallic character, whilst the crucible itself was lined with a black enamel, similar to the stain noticed in the former experiments. Nine grains of this residue, from which the metallic globules had been separated, were mixed with 30 grains of nitrate of potash, and the same weight of carbonate of potash prepared from the bicarbonate, and the mixture fused in a platinum crucible; when cold, the fused mass, treated with water, afforded a solution of a deep purple colour, exactly resembling permanganate of potash; by rapid filtration this solution was obtained clear; decomposition appeared to be going on during filtration, for the liquid whilst on the filter was in a state of slight effervescence from the escape of a gas; by exposure to atmospheric air this solution rapidly decomposed, gradually losing its pink colour, becoming turbid, and finally deposited a reddish brown powder, leaving a colourless solution; this brown powder proved on examination to be peroxide of iron without a trace of manganese*.

The colourless and alkaline solution, tested in the usual way for silica, afforded a trace of this substance, which having reason to believe resulted from impurity in the carbonate of potash used in this experiment, it was tested for silica, and found to contain apparently the same quantity as that obtained from the alkaline solution just noticed; in each case the quantity was so minute as not to affect the most delicate balance, though appreciable by the eye. The oxide of iron, separated by filtration from the purple solution, and tested in the usual way for silica, did not afford the slightest trace of this substance.

The metallic globules before noticed were treated with hydrochloric and nitric acids; a black residue was obtained, which, ignited, left a light brown ash soluble in hydrochloric acid.—(J. D. S.)

Two hundred grains of carbonate of potash, heated in an iron tube to redness for three hours, and tested in the usual way for silica, gave 0·3 grain of this substance; when the quantity of silica contained in the carbonate of potash was thus ascertained, 200 grains of it were mixed with 50 grains of dry prussiate of potash, and ignited to redness in the same iron tube, closed with an iron plug and luted with China clay, for three hours; when cool, the contents of the tube were washed out with water and a little hydrochloric acid; a powerful odour of prussic acid was observed, and when the hydrochloric acid had been added to supersaturation, abundance of Prussian blue was formed; this was evaporated to dryness, and pure nitric acid added to the residue, the mixture transferred to a flask and heated; when the Prussian blue was entirely decomposed, the contents of the flask were evaporated to dryness, ignited, and boiled with water and hydrochloric acid; the colourless insoluble matter collected, dried and ignited, proved to be silica, and weighed 0·33 grain, showing a gain of 0·03 grain of silica, a quantity so small as readily to have been originally mixed with the 50 grains of ferrocyanide of potassium, or to be merely the error of experiment.—(R. H. B.)

---

* I believe that this combination of oxide of iron with an alkali has before been noticed, but I cannot remember the authority, nor do I think any investigation of the fact was attempted. It appears to me clearly to indicate the existence of iron in a state of oxidation, doubtless higher than any we are yet acquainted with, analogous to the acid oxides of manganese. I shall pursue this investigation, and endeavour to isolate, or at least ascertain the nature and composition of, this apparent *ferric acid*(?).—J. D. S.

[See *Journal de Pharmacie,* tom. xxvii. p. 97.—Ed.]

When 30 grains of dried ferrocyanide of potassium were mixed with four times its weight of pure carbonate of potash and ignited in an iron box, inclosed in a closely covered and luted earthen crucible, to a white heat for three hours, the iron box was found on examination to have fused, and this fused iron quite malleable: this experiment was of course spoilt, and is merely mentioned to show the heat employed by us when we speak of a *white* heat.—(J. D. S.)

Another experiment, with twice the quantities of ferrocyanide and carbonate employed in the last experiment, and exposed to a full yellow heat for three hours, also failed from the energetic action of the fused contents on the iron box, by which the bottom of the box was corroded and perforated. —(J. D. S.)

Another experiment was tried upon 150 and 600 grains of the ferrocyanide and carbonate respectively, and ignited in a gun-barrel, protected by luting and well secured at the orifice, for six hours, at a full yellow heat. Notwithstanding the precaution of luting, the gun-barrel was corroded, and evidently from the action of the fused materials, and thus a great portion of the contents were lost; what remained was washed out by digesting water upon it for twenty-four hours, and the black insoluble matter separated from the alkaline solution, which latter evolved a strong odour of hydrocyanic acid on the addition of hydrochloric acid; the black residue, when ignited, was converted into a reddish brown powder, entirely soluble in hydrochloric acid: when these two solutions were evaporated together to dryness, and the Prussian blue formed decomposed by nitric acid, the residuum was fused and then treated with hydrochloric acid, which left a trace of insoluble matter, probably silica, but which was so minute as not to affect a balance sensible to $\frac{1}{1000}$ gr.; this minute quantity was most probably accidental, and very probably was derived from the iron tube which was corroded. The weight of the residual chlorides, when evaporated to dryness, was 104 grs., so that if we admit that only one-seventh of the quantities originally employed were tested for silica, we should have above 20 grs. of ferrocyanide of potassium submitted to this trial, which, according to Dr. Brown (p. 245), should yield 3·6 grs. of silicic acid.—(J. D. S.)

Subsequently to these experiments, it was observed in two or three instances, that when paracyanogen made from the bicyanide of mercury was submitted to the blowpipe flame in a tube of common flint-glass, closed at one end and drawn to a capillary at the other, that gaseous matter was evolved, and a small hole blown out at that end containing the paracyanogen, through which aperture the gas escaped, and invariably burnt with the characteristic flame of cyanogen.—(R. H. B.)

This experiment was followed by placing, in a tube of Bohemian glass, having a bulb at one extremity, a small quantity of paracyanogen, prepared by dissolving the dark-coloured product which results from the spontaneous decomposition of hydrocyanic acid in sulphuric acid, and precipitating by water, well washing, and drying at a temperature of 260° to 300° Fahr., and plugging up the opening of the tube with plaster of Paris. After causing the blowpipe flame to play upon the bulb containing the paracyanogen for a short time, a small hole was blown out as before, and cyanogen continued to burn for some time.

Some more paracyanogen, prepared in a similar manner, was placed in a similar tube of Bohemian glass, but in this instance the bulb was carefully coated with luting, the open extremity plugged as before, and the apparatus carefully dried; the flame of the blowpipe was then applied to the bulb for nearly half an hour without any aperture being made in it. When the contents of the bulb were removed, they presented precisely the appearance of precipitated peroxide of iron immediately after ignition, viz. a dark black glistening aspect; this residue, when burnt in a platinum capsule, was entirely dissipated, leaving no residue whatever.— (R. H. B.)

The disengagement of cyanogen in the last experiment but one is completely at variance with the statement of Dr. Brown in his paper 'On Paracyanogen,' p. 168, "that *pure* paracyanogen (precipitated by atmospheric moisture from the sulphuric acid solution of the common product) does not afford the slightest appearance of cyanogen, warrants the conclusion that paracyanogen once formed from cyanogen or its elements cannot be rechanged into cyanogen by heat." For it can hardly, we apprehend, be argued that the paracyanogen employed in our experiment, prepared from the crude product resulting from the decomposition of hydrocyanic acid, solution in sulphuric acid, and precipitation by water, is less pure than that obtained from paracyanogen procured from bicyanide of mercury, dissolved in sulphuric acid, and gradually precipitated by the absorption of atmospheric moisture; nor can we see how it can be argued that the failure in effecting the transformation of carbon into silicon, in the experiment just detailed, can be attributed to the employment of an impure paracyanogen, or to a substance used not being paracyanogen, because it was not obtained by the action of heat on bicyanide of mercury.

As our object in performing the experiments we have detailed was simply an inquiry respecting the fact of the statement made by Dr. Brown, of the conversion of the carbon of paracyanogen into silicon, we have not entered into a critical examination of the paper, nor do we attempt to offer any explanation of the results arrived at by us in every one of our experiments (that *the carbon of paracyanogen is incapable*, by such of the processes as we have tried, which are recommended by Dr. Brown, *of conversion into silicon*), being in every instance in complete opposition to those published by that author. We leave those interested in this subject to decide whether Dr. Brown's experiments or ours are correct, and conclude this examination by stating that the terms "white heat" and "yellow heat," when employed by us, mean respectively temperatures at which malleable iron and manganese fuse in the first case, and at which copper melts in the second; whereas Dr. Brown's white heat is a temperature evidently below our yellow heat, as he speaks of exposing a copper tube to a white heat for upwards of an hour, and does not mention the fusion of the "gypsum" when kept at a white heat for one hour and a half.

Liverpool, Sept. 17, 1841.

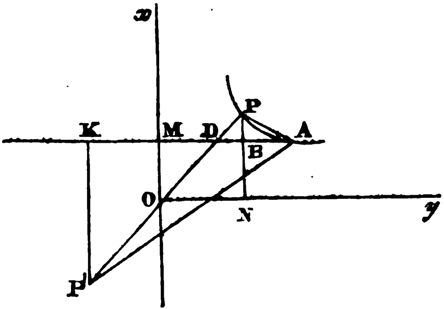
XLIV. *Solution of a Geometrical Problem on the Form of the Black Cross in Biaxal Crystals.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

In turning over some old papers, I have fallen upon the following very simple method of solving a geometrical problem which occurs in investigating the form of the black cross in biaxal crystals. Sir J. Herschel has given a solution in Art. 1072 of his 'Treatise on Light,' but which, in my opinion, is unnecessarily tedious and prolix.

Problem.—Required a curve P A, such that a line drawn from A bisecting the < between lines A P, A P′ drawn to two given points P, P′, shall always be $\perp^{\text{ar}}$ to a given line x O.



Solution.—Let $OP = OP' = a$, $OM = x$, $MA = y$, $PAD = P'AD = \theta$, $DOM = \alpha$.

By Euclid,

$$\frac{AP'}{AP} = \frac{P'D}{PD} = \frac{a + \dfrac{x}{\cos\alpha}}{a - \dfrac{x}{\cos\alpha}} = \frac{a\cos\alpha + x}{a\cos\alpha - x}. \quad . \quad . \quad . \qquad (1.)$$

$$\text{Also,}\quad KA = AP'\cos\theta = KM + MA = a\sin\alpha + y$$
$$AB = AP\cos\theta = AM - MB = y - a\sin\alpha;$$

$$\therefore\ \frac{AP'}{AP} = \frac{a\sin\alpha + y}{y - a\sin\alpha} = \frac{a\cos\alpha + x}{a\cos\alpha - x} \text{ by (1.)}$$

$$\therefore\ \frac{y}{a\sin\alpha} = \frac{a\cos\alpha}{x}, \text{ or } xy = a^2\sin\alpha\cos\alpha = \frac{a^2}{2}\sin 2\alpha.$$

Trinity College, Cambridge, Sept. 16, 1841. J. E.

XLV. *On Malic Acid, and the Changes undergone by its Salts at High Temperatures.* By Robert Hagen, *Ph. D.*[*]

AFTER the publication of Mr. Graham's observation, that phosphoric acid, in its different states, possesses the property of combining with one, with two, and with three atoms of base respectively, it was discovered by Liebig that the same

[*] Read before the Chemical Society, June 1, 1841, having been translated and communicated by T. G. Tilley, Esq., now communicated by the Society.

law holds good in part with many organic acids; some of these acids requiring two, and some three atoms of base, to form neutral salts. The hydrates of each acid contain a corresponding number of atoms of basic water, which cannot be removed without the decomposition of the acids themselves. These chemists showed that, though a salt of any acid, with magnesia or with oxides isomorphous with it, possessed the power of combining with a potash salt of the same acid, and forming a double salt, such as the sulphate of magnesia and potash, that is not ground for doubling the atomic weight of the acid, or for viewing it as bibasic.

They proved at the same time that a monobasic acid is incapable of forming a double salt with two isomorphous bases. The proportion of base which unites with a polybasic acid is constant, generally either two or three atoms. In the memoir on organic acids by Liebig, here alluded to, he had made it not improbable that *malic acid* is bibasic. At his suggestion I have made several analyses of its various salts, which form the subject of this paper.

Malic acid was first discovered by Scheele in the juice of the apple; it was again discovered by Donovan * in the juice of several plants, and described by him as a new acid. The identity of the acid of Donovan with malic acid was proved by Braconnot. This acid has been most fully described by Liebig. It has also been partially examined by Pelouze, Braconnot, and Richardson.

The malic acid used in the present investigations was prepared from the expressed juice of the berries of the *Sorbus aucuparia*, or Service tree, in the following manner. The expressed juice was mixed in a copper pan with finely divided and levigated hydrate of lime, care being taken not to saturate the fluid completely, but to allow it to remain sensibly sour. Being placed on the fire, it was made to boil for some hours, during which time it gave off a peculiar pungent vapour, which strongly affects the eyes. By degrees neutral malate of lime precipitates, and may be removed with a ladle. By continued boiling, more of the salt is obtained. When no more falls, the vessel is removed from the fire, and allowed to cool, when a little more is precipitated. We must take care in the beginning not to saturate the expressed juice entirely with lime, or so much colouring matter falls with the malate of lime, as to render the acid impure. The neutral malate of lime thus obtained, is dissolved in dilute nitric acid (1 part acid to 10 of water), filtered and evaporated; upon

---

[* Mr. Donovan's paper was reprinted from the Phil. Trans. in Phil. Mag., first series, vol. xlvi. p. 433.]

cooling, acid malate of lime crystallizes out in perfectly colourless crystals. It must be well washed with cold water, again dissolved in boiling water, and precipitated by acetate of lead. The lead salt is decomposed by sulphuretted hydrogen, and the malic acid obtained pure by evaporation.

Malic acid forms with bases two neutral salts, one of which becomes anhydrous when dried at 100° C., while the other still retains water at that temperature. It possesses decided bibasic properties, and the hitherto received atomic weight is necessarily doubled.

The following salts have been examined:—

MALATES OF LIME.

a. Neutral anhydrous Malate of Lime.

$$C_8 H_4 O_8 + 2\, Ca\, O, \text{ or } M\, 2\, Ca\, O.$$

This salt is obtained by saturating a solution of malic acid with lime water. It is a crystalline powder, perfectly insoluble in hot and cold water.

0·489 gramme of this salt gave 0·319 sulphate of lime, or 32·188 per cent. lime. This gives for the atomic weight of the salt the number 2212·40.

		Calculated.	Found.
1 equivalent of Malic acid	1461·39	67·24	67·81
2 equivalents of Lime . . .	712·04	32·76	32·18

b. Neutral hydrated Malate of Lime.

$$\overline{M}\ 2\, Ca\, O + 5\, aq.$$
$$\overline{M}\ 2\, Ca\, O + 4\, aq.\ (100°\, C.)$$

If acid malate of lime is saturated with potash, soda, or ammonia, and the solution evaporated at a gentle temperature, we obtain instead of a double salt, malate of lime with 5 equivalents of water, in hard shining crystals. When heated to 100° C. this salt is converted into a porcellanous mass, and is found to have lost one atom of water. At 150° C. it becomes quite anhydrous. Of the salt in its first state of hydration, 0·422 gramme dried at the temperature of the atmosphere, gave 0·2655 sulphate of lime, or 26·113 per cent. lime, which makes the atomic weight of the salt 2725·0, and gives the following composition:—

		Calculated.	Found.
1 eq. Malic Acid	1461·39	53·44	
2 ... Lime . . .	712·04	26·03	26·113
5 ... Water. . .	562·40	20·53	
	2735·83	100·00	

Of the salt dried at 100°:—

(1.) 0·6135 gramme gave 0·4045 sulphate of lime = 27·383 per cent. lime; and consequently the atomic weight, 2600·0.

(2.) 0·3660 gramme gave sulph. lime 0·241, or 27·344 per cent. lime.

(3.) 0·335 salt, gave 0·353 sulphate lime, and consequently the atomic weight, 2598·28.

These give—

			Calculated.	Found.	
			1.	2.	3.
1 Malic acid	1461·39	56·71			
2 Lime . . .	712·04	27·14	27·38	27·34	27·40
4 Water . .	449·92	17·15			
	2623·35				

The previous analyses of the lime salt dried without heat, shows the necessity of doubling the atomic weight of the acid, as otherwise we should be obliged to give it the formula

$$C^4 H^2 O^4 Ca O + 2\tfrac{1}{2} aq.;$$

which is at variance with the atomic theory.

ACID MALATE OF LIME. $\overline{M}$ Ca O H^2 O + 6 aq.

This salt is obtained when neutral malate of lime is dissolved in nitric acid. It crystallizes in large transparent octahedrons. Dried at 100° C. it loses water, and is converted into a viscid, stringy mass.

0·706 salt gave 0·2345 sulphate of lime, or 13·794 per cent. lime; atomic weight, 2585·8.

		Calculated.	Found.
1 atom Malic acid	1461·39	56·10	
1 atom Lime . . .	356·02	13·67	13·79
7 Water	787·36	30·23	
	2604·77		

Richardson and Merydorf concluded the formula of this salt to be, $\overline{M}$ Ca O H^2 O + 8 aq.

MALATES OF MAGNESIA.

a. Neutral hydrated Malate of Magnesia.

$$\overline{M}\ 2\,Mg\,O + 10\,aq$$
$$\overline{M}\ 2\,Mg\,O + 2\,aq\ (100° C.).$$

This salt is obtained by boiling magnesia in a solution of malic acid, and crystallizing. It loses 8 atoms of water by 100° C.

0·5505 salt gave 0·2708 sulphate of magnesia, equivalent to 16·713 per cent. of magnesia; and makes the atomic weight 3091·4.

		Calculated.	Found.
1 eq. Malic acid	1461·39	47·09	
2 ... Magnesia..	516·70	16·66	16·713
10 ... Water. . .	1124·8	36·55	
	3102·89		

Of the salt dried at 100° C., 0·466 gave 0·109 sulphate of magnesia, equivalent to 23·390° per cent. magnesia.

		Calculated.	Found.
1 at. Malic acid	1461·39	66·34	
2 ... Magnesia .	516·70	23·45	23·39
2 ... Water . .	224·96	10·21	
	2203·05		

This salt was also analysed by Professor Liebig with the same result.

b. Neutral anhydrous Malate of Magnesia. $\bar{M}\ 2\,MgO$.

This salt is obtained by precipitating a saturated solution of the former salt (*a.*) with alcohol, and drying at 100° C.

0·344 salt gave 0·0935 magnesia, equivalent to 26·945 per cent.; atomic weight, 1906·88.

		Calculated.	Found.
1 atom Malic acid	1461·39	73·83	
2 atoms Magnesia	516·70	26·12	26·94
	1978·09		

ACID MALATE OF MAGNESIA. $\bar{M}\ MgO\,H^2O + 3\,aq$

$\bar{M}\ MgO\,H^2O + aq\,(100°)$.

Obtained by dividing and saturating one half of the malic acid with carbonate of magnesia, and evaporating to crystallization. It loses 2 atoms of water by 100° C.; at a higher temperature it melts.

1·1755 salt gave 0·1405 magnesia, equivalent to 11·952 per cent.; and for the atomic weight, 2161·5.

		Calculated.	Found.
1 atom Malic acid	1461·39	67·35	
1 ... Magnesia	258·35	11·91	11·952
4 atoms Water ...	449·91	20·74	
	2169·66		

Of the salt dried at 100° C., 0·698 gave 0·0795 magnesia, or 13·294 per cent.; atomic weight, 1943·0.

			Calculated.	Found.
1 eq.	Malic acid	1461·39	75·147	
1 ...	Magnesia	258·35	13·285	13·294
2 ...	Water . .	224·96	11·568	
		1944·70		

MALATES OF ZINC.

a. Neutral Salt.

$$\overline{M}\ 2\,ZO + 6\,aq$$

$$\overline{M}\ 2\,ZO + -\ (100°\ C.).$$

Is prepared by digesting carbonate of zinc with malic acid, at a temperature not above 30° C. Dried at 100° C. it becomes anhydrous.

Of this salt, 0·4570 gave 0·2935 sulphate of zinc, or 32·179 oxide of zinc, making the atomic weight 3127·8; the calculated one being 3142·77.

0·695 of the salt dried at 100° C. gave 0·566 sulphate of zinc, or 40·302 oxide of zinc; making the atomic weight 2463·6; the calculated is 2467·6.

ACID MALATE OF ZINC. $\overline{M}.\ ZOHO + 2\,aq.$

Prepared by adding excess of malic acid to the neutral salt.

0·190 of this salt gave 0·082 sulphate of zinc, or 21·343 per cent. of oxide of zinc; the atomic weight deduced from which is 2351·7. The composition of the salt is therefore—

			Calculated.	Found.
1 eq.	Malic acid . .	1461·4	63·480	
1 ...	Oxide of zinc	503·2	21·861	21·343
3 ...	Water	337·4	14·659	
		2302·0		

Braconnot analysed this salt with the same results.

BASIC MALATE OF ZINC.

If malic acid is long boiled with excess of carbonate of zinc, there falls down a sandy powder; of this salt dried at 100°,

(1.) 0·3935 gave 0·178, or 44·66 per cent. of oxide of zinc; atomic weight, 2253·3.

(2.) 0·255 gave 0·224 sulphate of zinc, or 44·015 per cent. of oxide of zinc; atomic weight, 2286·8.

(1.) 0·474, burnt with oxide of copper, gave 0·1075 water, and 0·329 carbonic acid.

(2.) 0·5510 gave water 0·1335, and 0·3885 carbonic acid. This salt is therefore composed of

		Calculated.		Found.	
12 at. Carbon . . .	917·22	20·19		19·191	19·24
9 ... Hydrogen. .	112·32	2·47		2·52	2·69
15 ... Oxygen . . .	1500·00	33·03		33·62	34·04
4 ... Oxide of zinc	2012·9	44·31		44·66	44·015
	4542·44				

Heated to 100° C. it lost 4 atoms of water; and 0·420 gave 0·411 sulphate of zinc, or 49·082 per cent. oxide of zinc; atomic weight, 2052·3. 0·4225 gave 0·0715 water, and 0·334 carbonic acid, which gives the formula $C^{12} H^5 O^{11} + 4 ZO$. This salt, however, is then essentially altered, part of its malic acid being converted into fumaric acid, as will be shown in the sequel.

ACID MALATE OF COPPER. $\overline{M}$ Cu O H^2O + 2 aq
$\overline{M}$ Cu O H^2O (100° C.).

Prepared by dissolving hydrated oxide of copper in malic acid, and evaporating at a temperature of 30° to 40° C., as a small blue crystalline body. 0·690 of this salt gave 0·149 oxide, or 21·521 per cent. of oxide of copper; atomic weight, 2302·17.

The composition calculated from this, is as follows:—

		Calculated.	Found.
1 atom Malic acid	1461·39	63·69	
1 ... Oxide of copper	495·70	21·60	21·821
3 atoms Water	337·44	14·71	
	2294·53		

Dried at 100° it loses 2 atoms of water, and its atomic weight becomes 2069·57.

MALATE OF SILVER. $\overline{M}$ 2 Ag O.

0·2877 salt gave 0·1777 silver, or 66·339 per cent. of oxide of silver. Hence

		Calculated.	Found.
1 eq. Malic acid. . .	1461·39	33·48	33·66
2 ... Oxide of silver	2903·21	66·52	66·33
	4361·60		

MALATE OF BARYTES. $\overline{M}$ 2 Ba O + 2 aq
$\overline{M}$ 2 Ba O (100° C.).

A solution of malic acid is saturated with barytes water, and evaporated at a very moderate temperature. The salt found crystallizes. At 30° C. it loses one atom of water; at 100° C. it becomes quite anhydrous.

Of the salt, 0·5575, dried at the ordinary temperature, gave 0·452 sulphate of baryta, or 53·207 per cent. of barytes; which makes the atomic weight 3956·8, the calculated one being 3600·10. The salt dried by 30° C. gave 54·426 per cent. barytes. That dried at 100° C. is composed of 1 atom of malic acid and 2 atoms of barytes.

MALATE OF STRONTIAN. $\overline{\mathrm{M}}\ 2\,\mathrm{St\,O} + 3\,\mathrm{aq}$
$\overline{\mathrm{M}}\ 2\,\mathrm{St\,O} + 2\,\mathrm{aq}$ (100° C.).

FUMARATE OF OXIDE OF ÆTHYL.

This æther is formed whenever malic acid is brought into contact with muriatic æther. Malic acid also, when long mixed with absolute alcohol, or with strong fuming hydrochloric acid, is converted into fumaric acid.

This combination, or fumaric æther, is heavier than water, and has a grateful odour, like that of fruit. It is slightly soluble in water, and is therefore better separated from muriatic æther, when mixed with the latter by distillation, than by means of water. By potash, fumaric æther is converted into alcohol and the fumarate of potash. Kept long in contact with ammonia, it is converted into fumaramide. Purified by being distilled over chloride of calcium, 0·3315 æther gave 0·208 water, and 0·669 carbonic acid. Hence

			Calculated.	Found.
8 atoms	Carbon . .	611·48	56·29	55·803
6 ...	Hydrogen	74·87	6·89	6·97
4 ...	Oxygen . .	400·00	36·82	37·22
		1086·35		

FUMARAMIDE. $\mathrm{C^4\,H\,O^2,\,N\,H^2}$.

This amide is obtained when fumaric æther is left a long time in contact with an excess of caustic ammonia. Its formation is quite analogous to that of oxamide and the other compounds of amide. In cold water and absolute alcohol it is quite insoluble. It is soluble in boiling water, and again precipitates as the water cools. Left long in contact with water, it is completely converted into fumarate of ammonia. Ammonia is disengaged by the fixed alkalies, and a fumarate formed. By distillation it is decomposed into ammonia; a white body, probably maleinic acid, and a residue of charcoal are left.

0·426 of fumaramide gave 0·1335 water, and 0·2780 carbonic acid. By a qualitative determination of the nitrogen, the latter was found to be to the carbonic acid in volume as 1 to 4.

Carbon	42·37	
Hydrogen . . .	5·33	
Nitrogen . . .	24·53	
Oxygen	27·77	
		100·00

This gives—

			Calculated.
4 atoms	Carbon	305·74	42·46
3 ...	Hydrogen . . .	37·44	5·19
1 ...	Nitrogen . . .	177·04	24·59
2 ...	Oxygen	200·00	27·76
		720·22	100·00

MALATES AT HIGH TEMPERATURES.

If malates of the earths or alkalies are kept for some time at a temperature varying from 250° to 300° C., they are changed into fumarates, water being the only other product. The changes produced are best observed in the following manner. The fumarate produced is dissolved in as small a quantity of boiling water as possible, and a small excess of nitric acid is added to it. The fumaric acid crystallizes from the solution in its peculiar form, possessing all the properties ascribed to it by Pelouze. I have prepared its silver salt to identify it with certainty. 0·2726 acid gave 0·0885 water, and 0·4115 carbonic acid. This gives the following formula for its composition:—

			Calculated.	Found.
4 atoms	Carbon. . .	305·74	41·84	41·73
2 ...	Hydrogen	24·95	3·41	3·60
4 ...	Oxygen . .	400·00	54·75	54·683
		730·69		

0·3735 of the silver salt gave 0·3205 chloride of silver, or 69·422 oxide of silver; and 0·4270 gave 0·029 water, and 0·224 carbonic acid.

		Calculated.	Found.
4 atoms Carbon	305·74	14·77	14·50
1 atom Hydrogen . . .	12·48	0·60	0·75
3 atoms Oxygen	300·00	14·49	15·31
1 atom Oxide of silver	1451·6	70·14	69·42
	2069·82		

This remarkable change of malic acid salts into those of fumaric acid, appears to me to bear a strong analogy to the formation of the pyro- and metaphosphates, but this is as yet not sufficiently proved by experiment. I have kept a saturated solution of fumaric acid at a boiling temperature for several days without the slightest change in it. And I have also kept a like solution, in a tube hermetically sealed, for a considerable time at a temperature of 250°, under a pressure therefore of nearly 15 atmospheres, without its being altered in any of its properties. Hence fumaric acid does not appear to be reconvertible into malic acid.

XLVI. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 175.]

Jan. 20, 1841. A PAPER was first read, "On the Teeth of Species of the Genus *Labyrinthodon* (*Mastodonsaurus Salamandroides*, and *Phytosaurus* (?) of Jäger) from the German Keuper and the Sandstone of Warwick and Leamington," by Richard Owen, Esq., F.G.S., F.R.S.

The Warwick sandstone having been considered by some geologists to be the equivalent of the Keuper[*], and by others of the Bunter Sandstein[†], and as its true position remains to be determined, Mr. Owen, in the preliminary remarks to his memoir, points out the assistance which the discovery of reptilian remains in the Warwick sandstone of the same generic characters as those of fossils obtained in the Keuper of Germany, may afford in determining the question.

Before he proceeds to describe the fossils forming the immediate object of his paper, Mr. Owen shows that the genus *Phytosaurus* was established on the casts of the sockets of the teeth of *Mastodonsaurus*; and that the latter generic appellation ought not to be retained, because it recalls unavoidably the idea of the mammalian genus *Mastodon*, or else a mammilloid form of the tooth, whereas all the teeth of the genus so designated are originally and, for the greater number, permanently of a cuspidate and not of a mammilloid form; and because the second element of the word, *saurus*, indicates a false affinity, the remains belonging, not to the Saurian, but to the Batrachian order of Reptiles. For these reasons, and believing that he has discovered the true and peculiarly distinctive dental characters of the fossil, he proposes to designate the genus by the term *Labyrinthodon*.

The only portions of the Batrachian found in the Keuper of Germany, which have hitherto been described, consist of teeth, a fragment of the skull, and a few broken vertebræ; and in the Warwick sandstone of teeth only. In this memoir, therefore, Mr. Owen confines his attention to a comparison of the dental structure of the Continental and English remains. The teeth of the *Labyrinthodon Jaegeri* (*Mastodonsaurus Jaegeri*, Meyer) of the Keuper are of a simple, conical form, with numerous fine longitudinal striations; and the teeth transmitted to Mr. Owen from the Warwick sandstone by Dr. Lloyd, bear a very close resemblance to them. Their external characters not being sufficient to establish either specific or generic identity, Mr. Owen had sections prepared for microscopic examination of portions of teeth of the *Labyrinthodon Jaegeri* forwarded to him by Prof. Jäger, and of the English reptile; and though, from his previous examination of the intimate texture of the teeth of the Plesiosaur, Megalosaur, as well as of the Crocodile, Monitor, and most recent Lacertians, he did not hope to detect such modifications of structure as would obviously mark specific or even generic identity, yet the slices exhibited such decided characters, and those of the German fossils agreed so intimately with the sections obtained from the Warwick specimen, that Mr. Owen was enabled not merely to separate these fossils from all known reptilian animals, but to establish a generic community of character in the Keuper and sandstone remains. It was not, however, until he had caused sections to be made in various directions, and had studied them attentively in comparison with the teeth of true Saurians, Batrachians, and other animals, that he was enabled to comprehend the principle of the singular cerebriform convolutions which pervade the dental structure of this remarkable reptile. The base of the tooth of the *Ichthyosaurus* approaches most nearly in character to the peculiarities of nearly the entire tooth of the *Labyrinthodon*. It is impossible to convey clearly without illustrations the structure alluded to. It may, however, be stated, that in the fang of the tooth of the *Ichthyosaurus* vertical folds of the external layer of cement (the enamel ceasing at the base of the crown) are inflected inwards, at pretty regular distances around the circumference of the tooth, towards the centre to a distance about equal the breadth of the interspaces of the inflected folds; the interspaces being occupied by corresponding processes of the dentine, which radiate from the central mass of that substance. The thickness of this interblended cement and dentine, surrounding the pulp-cavity, is about one-eighth of the diameter of the tooth.

---

[*] See Proceedings, vol. ii. p. 453. [or Phil. Mag., Third Series, vol. xi. p. 106.]

[†] Ibid. vol. ii. p. 565. [or Phil. Mag., Third Series, vol. xi. p. 320.]

The plan and principle of the structure of the tooth of the *Labyrinthodon* are the same as those of the tooth of the *Ichthyosaurus*, but they are carried out to the highest degree of complication. The converging vertical folds of the external cement are continued close to the centre of the tooth, and, instead of being straight, simple lamellæ, they present a series of irregular folds, increasing in complexity as they proceed inwards, and resembling the labyrinthic anfractuosities of the surface of the brain; each converging fold is slightly dilated at its termination close to the pulp-cavity. The ordinary laws of dental structure are, however, strictly adhered to, and every space intercepted by a convolution of the folds of the cement is occupied by corresponding processes of the dentine. These characters were presented by a transverse section of a fragment of a tooth of the *Labyrinthodon Jaegeri* from the German Keuper, which included about the middle third part of a tooth, and Mr. Owen considers that the entire length of the tooth might be 3½ inches, and the breadth at the basis 1½ inch.

The external longitudinal grooves, which correspond to the inflected folds of the cement, extend upwards from the base of the tooth to about three-fourths of its height, decreasing in number as the tooth diminishes in thickness, and disappearing about half an inch from the summit of the tooth. Each fold of cement penetrates less deeply as the groove approaches its termination; and Mr. Owen conceives that the structure of the upper part of the tooth may be more simple than that of the lower, but he has not yet been able to extend his investigations to it.

The dentine consists of a slender, central, conical column or "modiolus," hollow for a certain distance from its base, and radiating outwards from its circumference a series of vertical plates, which divide into two, once or twice, before they terminate at the periphery of the tooth. Each of these diverging and dichotomizing vertical plates gives off throughout its course narrower vertical plates, which stand at nearly right angles to the main plate, in relation to which they are generally opposite, but sometimes alternate. Many of the secondary plates, which are given off near the centre of the tooth, also divide into two before they terminate. They partake of all the undulations which characterize the inflected folds of the cement.

The central pulp-cavity is reduced to a line, about the upper third of the tooth; but fissures radiate from it, corresponding in number with the radiating plates of the dentine. One of these fissures is continued along the middle of each plate, dividing where it divides, and penetrating each bifurcation and process; the main fissures extend to within a line or half a line of the periphery of the tooth; the terminations of these, as well as the fissures of the lateral processes, suddenly dilating into subcircular, oval, or pyriform spaces. All these spaces constitute centres of radiation of the fine calcigerous tubes, which, with their uniting clear substance, constitute the dentine. The number of these calcigerous tubes, which are the centres of minor ramifications, defies all calculations. Their diameter is the $\frac{1}{7000}$th of a line, with interspaces equal to seven diameters of their cavities.

Mr. Owen then compares the structure of the section of a tooth procured in the sandstone of Coton-End Quarry, and lent to him by Dr. Lloyd of Leamington. The tooth nearly resembles in size and form the smaller teeth of *Labyrinthodon* figured by Prof. Jäger. All the peculiarities of the labyrinthic structure of the Keuper tooth are so clearly preserved in this specimen, that the differences are merely of a specific nature.

At the upper part of the tooth a thin layer of enamel*, besides a coating of cement, is inflected at each groove towards the centre of the dentine; but about the middle of the tooth the enamel disappears, and the convolutions consist of interblended layers of cement and dentine. Thus, on the supposition that the tooth of the *Labyrinthodon* of the German Keuper be capped with enamel, its extent must be less than in the tooth of the Warwick sandstone.

The inflected folds are continued for a greater relative distance before the lateral inflections commence than in the German species, and the anfractuosities are fewer in number, and some of the folds are reflected backwards from near the central pulp-cavity for a short distance before they terminate.

The modifications of the complex diverging plates of the dentine hardly exceed those of a specific character, and the dentine itself is composed of calcigerous tubes of the same relative size and disposition as in the *Labyrinthodon Jaegeri.*

In a section taken from the middle of a smaller and relatively broader and shorter conical tooth from the Warwick sandstone, Mr. Owen found that the anfractuosities were more complicated, with numerous secondary and tertiary foldings, and the external layer of cement was relatively thicker than in the *Lab. Jaegeri.*

The generic identity of the Reptiles, indicated by the teeth from the Warwick sandstones, with the *Mastodonsaurus* of the German Keuper, Mr. Owen believes to be fully established by the concordance of their peculiar dental structure above described. And in conclusion, he says, if, on the one hand, geology has in this instance really derived any essential aid from minute anatomy, on the other hand, in no instance has the comparative anatomist been more indebted to geology than for the fossils which have revealed the most singular and complicated modification of dental structure hitherto known; and of which not the slightest conception could have been gained from an investigation, however close and extensive, of the teeth of existing animals.

A paper was next read, entitled "Observations relative to the Elevation of Land on the shores of Waterford Haven during the Human Period, and on the Geological Structure of the District," by Thomas Austin, Esq.

The shore on the west side of Waterford Haven, from the rock of Passage to Woodstown, a distance of three miles, presents an almost uninterrupted cliff of clay and gravel, composed chiefly, if not wholly, of detritus of old red sandstone, and enclosing a bed from one to four feet thick of *Cardium edule*, with other marine testacea of existing species, and a few land-shells. This bed of shells is not confined exclusively to the coast, but it extends inwards to the distance of eight miles, distinct traces of it occurring between Waterford and Tramore, and at several intermediate points. In the alluvial valley of Woodstown, close to Newtown Head, the shells rest on an ancient peat bed, raised but a few inches above the sea-level. On the eastern side of Waterford Haven beds of similar shells occur at the same level; also in the cliff north of Bluff Head, at the height of eight feet. The greatest elevation at which the shelly beds have been observed by Mr. Austin in the county of Waterford, is forty feet.

---

* Mr. Owen has subsequently ascertained that this is not true enamel, but a layer of firm dentine, separated from the rest by a thin stratum of fine calcigerous cells.

Immediately north of Newtown Head, at the point where a gradual rise takes place in the cliff, the greater part of a human skeleton was found resting on its back, five feet three inches below the surface, and about the same distance above high-water level, in the centre of the shelly bed. The *Cardium edule* was as numerous in and around the skeleton as in other portions of the bed, many of them being lodged in the cavity of the skull. Mr. Austin carefully examined the conditions under which the skeleton was found, and he is convinced that the ground had never been disturbed for sepulture, the continuity of the shelly bed being unbroken where the skeleton occurred, and no specimens of the *Cardium edule* being dispersed at random through the incumbent loam. He is therefore of opinion, that the body was washed into the estuary during the period when the shelly bed was accumulated; that it was arrested at the point where it has been found by the rise in the level of the bed; and that consequently an elevation of the country has taken place since the commencement of the human period.

From an extended examination Mr. Austin is convinced, that the estuary now limited to Waterford Haven formerly covered a much larger area, as proved, in part, by the patches of shells noticed above; and that the change of relative level has been slow and uniform, producing no local disturbances; and he is further of opinion, that the operation may be still in progress.

Mr. Austin then gives a general description of the geological structure of the two shores of Waterford Haven and of the adjacent districts. The formations consist of mountain-limestone, old red sandstone, schistose strata, considered to be of the age of the Silurian system on account of the fossils found near Duncannon Fort and Newtown Head, and trap rocks.

The mountain-limestone constitutes Hook Point, the southernmost headland of the Wexford side of the Haven. It is succeeded to the north, conformably, by a red or yellow sandstone, containing obscure vegetable remains, also thin seams and nodules of anthracite, likewise some small masses of black copper ore. These beds are assigned by the author to the upper part of the old red sandstone. They are succeeded in regular descending order by various marls, sandstones and conglomerates, composing the mass of the formation, and estimated to be 1600 feet thick. A series of contorted and tilted slaty beds are then presented; but at Broom Hill the conglomerates of the old red sandstone reappear with the same dip towards the south. Immediately north of this promontory the slates recommence, and are displayed in unconformable juxtaposition with the old red sandstone, the latter dipping southwards, and the former at a higher angle northwards. From Broom Head to Arthurstown the slates constitute the whole line of coast, except at Duncannon Fort. The strata are, for the greater part, variously contorted; but near Arthurstown they dip 70° to the north, and are overlaid by beds of old red sandstone, which also dip to the north, but at an angle of only 30°. At Duncannon Fort an impure limestone occurs, containing Trilobites, corals and testacea, and considered by Mr. Austin to be analogous to species found in the Silurian system.

On the opposite or Waterford side of the Haven the old red sandstone occurs at Creden Hill and Knockavelish Head, eminences corresponding to Broom Hill; a small patch of it is displayed a little to the northwards, inserted unconformably in the slate series; and it forms the rock of Passage, a prolongation of the old red sandstone near Arthurstown. Between Knockavelish Head and Passage the slate series prevails, except near Newtown Head, where trap-rocks are exposed. A little to the north of that headland are some highly inclined fossiliferous strata, corresponding in position to the beds near Duncannon Fort on the opposite side of the Haven; they are visible only at ebb-tide.

The trap-rocks constitute the point on which stands Duncannon Fort; Newtown Head is also formed of trap; and Mr. Austin is of opinion that the same mass strikes westwards to Tramore and thence to Great Newtown Head, where it is lost in the St. George's Channel. Along this line, wherever the trap comes to the surface, the slates are tilted.

With respect to the numerous contortions exhibited in the schistose rocks, Mr. Austin ascribes their existence to lateral pressure, which he says must have been excessive; and he is of opinion that a considerable portion of the upper part of these contorted beds has been removed by denudation.

A paper by C. Lyell, Esq., F.G.S., was afterwards read, "On the Freshwater Fossil Fishes of Mundesley, as determined by M. Agassiz."

In a memoir on the boulder formation and associated freshwater deposits of Eastern Norfolk*, Mr. Lyell stated, on the authority of Mr. Yarrell and the Rev. L. Jenyns, that the scales and teeth of fishes which had been then procured in the fluviatile beds of Mundesley belonged to the *Esox lucius*, to a trout or an undeterminable species of *Salmo*, to a carp, probably the *Cyprinus carpio*, and to a distinct species of *Perca*.

This collection, with some additions recently sent to the author by Mr. Wigham, was examined by M. Agassiz during his late visit to England. The decision of Mr. Jenyns with respect to the distinctness of the perch, M. Agassiz fully confirmed; but he was of opinion that the pike differs from the *Esox lucius*, and that the supposed carp is a species of *Leuciscus*; and that the trout is not truly a trout, although one of the same great family.

From this examination, therefore, Mr. Lyell says it is apparent that these remains belong to species not identical with any European freshwater fishes hitherto described; but that they nevertheless belong to an ichthyological fauna, more modern and more nearly resembling the recent than any other with which M. Agassiz is acquainted in a fossil state.

---

* See Proceedings, *antè*, p. 171. [The paper here referred to appeared in Phil. Mag., Third Series, vol. xvi. p. 345.]

Similar remains have been found by Mr. Lyell at Runton, near Cromer, but both there and at Mundesley the associated testacea all belong to living freshwater species; even the *Paludina minuta* (Strickland), which Mr. Morris has pointed out to the author to be identical with the *P. marginata* of Michaud, a living French species. It is a question therefore, the author states, whether these unknown fishes may not still inhabit the rivers and lakes of the more northern parts of Europe or America, especially as M. Agassiz is at present unacquainted with the freshwater fishes of Norway, Sweden, Spitzbergen, Iceland, Greenland, Labrador and Canada, and even of the northernmost parts of Scotland and the Shetland Islands; and in conclusion Mr. Lyell says, it seems natural to look northward for types analogous to the Mundesley fishes, because the beds in which they occur were deposited contemporaneously with the drift accumulated by the agency of floating ice.

Feb. 3.—A paper was read, "On the Geological Structure of the Wealden District, and of the Bas Boulonnais," by William Hopkins, Esq., F.G.S.

This paper is divided into two parts. In the first the author describes the phænomena of elevation presented in the two districts comprised respectively within the boundary of the great Chalk escarpment of the south-eastern part of England, and an exactly similar escarpment forming the inland boundary of the Bas Boulonnais. The former is well known as extending from the coast at Folkstone, by Seven Oaks, Godstone, Farnham, Petersfield, &c., round to the coast again at Beachy Head. On the opposite side of the channel, the escarpment, commencing at Wisant on the north, forms nearly a semicircle, of which Boulogne is not far from the centre. If we conceive the northern Weald escarpment continued from Folkstone to Wisant, and the southern one from Beachy Head to the southern extremity of the Bas Boulonnais, it will be seen that the whole tract comprised within the Chalk would be a regular oval, except that its axis instead of being straight is *curved*, so as to incline towards the S.E. in its eastern portion. These two districts are thus connected by relative position not less than by a community of geological character.

In the second part of his paper the author compares the laws of the existing phænomena in these districts with the results given by his 'Theory of Elevation,' published in the Transactions of the Cambridge Philosophical Society (Vol. VI. Part I.).*

I. The lines of elevation in the Wealden district are partly marked by an anticlinal arrangement of the beds, and partly by strong flexures, forming *one-sided saddles*. The latter have been termed by the

---

[* Mr. Hopkins gave a view of this subject in Phil. Mag., Third Series, vol. viii. p. 227.—Edit.]

author *lines of flexure*. The central portion of the district is first described. The following lines of elevation are found in it.

1. *Hastings Line*.—This line runs to the north-east of Hastings towards Battle. It has been mentioned by Dr. Fitton and other geologists. The author had not had time to examine it himself.

2. *Brightling Line*.—This is strongly anticlinal, and runs along the high ridge of Brightling Down as far as Heathfield Park, where its distinct features are lost. The author has not ascertained whether it is a continuation of the Hastings line.

3. *Wadhurst Line*.—This line runs by Wadhurst and Hawkshurst, to the south-west of which place it is lost. It also ranges westerly along the ridge between Wadhurst and Mark Cross.

4. *Crowborough Line*.—Crowborough Beacon stands on what must be regarded as the great central ridge of the district. The anticlinal line runs near the Beacon and is continued westerly to the north of Balcombe. No traces of it however are distinguishable beyond Horsham.

5. *Cuckfield Line*.—This line extends parallel to that last described, and immediately to the north of Cuckfield. It is not to be traced far to the west of that place. To the east it is continued across the Brighton railway, where it was very distinctly exhibited in the new cuttings.

6. *Frant Line*.—At Lamberhurst this line is distinctly marked. It proceeds westward along Frant Hill, where its evidence, however, is not very distinct. It appears to be lost entirely not far to the west of Frant.

7. *Bidborough and Brenchley Line*.—Bidborough Hill is formed by a strong flexure of the beds by which the Hastings sand is brought up from beneath the Weald clay. Brenchley Hill is formed in the same manner, but presents a more distinct anticlinal arrangement. These hills are separated by a wide transverse valley of denudation, but there can be little doubt, it is conceived, that they belong to the same line of elevation. The dislocation is also continued westward, but with less distinctness, across the Medway.

All these lines preserve a remarkable parallelism with each other and with the *curved central axis* of the district.

The author also describes several transverse valleys of the central portion of the district, and states the evidence on which he believes them to have originated in transverse dislocations.

8. *Greenhurst Line*.—This line has been described by Mr. Martin, by whom it was first detected. It is distinctly marked from a point south-west of Pulborough, whence it runs not far from and parallel to the chalk escarpment, till it strikes into the chalk at Piecomb. Its continuation westward is not very distinct, but eastward it is strongly defined at Lewes. Several remarkable transverse valleys across the greensand ridge are also found in the south-western part of the district, and present evidence of having originated in transverse dislocations. Their directions are as nearly as possible perpendicular to that of the Greenhurst line.

9. *Line from Farnham to Seven Oaks*.—This line runs parallel to the chalk escarpment of the North Downs and near to it. It is a

line of flexure, with a great dip to the north, but without the corresponding dip to the south necessary to form an anticlinal arrangement, except in one or two localities. Towards the west it runs immediately at the foot of the Hog's Back with a dip, which, near its western extremity, amounts to 70 or 80 degrees. Near Guildford it passes by the foot of the hill on which Margaret's chapel stands. To the east it passes south of Dorking and Reigate to the summit of Tilburstow Hill. It is afterwards continued by Limpsfield to the east of Seven Oaks, as formerly described by Dr. Fitton. At some points between these last-mentioned places, the line assumes a distinct anticlinal character.

Transverse valleys exist in the greensand ridge of this part of the district as well as in that on the southern side. The author also alludes to what he conceives to be incipient valleys of this description, and states his reasons for believing them to be indications of transverse fractures. He conceives this opinion to be strongly corroborated by the existence of the perennial springs by which these valleys are characterized. Several are pointed out, especially in Leith Hill and the Seven Oaks ridge overlooking the valley of the Weald.

Transverse river-courses through the Chalk escarpment form one of the most striking features in the geology of this district. The analogy which they bear to the transverse valleys across the greensand ridges would seem to leave no doubt of their being referable to the same physical cause; and as there are in many instances direct evidence which renders the origin of these latter valleys in transverse fractures highly probable, the same conclusion appears almost equally probable with respect to the river-courses through the Chalk. In the evidence of dislocation which the Chalk itself affords, there is nothing, however, very conclusive; but it must be remembered, that the evidence of faults is always difficult to detect in a massive formation like the Chalk, possessing not more than two general divisions which admit of distinct identification.

The central chalk ridge of the Isle of Wight is traversed in like manner by three transverse valleys, two of which are river-courses. The author has pointed out some direct evidence in support of the conclusion, that the central one (that of the Medina) has originated in transverse dislocation.

Bas Boulonnais.—With respect to the structure of the Bas Boulonnais, it is only necessary here to state, that the author has recognised three parallel lines of dislocation commencing at the coast and running in a direction coinciding with that of the lines of elevation of the Weald, supposing them produced across the Channel according to the law which they follow on this side of it. The southernmost of these lines passes immediately to the north of Boulogne.

II. In the second part of this paper, previously to his comparing the observed phænomena with theoretical deductions, the author recapitulates some fundamental points of his theory. It is assumed, that an elevatory force has acted simultaneously at every point of the lower surface of the elevated mass in each district throughout which the phænomena of elevation are observed to follow the same law, This force is not supposed to have been necessarily of uniform intensity throughout. If it has been greater in one portion of the district than in the rest of it, a corresponding modification will be produced in the directions of the lines of elevation, or a deviation from those positions in which they would have existed had the intensity of the force been uniform throughout. If the force has been uniform, the directions of the lines of dislocation and elevation will depend on the *form* of the boundary of the surface of the elevated area. If this be given, these directions must be investigated on mechanical principles; and if the force be supposed to have acted with greater intensity in any assigned portion of the district, the corresponding modification in the directions of the lines must be determined. This has been done by the author in some particular cases in the memoir above referred to in the Transactions of the Philosophical Society of Cambridge.

Any irregularity in the cohesive power of the elevated mass will have but little effect on the general directions of the lines of elevation; but if there be any considerable continuous portion of the district throughout which the elevated crust is *thinner*, and therefore lighter and weaker, the effect will manifestly be the same as if the crust had been of uniform thickness throughout, and had been acted on in this particular portion with a force of greater intensity. Consequently the modifications in the lines of elevation will be the same, whether they arise from a weaker crust or a greater intensity of force.

In the application of this theory, the boundary of the area under which the elevatory force has simultaneously acted must be determined as nearly as may be by the actual boundary of the disturbed district, throughout which we recognize a character of continuity in the phænomena of elevation. That portion of the district also in which the force may appear to have acted with greater intensity must be determined by the existing indications of greater elevation. Thus it is conceived, that a simultaneous effort of the elevatory force was made throughout the whole tract extending from the Bas Boulonnais on the east, beyond the Wiltshire Chalk on the west, and from the Vale of Pewsey and the valley of the Thames on the north, beyond the southern coast of this country on the south. The Wealden district, with the Bas Boulonnais, presents us also with a case, in which it is presumed, from its greater elevation, either that the disturbing force acted there with greater intensity, or that the elevated crust was there thinner, than in the other part generally of the district. Assuming such to have been the case, the author points out what would be the general directions of the lines of elevation throughout the Wealden and the Bas Boulonnais, and comparing them with the lines described in the first part of his paper, he shows the remarkable accordance which exists between the results of observation and of theory; an accordance which he considers as strongly confirmatory of his theory as applied to this district.

Hence the author concludes, that the fissures or dislocations in which he conceives all the observed lines of elevations (whether faults, anticlinal lines, or lines of flexure,) to have originated, must have been produced by one simultaneous and momentary effort of the elevatory force. It is not, however, to be regarded as a necessary consequence of this conclusion, that the whole elevation of the district was thus produced at once; it might be in some degree produced by previous, and in a considerable degree by subsequent movements; but it would seem at least highly probable, that that general movement which produced the dislocations of the elevated mass, and impressed upon it its present distinctive characters, should have been the most energetic of those repeated movements to which the whole elevation has probably been due.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 245.]

Dec. 11, 1840. (*Continued.*)—Description of a Method of dividing one Circle, B, by copying from another, A, previously divided. By Lieut.-Col. Everest, Director of the Trigonometrical Survey of India.

In Col. Everest's 'Account of the Measurement of an Arc of the Meridian in India,' published in 1830, it was stated that, with a view to avoid the effects of the errors of catalogues and the periodical irregularities to which all stars are more or less liable, it had been resolved to substitute two astronomical instruments, each with a vertical circle of three feet and an azimuth circle of two feet diameter, for the zenith-sector of Ramsden, formerly used in determining the amplitudes by his predecessor and himself; the intention being, that all celestial amplitudes should be determined by observations taken simultaneously at both extremities of the arc. Accordingly, two new altitude and azimuth instruments were ordered in London and forwarded to India, where they arrived in 1832; but an opportunity of giving them a fair trial did not occur until 1837, when a small observatory was constructed at Kaliana, the northern limit of one of the sections of the great arc of the meridian. The instruments proved, on trial, to be of too feeble construction for the purpose for which they were intended, and Colonel Everest found that, in order to render them serviceable, it would be necessary to construct for each a pair of stronger columns, new outriggers and friction-rollers, a new axis for the azimuth motion, a new table for supporting the columns, and a new azimuth circle; in short, to construct two entirely new instruments, with the exception of the vertical circles. To any other than a professed artist, an operation of this kind, even under the most favourable circumstances, would prove an arduous and formidable undertaking; but, in a half-barbarous country like Upper India, where no artificer was to be found who had ever tried his hand at dividing, or even seen a cutting-tool, it could not be otherwise than extremely embarrassing. Before Col. Everest undertook the alterations, he had obtained, indeed, the assistance of a mathematical instrument-maker in the service of the Company, at Calcutta; but, owing to some disagreement, this person left him before the most difficult part of the operation, that of dividing the new circles, was ready to be commenced, and he was thereby thrown entirely upon his own resources.

Colonel Everest resolved to construct the new tables and azimuth circles of cast-iron, which, though objectionable on account of its liability to rust, has the advantage of being lighter and less yielding than brass. The circles were inlaid with gold to receive the graduations; and at every tenth degree a circular disc of silver was let in, so that the large figures might stand prominent. Each was supplied with a strong edge-bar below, which put all yielding out of the question, and moreover was of great service in the process of graduation, by affording a support on which to affix the reading microscopes. With respect to the mode of dividing the circles, Col. Everest states that, being aware of the dilatoriness and difficulty of any method he had seen detailed, he devised a plan of his own, whereby the divisions of the original circles should be copied on the new. Without the aid of drawings, it is scarcely possible to impart an idea of the mechanical means which were employed for carrying this plan into effect. The two circles, the new one which was to be graduated, and the old one from which the graduation was to be copied, were placed the one directly over the other, and strongly secured by screws; and the principle of the method consists in keeping the circle to be graduated, which is placed below, steadily fixed on a substantial pillar of masonry, and making the circle to be copied from revolve above it, carrying with it the cutting-tool and clamp: and the apparatus being thus arranged, it is obviously only necessary to apply sufficient means of reading off the angular spaces passed over by the latter in its rotation, in order to transfer its graduation to the former. The mode of operation was assimilated to that which is followed in taking angles. A skilful person was appointed to each of the four microscopes; the different readings were made precisely equal to the arcs to be intercepted; and when the one mean reading subtracted from the other showed this value, the order was given to cut.

The author describes in minute detail the different parts of the operation, which appears to have been executed successfully, and he concludes his paper with an investigation of the mathematical relations of the method, and particularly the effect of eccentricity of the upper circle, and of any error in fixing the cutting-tool, whereby its point would be made to move obliquely to the radius.

Transits observed at Washington (United States), from January 1 to July 1, 1840; and Occultations observed at the same place, since June 1839. By J. Melville Gillies, Esq.

The transit is one of the 6-feet instruments made by Troughton for Mr. Hassler, in 1815, and mounted on substantial granite pillars. The usual methods were adopted for determining its errors of level and collimation, and the observations are free from all such. The deviation in azimuth was determined from the observed and true differences of right ascension of high and low stars, and registered in its proper column; but the proportional part due to each observation was not in any case applied. The observations were registered by a chronometer regulated to sidereal time, and its rate determined by a mean of successive transits of the same star. The true sidereal time and right ascensions were taken from the list of moon-culminating stars, the list of 100 stars, and the list of stars liable to occultation given in the Nautical Almanac; and the remainder computed by means of the Royal Astronomical Society's constants. The moon's right ascension was determined by applying the mean error of the chronometer to the observed right ascension.

Places of Bremicker's Comet, as determined with the Equatoreal Telescope at Mr. Bishop's Observatory. By the Rev. W. R. Dawes.

Date.	Greenwich Mean Time.	Right Ascension.	N.P.D.	Declination.	Remarks.
1840.	h m s	h m s	° ′ ″	° ′ ″	
Nov. 14.	9 4 19	......	30 39 6	= +59 20 24	Rather doubtful.
	9 29 9	20 31 13	......	......	Ditto.
16.	9 55 53	20 46 37	31 13 11	= +58 46 49	Tolerably good.
19.	8 38 13	21 9 18	32 15 3	= +57 44 57	Good.
	9 37 59	21 9 33	32 16 11	= +57 43 49	Ditto.
	9 50 2	21 9 39	32 16 39	= +57 43 21	Ditto.
21.	9 9 39	21 24 55	33 7 2	= +56 52 58	Very good.
24.	8 51 1	21 48 4	34 37 47	= +55 22 13	Excellent.
26.	6 54 40	22 2 43	35 45 10	= +54 14 50	Very good.
27.	5 43 15	22 10 3	36 21 10	= +53 38 50	Comet excessively faint, seen through smoke and fog. Doubtful observation *on meridian*.
Dec. 2.	7 31 58	22 46 39	40 0 11	= +49 59 49	Very good.
3.	6 33 59	22 53 14	40 46 8	= +49 13 52	Excellent.

LONDON ELECTRICAL SOCIETY.

Sept. 21, 1841.—The papers read at this meeting were,—1st, A letter from Mr. T. Pine to the Secretary, "On the Ripening or Maturing, and Decline of Vegetation." Third communication.

2nd. Notice of "The Effects of a Lightning Flash." By Mr. William Trackelton.

3rd. Notice of "A Test for Nitric Acid in the Sulphuric Acid employed in exciting Voltaic Batteries." By the Hon. Secretary.

Dissolve some sulphate of indigo in distilled water, and add some of the solution to the acid; if, on the application of heat for a few minutes, the blue colour disappears, nitric acid is present. This test was shown to the Secretary by Dr. Leeson, in the laboratory of St. Thomas's Hospital, in order that it might be communicated to the Society for the benefit of those who find their zinc consumed, and are not aware of this as one of the causes.

4th. Notice "On the Relative Powers of certain Diaphragm Voltaic Combinations," and on "A new Form for the negative Element of Voltaic Arrangements." By the Hon. Secretary.

The table of galvanometric deflections is suspended in the laboratory of the London Institution. Professor Grove furnished Mr. Walker with a copy for the Society; it contains the deflections for twenty-four arrangements. The negative element, also a suggestion of Mr. Grove's, is platinized silver gauze for Smee's battery; he has found the advantage from using cuprized copper, and has only delayed adopting the silver from the difficulty of obtaining such an article. The Secretary recommended platinized, plated-copper gauze.

5th. "On certain Phænomena connected with the Spark from a secondary Coil." By J. P. Gassiot, Esq., F.R.S., M.E.S., &c.

The wires of the coil were placed side by side, sometimes parallel, at other times at a considerable angle. In all cases the spark appeared at the cathode. With a coil containing twelve miles of wire, the secondary spark, in the flame of a spirit-lamp, was three-fourths of an inch long.

6th. "An Account of a Method of Electro-gilding and Electro-plating at the expense of a Gold or a Silver Anode." By Charles V. Walker, Esq., Hon. Sec.

This object is usually effected at the expense of a gold or silver solution, forming one of the exciting liquids in a single cell; and its strength is maintained by adding from time to time fresh supplies of the salt. It is obviously not the wisest principle to employ the salt if the metal itself can be used; but as the former plan has been so generally adopted, Mr. Walker suspected some physical difficulty might exist in the other mode. To satisfy himself, he went through a series of experiments, the results of which were, that from cyanide of silver, when electrolysed with a silver anode, an anion will be released that will combine with the anode, and that the like result attends the electrolysis of the cyanide of gold. The author employed a silver plate, which was considerably consumed; and a gold wire, which was also much acted upon. He recommends this as a surer, a more simple, and a much more œconomical method; and conceives that by this means the expense of each operation is reduced to a minimum. The communication extended to some length, and contained a description of a "Regulating Apparatus," to be employed when rich deposits are required, and instructions as to the management and preparation of the apparatus, &c.

Mr. Weekes's Monthly Register was then placed on the table, and the Society adjourned.

CHEMICAL SOCIETY OF LONDON.

[Continued from vol. xviii. p. 520.]

May 11, 1841.—"On a Simple and Cheap Method of preparing Hydrochloric Acid absolutely pure, and of any required strength," by Wm. Gregory, M.D. Professor, &c., King's College, Aberdeen.

Much difficulty is experienced in procuring pure and concentrated hydrochloric acid for chemical purposes, the common commercial acid containing various impurities, particularly sulphuric and sulphurous acids, free chlorine, chloride of iron and sulphate of soda; these arise from impurities contained in the materials employed; the chlorine from the action of nitric or nitrous acid (often present in oil of vitriol) on the hydrochloric acid; sulphurous acid from organic particles in the common salt employed; and chloride of iron from the presence of that metal also in the salt. Pure and clean materials are therefore the first requisite for a pure acid. Dr. Gregory finds, that if to one equivalent of salt two equivalents of sulphuric acid diluted with a certain quantity of water be used, instead of one equivalent as usually prescribed, the whole of the hydrochloric acid may be expelled without a trace of sulphuric acid passing over even into the first condensing bottle, and that two-thirds of the hydrochloric acid distil over before water is volatilized; on this observation the following process is founded.

Into a common Florence flask are introduced 4 ounces of the purest patent salt, and 5 fluid ounces of sulphuric acid of specific gravity 1·600; a gentle heat is applied, and the gas which is then generated, is conducted by a bent glass tube into a four-ounce phial containing 2 ounces of distilled water surrounded with snow or ice-cold water. No safety tube is required, as the tube is made to dip only about one-eighth of an inch into the water, so that should any absorption take place the rise of a little water in the tube exposes the extremity of it, so as to admit the air; or, for greater security, a small bulb may be blown on the descending limb of the tube. The gas is absorbed as fast as it comes over, and for the first hour and one quarter the heat hardly requires to be increased; if the temperature of the surrounding water has been kept so low as 50°, the 2 ounces of distilled water will have increased in volume to 3 ounces of colourless hydrochloric acid, fuming strongly, and having a specific gravity of 1·20 to 1·21, the gas passing over so dry that no part of the tube becomes warm. This portion being removed, its place is supplied by 2 ounces more of distilled water, and the heat gradually increased and continued for an hour longer; by that time all the hydrochloric acid is expelled, with some water, and the 2 ounces of water have become 3 ounces of hydrochloric acid of specific gravity 1·10. Both portions are absolutely pure. If 3 ounces of water are used in the first instance, 4·5 fluid ounces of acid of specific gravity 1·165 are obtained; and then replacing the acid by 2 ounces of water, 3·5 ounces more of specific gravity 1·065. If 5 ounces of water are used at once for condensing the acid and kept till the distillation is complete, 7·5 fluid ounces of specific gravity 1·155 are obtained. Dr. Clark finds sulphuric acid of a specific gravity of 1·65 to answer still better than acid of 1·60.

Dr. Clark then exhibited to the Society his method of ascertaining, quantitatively, the comparative hardness of water by means of the common test of tincture of soap, illustrated by experimental evidence, to prove the accuracy of which it was susceptible, and the facility of its application. Dr. Clark hoped at a future meeting to lay before the Society more mature details of the method exhibited.

May 18.—An extract of a letter from Mr. Maugham was read, "On the Mode of treating Copper Ores, and the Ores of other Metals combined with Sulphur, so as to ascertain the quantity of Sulphur in such Ores, and also the quantity of Copper in the native Sulphuret."

A quantity of the powdered ore, sulphuret of copper, about 50 or 100 grains, is placed in a porcelain tube traversing a small furnace, and made red-hot; after remaining for 5 or 6 minutes a portion of the sulphur will be expelled; a stream of oxygen gas is then passed over it, the remaining sulphur is then rapidly given off as sulphurous and sulphuric acids, and the copper thoroughly oxidized. By heating the ore when first introduced into the tube it becomes slightly adherent, which prevents any of it from being blown away by the oxygen gas. The contents of the tube are then removed into an assay crucible, with the addition of black flax and a little charcoal; the whole covered with dry carbonate of soda or borax, and submitted to a yellow heat, when a button of copper is obtained. Mr. Maugham finds that arsenic and other volatile metals that may happen to be present, are oxidated and expelled by the heat; but should tin be present it will be found with the reduced copper, and must be removed in the usual way. The process is known to be complete when no more vapours are seen to issue from the tube, or when the odour of sulphurous acid is no longer perceptible. It is, however, to be observed, that white vapours will be seen even after the process is complete, owing to a portion of sulphuric acid condensed in the tube returning to the hot part. An assay of this kind takes about twenty minutes to execute.

When the wet analysis is desirable, we have only to proceed as before in the tube part of the process, and to dissolve the residue in the proper acids.

Mr. Maugham speaks favourably of the use of chlorate of potash, added to hydrochloric acid, for dissolving certain ores where nitro-muriatic acid is generally employed, and afterwards expelling the excess of chlorine by heat; the known inconveniences of nitric acid in certain cases are thus avoided.

The quantity of sulphur contained in the ore is ascertained by elongating the tube traversing the furnace, so that it may dip into a vessel containing water saturated with chlorine, by which means the sulphurous acid is converted into sulphuric acid, and the quantity of sulphur found from the precipitate with chloride of barium.

A paper was read "On the Atomic Weight of Carbon," by Professors Redtenbacher of Prague and Liebig of Giessen[*].

June 1.—Extract of a letter from Col. Yorke "On a Specimen of Artificial Arragonite."

"This substance was taken from the interior of a copper boiler which was used to supply hot water for household purposes at Port Eliot, Lord St. Germaine's seat in Cornwall. The substance is about $\frac{4}{10}$ths of an inch thick, and by its non-conducting power it caused, as I understood, the destruction of the boiler. On the surface which was next to the copper it is coated by dioxide of copper, and the mass appears made up of an aggregation of prismatic crystals, whose axes are perpendicular to the surface on which the incrustation formed: under a microscope these crystals appear to be six-sided prisms. I compared, under a polarizing microscope, portions of the powder of Iceland spar, and of arragonite from Bilin, with the powder of the specimen; the latter agreed very closely in appearance with that of arragonite.

---

[*] This paper is given entire in our last Number, p. 210.

"Among the powder of the specimen were seen some very acute double six-sided pyramids; these with little doubt are similar to those formed by G. Rose* by evaporating solutions of carbonate of lime at a boiling heat, and described by him as resembling some sapphire crystals.

"On chemical examination it was evident that the specimen consisted chiefly of carbonate of lime; water, however, dissolved from it a small quantity of sulphate of lime.

"The following is the result of an analysis made on 10 grains, but which does not pretend to minute accuracy:—

Matter insoluble in muriatic acid, silica, with oxides of iron and copper.	1·3
Sulphate of lime	1·8
Carbonate of lime	93·7
Carbonate of magnesia	3·2
	100

"Deprived of its coating of dioxide of copper, three trials were made of the specific gravity of its powder; the sulphate of lime being previously washed off with hot water.

"The two first trials were made by weighing about 80 grains of the powder in a small spherical-stoppered phial (whose contents in distilled water at 62° was previously determined), and then when filled up with water, the third trial was made in the manner described by Rose. The specific gravity being thus determined, the powder was in each case dried, and slightly ignited, (by which operation arragonite, as is known, is converted into calcareous spar) and the specific gravity again taken. The results were as follows:—

	Spec. Grav. before ignition.	Spec. Grav. after ignition.
1st trial	2·842	2·708
2nd	2·828	2·701
3rd	2·878	2·681†
Mean	2·849	2·696

The specific gravity of arragonite crystals from Bilin is 2·946.

"The highest specific gravity which Rose obtained of arragonite,

---

[* G. Rose's paper, here referred to, on the formation of calc-spar and arragonite, will be found in Phil. Mag., Third Series, vol. xii. p. 465.—Ed.]

† The loss by ignition on 43·8 grains was = ·08 grain.

formed by evaporating solutions of carbonate of lime, he states was =2·836.

"Specific gravity of Iceland spar is 2·72. I should suppose then that there can be little doubt but that the specimen affords an example of the formation of arragonite, and a verification of G. Rose's experiments.

"I have since made two attempts at producing arragonite by Rose's method of precipitation, but cannot boast of my success. The following is a note of the best experiment. A solution of 300 grains of chloride of calcium, in 4 ounces of water at 212°, was mixed rapidly with a solution of 330 grains of carbonate of ammonia in 8 ounces of water at 180°. The mixed liquor was not alkaline.

"The precipitate under the microscope consisted chiefly of radiating spicular crystals, extremely minute, with occasional rhombohedrons. The precipitate being washed, the specific gravity taken before drying came out = 2·751, after drying it was below 2·7. During the washing a slight crackling noise was heard, and I cannot help thinking the precipitate may have been thrown down as arragonite, but changed into calc-spar during the washing and drying."

Professor Kuhlman of Lille presented specimens of Chalk hardened by his process for the Silicification of Limestones, which consists of immersing them in a solution of silicate of potash, exposing to air for several days, and afterwards washing. Although the chalk did not contain more than three or four per cent. of silica, it was capable of scratching many cements and marbles. In a similar manner he could harden carbonate of lead and plaster of Paris. He finds alkaline salts in all the limestones containing silica, which are hydraulic, and believes that they originally resembled ordinary chalk in purity, but have been partially silicified by infiltration of water containing an alkaline silicate in solution, or by a natural process analogous to his artificial one.

Extract of a letter from Dr. R. F. Marchand of Berlin, "On the Atomic Weight of Carbon."

"I take this opportunity of communicating the results of experiments relative to the atomic weight of carbon, which Professor Erdman and myself have very lately obtained. The difference between the numbers recently given by Dumas and that of Berzelius was a sufficient inducement for us to examine and repeat Dumas's experiments, much occupied as we are with organic analysis. The burning of diamonds in oxygen gas was easily effected by us in a porcelain tube, by a pretty high temperature. The apparatus employed was very similar to that described by Dumas.

"The following are the results :—

No. 1. 0·8062 gramme diamonds left a residue weighing 0·0010 gramme, and gave 2·9467 grs. carbonic acid, consequently giving the atomic weight for carbon 75·19.

No. 2. 1·0867 gr. left a residue weighing 0·0009 gr., and gave 3·9875 grs. carbonic acid = carbon 74·84.

No. 3. 1·3575 gr. left a residue weighing 0·0018 gr., and gave 4·9659 grs. carbonic acid = carbon 75·10.

No. 4. 1·6330 gr. left a residue weighing 0·0025, and gave 5·97945 = carbon 74·98.

No. 5. 0·7510 gr. left a residue weighing 0·0010, and gave 2·7490 = carbon 75·03.

"Graphite gave the same numbers; the residues were pure white silex without a trace of oxide of iron:—

No. 1. 1·4580 gramme native graphite left a residue weighing 0·0075, and gave 5·31575 grs. = carbon 75·05 atomic weight.

No. 2. 1·5746 gr. graphite left a residue weighing 0·037, and gave 5·6377 grs. = carbon 75·02.

No. 3. 1·6578 gr. residue 0·0084, and gave 6·0385 = carbon 75·18.

No. 4. 1·9040 gr. artificial graphite, residue 0·0105 gr., gave 6·9355 grs. = carbon 75·10.

"The mean of these experiments give 75·07; we therefore consider 75 as the true number indicated by these experiments for the atomic weight of carbon. It is remarkable that this number was fixed upon theoretically by the English chemists, and has now been confirmed by experiments. If we take the number 6·239 for hydrogen, with a very small increase, viz. as 6·250, we arrive at the numbers for oxygen, carbon and hydrogen, viz. 16, 12, 2, or 8, 6, 1."

A paper was read "On Malic Acid, and the Salts of Malic Acid," by R. Hagen, M.D., translated and communicated by T. G. Tilley, Esq. This paper will be found in the present Number, p. 306.

A paper was read "On Pyroxylic Spirit," by Andrew Ure, M.D.

A paper was read "On the Ferrocyanides," by R. Corbett Campbell.

Both these papers will be inserted, entire, in a future Number of the Philosophical Magazine.

XLVII. *Intelligence and Miscellaneous Articles.*

PREPARATION OF LACTIC ACID AND LACTATES.

THE following process is recommended by MM. Boutron and E. Fremy, for preparing lactic acid and its salts:—Take about 6 pints of milk, and add to it about 8 ounces of sugar of milk dissolved in water. The mixture is to remain in an open vessel exposed to the air, for some days, at a temperature of about 60° Fahr. The liquor has then become very acid, and is to be saturated with bicarbonate of soda. In 24 to 36 hours it again becomes acid, and is to be again saturated, and so on, until all the sugar of milk is converted into lactic acid. When the conversion is reckoned complete, the milk is to be boiled to coagulate the caseum, and it is to be filtered and cautiously evaporated to the consistence of a syrup; the product of this evaporation is to be treated with alcohol, which dissolves the lactate of soda; to the alcoholic solution sufficient sulphuric acid is to be added to convert the soda into sulphate, which is precipitated, and the liquor filtered and evaporated yields lactic acid nearly pure; in order to obtain it perfectly so, it is to be saturated with chalk, and the lactate of lime formed immediately crystallizes in perfectly white mammillated masses, and from this the lactic acid is procured by the well-known processes.

It is evident, observe the authors, that lactic acid may be saturated by other bases, and they state that a chemical manufacturer has prepared this acid from the sour water of starch-makers, by saturating it with carbonate of lime.—*Journal de Pharmacie*, xxvii. 341.

PREPARATION OF FORMIC ÆTHER.

According to Wöhler, the following process yields formic æther in large quantity, and very readily: put into a retort ten parts of starch and 37 parts of peroxide of manganese, intimately mixed in very fine powder; add to them a mixture of 30 parts of sulphuric acid, 15 parts of highly rectified spirit of wine, and 15 parts of water; the mixture is to be gently boiled, and distillation continued until the product contains no æther. In order to separate the water and the alcohol, a sufficient quantity of chloride of calcium is to be dissolved in the product, and distillation is to be performed in a water-bath; the purification is to be completed by a second rectification in the same manner.

M. Wöhler states, that in the numerous experiments which M. Kolbe made on this subject at his request, appearances presented themselves which probably indicate the presence of a peculiar substance in this æther, requiring fresh experiments for elucidation: when the fragments of fused chloride of calcium have been for some time in contact with the æther which has been once rectified, and are partly dissolved, it becomes of a deepish yellow colour, and deposits small, very fine colourless crystals on the sides of the vessel. In general the colour disappears after a certain time; it is probably owing to chloride of iron; and probably also the crystals are a compound of chloride of calcium and alcohol, which are soluble in formic æther, and may crystallize from it.—*Journal de Pharmacie*, tom. xxvii. p. 91.

PREPARATION OF IODATE OF POTASH. BY M. MELLON.

When cold no action takes place between the above-named substances; but if to chlorate of potash about three or four times its weight of distilled water be added, and then heated to ebullition, iodine added to the solution disappears in considerable quantity; the liquor is colourless, and remains so until the equivalent of iodine is exceeded; after this the liquor becomes yellow and then brown; and, as a final result, there are obtained neutral iodate of potash and chloride of iodine, with greater or less excess of the latter. If the mixture be evaporated to dryness, the chloride of iodine is disengaged, and the iodate of potash remains pure.

If the action of the iodine on the chlorate be stopped before an equivalent of the iodine is added, the liquor even then contains iodate of potash, and also chlorate [chloride ?] of iodine, corresponding unquestionably to iodic acid; for if the liquor be strongly heated, chlorine is disengaged, and there remains chloride of iodine, which gives a precipitate of iodine with carbonate of potash.

According to M. Mellon, the formation of chloride of iodine explains the reaction that occurs: the iodine attracts the chlorine of the chlorate, whilst the greater affinity of the iodine for the oxygen, and the greater cohesion of the iodate, cause the iodine to enter into the chlorate instead of the chlorine. The action M. Mellon represents thus: $5\,\mathrm{ClO}^5,\mathrm{KO} + 6\mathrm{I} = 5\,\mathrm{IO}^5,\mathrm{KO} + \mathrm{ICl}^5$.

Journal de Pharmacie, tom. xxvii. p. 102.

PROFESSORSHIP OF GEOLOGY, UNIVERSITY COLLEGE, LONDON.

We have great pleasure in recording the establishment of a Chair of Geology in University College, and that Thomas Webster, Esq., formerly Secretary to the Geological Society, has been appointed to occupy it.

Professor Webster's Course on Geology at the College, consisting of Thirty Lectures, will commence on the first Tuesday in February 1842, and will be divided into three parts.

1. The description of such simple minerals as enter essentially into the composition of rocks.
2. Geology, properly so called; or the characters and superposition of the strata composing the crust of the globe, together with an account of the various phænomena exhibited by them.
3. The application of the above subjects to the useful arts.

METEOROLOGICAL OBSERVATIONS FOR AUG. 1841.

Chiswick.—August 1. Slight rain: cloudy and fine. 2. Fine with clouds: rain. 3. Hazy: cloudy and mild: rain. 4. Cloudy and fine. 5. Fine: slight rain. 6, 7. Fine. 8. Rain: cloudy and fine. 9. Very fine. 10. Very fine: rain. 11. Stormy and wet. 12. Fine. 13. Cloudy. 14. Rain: showery: clear at night. 15—17. Cloudy and fine. 18. Hazy: fine. 19, 20. Very fine. 21. Cloudy. 22. Cloudy: slight rain. 23. Rain: cloudy and fine. 24. Showery: clear. 25. Drizzly. 26. Hazy and mild. 27. Heavy dew: cloudy and hot. 28—30. Foggy in the mornings: very fine: evenings clear. 31. Overcast and fine.

Boston.—August 1. Fine: rain P.M. 2. Fine. 3. Cloudy: rain P.M. 4. Fine: rain early A.M. 5. Cloudy: rain P.M. 6. Cloudy and stormy. 7. Cloudy: rain P.M. 8, 9. Cloudy. 10. Fine. 11. Cloudy: rain early A.M.: rain P.M. 12. Stormy. 13. Cloudy. 14. Cloudy: rain early A.M. 15, 16. Cloudy. 17. Fine: rain P.M. 18, 19. Fine. 20. Fine: thermometer 77° half-past two P.M. 21. Fine: rain A.M. 22. Fine. 23, 24. Fine: rain early A.M. 25. Rain: rain early A.M. 26. Cloudy: thermometer 75° three-quarters past two P.M. 27. Fine: thermometer 75° quarter-past eleven A.M. 28, 29. Fine. 30, 31. Cloudy.

Applegarth Manse, Dumfries-shire.—August 1. Fair, but cool and cloudy. 2. Fair and fine. 3. Wet A.M.: cleared and was fine. 4. Fair and fine. 5. Rain all day. 6. Wet A.M.: cleared and was fine. 7. Wet, slightly. 8. Fine though showery: thunder. 9. Wet A.M.: became fine. 10. Showery. 11. Fair. 12. Showery all day. 13. Partial showers. 14. Wet A.M.: became fine. 15. Fine till P.M.: then rain. 16. Wet A.M.: cleared P.M. 17. Fair throughout. 18. Fair A.M.: wet P.M. 19. Fair and warm: air electrical. 20. Wet nearly all day: thunder. 21. Wet P.M.: flood. 22. Fine and fair. 23. Occasional slight showers. 24. Wet P.M. and evening: thunder. 25. Showery. 26. Rain early A.M.: cleared. 27. Fine: one shower A.M. 28. Wet morning: cleared. 29. Fine but cloudy. 30. Wet all day. 31. Fair and fine.

Days of Month. 1841. Aug.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8¾ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·830	29·923	29·790	29·30	29·68	29·75	58·3	65·2	50·0	66	49	57	61	49	NW.	SW.	calm	WNW.	·055	·02	...	...	54
○ 2.	29·986	29·937	29·765	29·42	29·75	29·67	62·5	64·0	54·0	70	58	61	62½	44	WSW.	SW.	NW.	SW.	·005	·38	·05	...	55
3.	29·742	29·691	29·411	29·16	29·53	29·38	62·8	70·8	60·4	73	57	64·5	63	53½	NNW.	SW.	calm	SSW.	·377	·38	...	...	59
4.	29·492	29·785	29·385	28·94	29·53	29·60	60·0	71·0	59·0	69	57	63	65½	51	N.	N.	N.	S.	·366	...	·86	...	58
5.	29·796	29·742	29·609	29·17	29·42	29·21	65·0	69·0	60·0	64	57	64	71	53	SW.	SW.	SE.	E.	...	·01	...	...	61
6.	29·748	29·851	29·663	29·05	29·34	29·55	60·6	66·0	58·5	67	57	62	60	54	W.	W.	W.	W.	·008	·07	·06	...	57
7.	29·962	29·893	29·798	29·33	29·59	29·50	64·0	69·5	60·5	74	58	62·5	61	52½	W.	W.	calm	SSW.	·088	...	...	1·47	57
8.	29·738	29·716	29·619	29·12	29·38	29·41	61·0	71·4	59·5	67	52	63	60	55½	SSW.	S.	W.	SW.	·008	·01	·01	...	59
9.	29·646	29·684	29·596	29·04	29·39	29·38	59·5	69·0	56·0	70	49	60	59	50½	W.	W.	calm	W.	...	·01	...	...	57
☾ 10.	29·874	29·800	29·678	29·26	29·49	29·58	61·0	72·5	53·0	68	55	61·5	58	48	SW.	S.	SW.	SW.	...	·36	...	...	56
11.	29·512	29·662	29·458	28·95	29·49	29·56	61·4	67·5	58·0	68	46	60	60	45	SW.	SW.	S.	NW.	·277	·30	·61	...	59
12.	29·958	29·923	29·888	29·34	29·70	29·74	59·2	68·0	51·0	65	41	57	61	44	W var.	NW.	W.	NNW.	·080	...	·45	...	51
13.	29·928	29·886	29·772	29·36	29·65	29·61	62·0	64·0	61·5	70	54	60	61	42	W.	S.	W.	E.	·013	·30	...	...	56
14.	29·646	29·719	29·599	29·08	29·39	29·44	62·0	66·5	57·0	72	54	61	62½	53	SW.	SW.	SW.	SW.	·213	·04	·25	0·28	58
15.	29·766	29·746	29·687	29·16	29·49	29·62	64·5	68·6	57·0	70	50	61·5	67	55	W.	SW.	calm	W.	·036	...	...	...	59
● 16.	29·928	29·921	29·851	29·32	29·72	29·72	61·5	71·6	56·0	74	58	60	64	45	WNW.	S.	calm	SW.	·011	...	...	...	57
17.	30·018	30·056	29·969	29·35	29·73	29·94	64·5	71·5	60·0	75	56	63·5	62½	54	SW.	W.	W.	NW.	...	...	...	...	60
18.	30·210	30·173	30·140	29·61	30·00	30·04	63·7	73·0	58·4	76	56	62·5	64	43½	NW.	W.	calm	NW.	...	...	·01	...	60
19.	30·276	30·197	30·006	29·60	30·00	29·95	63·5	69·5	56·0	77	47	61	61½	53	WSW.	SW.	calm	S.	...	...	...	...	57
20.	30·006	29·734	29·629	29·37	29·75	29·64	66·5	73·0	56·0	80	51	66	69	58	SSE.	W.	S.	W.	...	...	...	...	59
21.	29·684	29·903	29·797	29·00	29·55	29·52	66·7	77·5	53·0	72	48	67	61	49	W.	S.	SE.	NW.	...	...	...	2·00	63
22.	29·978	29·874	29·800	29·36	29·65	29·48	63·0	73·0	56·0	68	54	59	61	48½	SW.	S.	calm	E.	...	·58	·12	...	56
☽ 23.	29·904	29·884	29·797	29·30	29·60	29·78	58·0	68·0	57·0	72	44	57	63	49	WNW.	W.	calm	W.	·416	·01	·18	...	57
24.	30·124	30·094	30·048	29·36	29·94	29·94	57·0	67·5	50·5	67	45	56	61½	41½	WNW.	N.	NW.	WNW.	·015	·13	·04	...	48
25.	30·148	30·079	30·059	29·44	29·83	29·90	57·0	65·0	52·0	67	60	55·5	64½	50	SW.	SW.	NW.	SW.	·130	·07	·21	...	...
26.	30·268	30·225	30·181	29·59	30·00	30·10	63·2	65·5	57·0	79	62	64	63	55	W.	SW.	calm	SW.	·061	...	...	...	61
27.	30·332	30·244	30·142	29·59	30·07	30·00	69·7	70·0	65·0	81	51	69	65½	56½	W.	SW.	calm	SW.	·011	...	...	...	64
28.	30·264	30·189	30·125	29·54	30·01	30·03	62·7	76·5	59·5	77	52	62·5	65	54	W.	W.	calm	W.	...	...	...	1·26	59
29.	30·236	30·159	29·920	29·55	29·92	29·85	65·4	72·5	59·0	81	51	65	66	54½	WSW.	SW.	calm	SW.	...	...	...	...	62
30.	30·064	29·998	29·840	29·35	29·73	29·60	63·5	77·0	60·0	80	67	65	61	56½	W.	S.	calm	SSW.	...	...	...	...	60
31.	29·826	29·844	29·729	29·16	29·69	29·80	66·7	76·0	63·0	74	45	65	62	50½	W.	NW.	calm	WNW.	...	·02	...	...	63
Mean.	29·932	29·920	29·798	29·[illegible]	29·680	29·716	62·5	70·0	57·2	72·03	52·93	61·8	62·1	50·5					Sum. 2·168	2·69	2·85	5·01	Mean. 58

THE

LONDON, EDINBURGH AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[THIRD SERIES.]

NOVEMBER 1841.

XLVIII. *On the Chemical Statics of Organized Beings.* *By* M. DUMAS.

[The discourse of which the following is a translation, attracted, as we are informed, much attention, on its delivery by M. Dumas. It formed the concluding Lecture of his course at the *Ecole de Médecine*, and has just been published.—ED.]

LIFE, whose painful mysteries you are called upon to fathom, exhibits among its phænomena some which are manifestly connected with the forces that inanimate nature herself brings into action, others which emanate from a more elevated source, less within the reach of our boldest stretch of thought.

It has not been my province to accompany you in looking with an inquisitive eye into all that part of your studies under which those facts which appertain to the normal or irregular exercise of the instincts of life arrange themselves. Still less have we ever had to bring under our consideration those noble faculties, by means of which the human intellect, mastering all that surrounds it, breaking down all obstacles, bending all the powers of nature to its wants, has step by step made conquest of the earth, of the seas, of the whole globe;—a vast domain, which our recollections, our presentiments perhaps, so often make us consider as too narrow a prison. To others more fortunate belongs the care of initiating you in these important studies, the privilege of unfolding to you these lofty themes; our task, more humble, must be limited to the field of the physical phænomena of life; and there are still some which have not found a place in our lectures.

It is specially, indeed, the functions of matter in the production and growth of organized beings, the part which it takes in the accomplishment of the phænomena of their daily existence, the alterations which it undergoes after their death, that we have had to study together, and this study alone has quite sufficed us for this year's occupation.

I.—Plants, animals, man, contain matter. Whence comes it? What does it effect in their tissues and in the fluids which bathe them? What becomes of it when death breaks the bonds by which its different parts were so closely united?

These are the questions which we touched upon together, at first, with hesitation, for the problem might be far above the powers of modern chemistry; we afterwards considered them with somewhat more confidence, as we felt from the silent and inward assent of our understandings that the path was sure, and that we could descry the goal gradually standing out, clear of all that obstructed our vision. If from these labours, which you have witnessed, or I should rather say, in which you have taken part; if from this scientific effort there have arisen some general views, some simple formulæ, it is my duty to become their historian; but allow me the pleasure of adding, that they belong to you, that they belong to our school, the intelligence of which has been exercised on this new ground. It is the ardour with which you have followed me in this career that has given me strength to pursue it; it is your interest which has sustained me; your curiosity which has awakened mine; your confidence which has made me see, and which proves to me at this moment that we are still in the path of truth.

These remarks will remind you of the wonder with which we found, that of the numerous elements of modern chemistry, organic nature borrows but a very small number; that from these vegetable or animal matters, now multiplied to infinity, general physiology borrows not more than from ten to twelve species; and that all the phænomena of life, so complicated in appearance, belong, essentially, to a general formula so simple, that, so to speak, in a few words the whole is stated, the whole summed up, the whole foreseen.

Have we not proved, in fact, by a multitude of results, that animals constitute, in a chemical point of view, a real apparatus for combustion, by means of which burnt carbon incessantly returns to the atmosphere under the form of carbonic acid; in which hydrogen burnt without ceasing, on its part continually engenders water; whence, in fine, free azote is incessantly exhaled by respiration, and azote in the state of oxide of ammonium by the urine?

Thus from the animal kingdom, considered collectively, constantly escape carbonic acid, water in the state of vapour, azote, and oxide of ammonium,—simple substances, and few in number, the formation of which is strictly connected with the history of the air itself. Have we not, on the other hand, proved that plants, in their normal life, decompose carbonic acid for the purpose of fixing its carbon and of disengaging its oxygen; that they decompose water to combine with its hydrogen, and to disengage also its oxygen; that, in fine, they sometimes borrow azote directly from the air, and sometimes indirectly from the oxide of ammonium, or from nitric acid, thus working in every case in a manner the inverse of that which is peculiar to animals? If the animal kingdom constitutes an immense apparatus for combustion, the vegetable kingdom, in its turn, constitutes an immense apparatus for reduction, in which reduced carbonic acid yields its carbon, reduced water its hydrogen, and in which also reduced oxide of ammonium and nitric acid yield their ammonium or their azote.

If animals then continually produce carbonic acid, water, azote, oxide of ammonium,—plants incessantly consume oxide of ammonium, azote, water, carbonic acid. What the one class of beings gives to the air, the others take back from it; so that to take these facts at the loftiest point of view of terrestrial physics, we must say that, as to their truly organic elements, plants and animals spring from air,—are nothing but condensed air; and that, in order to form a just and true idea of the constitution of the atmosphere at the epochs which preceded the birth of the first organized beings on the surface of the globe, there must be placed to the account of the air, by calculation, that carbonic acid and azote, whose elements have been appropriated by plants and animals. Thus plants and animals come from the air, and thus to it they return; they are real dependences of the atmosphere.

Plants, then, incessantly take from the air what is given to it by animals; that is to say, carbon, hydrogen and azote, or rather, carbonic acid, water and ammonia.

It now remains to be stated, how in their turn, animals acquire those elements which they restore to the atmosphere; and we cannot see without admiring the sublime simplicity of all these laws of nature, that animals always borrow these elements from plants themselves.

We have, indeed, ascertained, from the most satisfactory results, that animals do not create true organic matters, but that they destroy them; that plants, on the contrary, habitually create these same matters, and that they destroy but few of them, and that in order to effectuate particular and determinate conditions.

Thus it is in the vegetable kingdom that the great laboratory of organic life resides; there it is that the vegetable and animal matters are formed, and they are there produced at the cost of the air:

From vegetables, these matters pass ready-formed into the herbivorous animals, which destroy a portion of them, and accumulate the remainder in their tissues:

From herbivorous animals, they pass ready-formed into the carnivorous animals, who destroy or retain some of them according to their wants:

Lastly, during the life of these animals, or after their death, these organic matters, as they are destroyed, return to the atmosphere whence they proceeded.

Thus closes this mysterious circle of organic life at the surface of the globe. The air contains or engenders oxidized products, as carbonic acid, water, nitric acid, oxide of ammonium. Plants, constituting true reducing apparatus, possess themselves of their radicals, carbon, hydrogen, azote, ammonium. With these radicals they form all the organic or organizable matters which they yield to animals. These, forming, in their turn, true apparatus for combustion, reproduce carbonic acid, water, oxide of ammonium and nitric acid, which return to the air to produce anew and through endless ages the same phænomena.

And if we add to this picture, already, from its simplicity and its grandeur, so striking, the indisputable function of the solar light, which alone has the power of putting in motion this immense apparatus,—this apparatus never yet imitated, constituted of the vegetable kingdom, and in which is accomplished the reduction of the oxidized products of air,—we shall be struck with the import of these words of Lavoisier:—

"Organization, sensation, spontaneous movement, life, exist only at the surface of the earth, and in places exposed to the light. It would seem that the fable of the torch of Prometheus was the expression of a philosophic truth which had not escaped the ancients. Without light, nature was without life, was dead and inanimate: by the gift of light, a beneficent God spread upon the surface of the earth organization, feeling and thought."

These words are as true as they are beautiful. If feeling and thought, if the noblest faculties of the soul and of the intellect, have need, for their manifestation, of a material covering, to plants is assigned the framing of its web with the elements which they borrow from the air, and under the influence of the light which the sun, its inexhaustible source, pours in unceasing floods upon the surface of the globe.

And as if, in these great phænomena, all must be connected with causes which appear the most distant from them, we must moreover remark how the oxide of ammonium, the nitric acid, from which plants borrow a part of their azote, are themselves almost always derived from the action of the great electric sparks which flash forth in stormy clouds, and which (furrowing the air through a vast extent) produce there the nitrate of ammonia which analysis detects in it.

Thus, from the craters of those volcanos whose convulsions so often agitate the crust of the globe, continually escapes carbonic acid, the principal nutriment of plants; from the atmosphere flashing with lightnings, and from the midst of the tempest itself, there descends upon the earth the other and no less indispensable nutriment of plants, that whence they derive almost all their azote, the nitrate of ammonia, contained in storm-showers.

Might not this be called, as it were, an idea of that chaos of which the Bible speaks, of those times of disorder and of tumult of the elements, which preceded the appearance of organized beings upon the earth?

But scarcely are the carbonic acid and the nitrate of ammonia produced, than a form more calm, although not of inferior energy, comes to put them in action,—it is Light. Through her influence, the carbonic acid yields its carbon, the water its hydrogen, and the nitrate of ammonia its azote. These elements unite, organized matters form, and the earth puts on its rich carpet of verdure.

It is then by continually absorbing a real force, the light and the heat emanating from the sun, that plants perform their functions, and that they produce this immense quantity of organized or organic matter, pasture destined for the consumption of the animal kingdom.

And if we add, that animals on their part produce heat and force in consuming what the vegetable kingdom* has produced and has slowly accumulated, does it not seem that the ultimate end of all these phænomena, their most general formula, reveals itself to our sight?

The atmosphere appears to us as containing the primary substances of all organization, volcanos and storms as the laboratories in which were first produced the carbonic acid and the nitrate of ammonia which life required for its manifestation or its multiplication.

In aid of these comes light, and developes the vegetable kingdom,—immense producer of organic matter: plants absorb the chemical force which they derive from the sun to decompose carbonic acid, water and nitrate of ammonia;

[* "*Le règne animal*" in the original; but this is obviously an error.—Edit.]

as if plants realized a reducing apparatus superior to all those with which we are acquainted; for none of these would decompose carbonic acid in the cold.

Next come animals, consumers of matter and producers of heat and force, true apparatus for combustion. It is in them undoubtedly that organized matter puts on its highest expression. But it is not without suffering from it that it becomes the instrument of sensation and of thought; under this influence organized matter undergoes combustion; and in reproducing the heat and the electricity, which produce our strength and which are the measure of its power, these organized or organic matters become annihilated in order to return to the atmosphere whence they came. Thus the atmosphere constitutes the mysterious link which binds the vegetable to the animal kingdom.

Vegetables then absorb heat and accumulate matter which they have the power to organize.

Animals, through whom this organized matter only passes, burn or consume it in order to produce in its aid the heat and the different powers which their movements turn to account.

Suffer me, therefore, if, borrowing from modern sciences an image of sufficient magnitude to bear comparison with these great phænomena, we should liken the existing vegetation,—truly a storehouse in which animal life is fed,—to that other storehouse of carbon constituted of the ancient deposits of pit-coal, and which, burnt by the genius of Papin and of Watt, also produces carbonic acid, water, heat, motion,—one might almost say life and intelligence.

In our view, therefore, the vegetable kingdom will constitute an immense depôt of combustible matter destined to be consumed by the animal kingdom, and in which the latter finds the source of the heat and of the locomotive powers of which it avails itself.

Thus we observe a common tie between the two kingdoms, the atmosphere; four elements in plants and in animals, carbon, hydrogen, azote and oxygen; a very small number of forms under which vegetables accumulate them, and under which animals consume them; some very simple laws, which their connexion simplifies still more; such would be the picture of the most elevated state of organic chemistry which would result from our conferences of the present year.

You, like myself, have felt, that before separating we have need of collecting our thoughts, of fixing with precision all the facts, of bringing together and summing up the opinions which explain and develop these great principles; lastly, that it was useful, as regarded your future studies, to give you in writing and in a clearer form the expression of these views, which were partly brought into existence under the stimulus of your presence, and consequently reduced into form with the hesitation which so often accompanies the first enunciation of our thoughts.

II.—Since [the causes of] all the phænomena of life are exerted upon matters which have for their base carbon, hydrogen, azote, oxygen; since these matters pass over from the animal kingdom to the vegetable kingdom by intermediary forms, carbonic acid, water, and the oxide of ammonium; lastly, since air is the source whence the vegetable kingdom is fed, and the reservoir in which the animal kingdom is annihilated; we are led to take a rapid survey of these different bodies with a special view to general physiology.

Composition of Water.—Water is incessantly formed and decomposed in animals and plants; to appreciate what results from this, let us first see how it is composed. Some experiments founded on the direct combustion of hydrogen, and in which I have produced more than two pounds of artificial water,—experiments which are in truth very difficult and very delicate, but in which any errors would be unimportant with regard to the circumstances which we are engaged upon,—make it very probable that water is formed, in weight,

Of 1 part hydrogen
And 8 parts oxygen,

and that these whole and simple numbers express the true relation according to which these two elements combine to form water.

As substances always present themselves to the eyes of the chemist by molecules, as he always endeavours to connect in his thoughts, with the name of each substance, the weight of the molecule, the simplicity of this relation is not unimportant.

In fact, each molecule of water being formed of one molecule of hydrogen and one molecule of oxygen, we arrive at these simple numbers, which cannot be forgotten.

A molecule of hydrogen weighs 1; a molecule of oxygen weighs 8; and a molecule of water weighs 9.

Composition of Carbonic Acid.—Carbonic acid keeps incessantly forming in animals, and is continually undergoing decomposition in plants; its composition, therefore, deserves a special notice in its turn.

Now carbonic acid, like water, is represented by the most simple numbers.

Experiments founded on the direct combustion of the diamond, and on its conversion into carbonic acid, have proved to me that this acid is formed of the combination of 6 parts by weight of carbon and 16 parts by weight of oxygen.

We are therefore led to represent carbonic acid as being formed of one molecule of carbon weighing 6, and two molecules of oxygen weighing 16, which constitute one molecule of carbonic acid weighing 22.

Composition of Ammonia.—Lastly, ammonia, in its turn, seems formed in whole numbers of 3 parts of hydrogen and 14 of azote, which may be represented by 3 molecules of hydrogen weighing 3, and by 1 molecule of azote weighing 14.

Thus, as if the better to show all her power, nature operates, in the business of organization, upon a very small number only of elements, combined in the most simple proportions.

The atomic system of the physiologist revolves on these four numbers: 1, 6, 7, 8.

1, is the molecule of hydrogen;
6, that of carbon;
7, or twice 7, i. e. 14, that of azote;
8, that of oxygen.

These numbers should always be associated with these names, because for the chemist there can exist no abstract hydrogen, nor carbon, nor azote, nor oxygen. They are beings in their reality which he has always in view; it is of their molecules that he always speaks; and to him the word hydrogen depicts a molecule which weighs 1; the word carbon, a molecule which weighs 6; and the word oxygen, a molecule which weighs 8.

Composition of the Air.—Does atmospheric air, which performs so great a part in organic nature, also possess as simple a composition as water, carbonic acid and ammonia? This is the question which M. Boussingault and I have recently been studying. Now, we have found that, as the greater number of chemists have thought, and contrary to the opinion of Dr. Prout, to whom chemistry owes so many ingenious views, air is a mixture,—a true mixture.

In weight, air contains 2,300 of oxygen for 7,700 of azote; in volume, 208 of the first for 792 of the second.

The air, besides, contains from 4 to 6–10,000ths of carbonic acid in volume, whether it be taken at Paris or in the country. Ordinarily, it contains 4–10,000ths.

Moreover, it contains a nearly equal quantity of the carburetted hydrogen gas which is called marsh gas, and which stagnant waters disengage perpetually.

We do not speak of aqueous vapour, which is so variable;

of oxide of ammonium and of nitric acid, which can only have a momentary existence in the air because of their solubility in water.

The air, then, is constituted of a mixture of oxygen, azote, carbonic acid, and marsh gas.

The carbonic acid in it varies, and indeed greatly, since the differences in it extend almost from the simple to the double, from 4 to 6–10,000ths. May not this be a proof that plants take from the air this carbonic acid, and that animals take back a part from it? in a word, may this not be a proof of that equilibrium of the elements of the air attributed to the inverse actions which animals and plants produce upon it?

It has, indeed, been long since remarked; animals borrow from the air its oxygen, and give to it carbonic acid; plants, in their turn, decompose this carbonic acid in order to fix its carbon and restore its oxygen to the air.

As animals breathe continually; as plants breathe under the solar influence only; as in winter the earth is stript, whilst in summer it is covered with verdure; it has been supposed that the air must transfer all these influences into its constitution.

Carbonic acid should augment by night and diminish by day. Oxygen, in its turn, should follow an inverse progress.

Carbonic acid should also follow the course of the seasons, and oxygen obey the same law.

All this is true, without doubt, and quite perceptible as to a portion of air limited and confined under a jar; but in the mass of the atmosphere, all these local variations blend and disappear. Accumulated centuries are requisite in order effectually to put in action this balance of the two kingdoms, with regard to the composition of air; we are then very far from those daily or yearly variations, which we had been apt to look upon as being as easy to observe as to foresee.

With regard to oxygen, calculation shows that, exaggerating all the data, not less than 800,000 years would be required for the animals living on the surface of the earth to consume it entirely.

Consequently, if we suppose that an analysis of the air had been made in 1800, and that during the entire century plants had ceased to perform their functions on the surface of the whole globe, the animals at the same time all continuing to live, the analysts in 1900 would find the oxygen of the air diminished by 1–8000th of its weight, a quantity which is beyond the reach of our most delicate methods of observation, and which, assuredly, would have no influence whatever on the life of animals or plants.

As to this, then, we cannot be deceived; the oxygen of the air is consumed by animals, who convert it into water and carbonic acid; it is restored by plants, which decompose these two bodies.

But nature has arranged everything so that the store of air should be such with relation to the consumption of animals, that the want of the intervention of plants for the purification of the air should not be felt until centuries have elapsed.

The air which surrounds us weighs as much as 581,000 cubic kilometres of copper; its oxygen weighs as much as 134,000 of these same cubes. Supposing the earth peopled with a thousand millions of men, and estimating the animal population at a quantity equivalent to three thousand millions of men, we should find that these quantities united consume in a century only a weight of oxygen equal to 15 or 16 cubic kilometres of copper, whilst the air contains 134,000 of it.

It would require 10,000 years for all these men to produce a perceptible effect upon the eudiometer of Volta, even supposing vegetable life annihilated during all this time.

In regard to the permanence of the composition of air, we may say with all confidence, that the proportion of oxygen which it contains is secured for many centuries, even reckoning for nothing the influence of vegetables, and that, nevertheless, these restore oxygen to it incessantly in quantity at least equal to that it loses, and perhaps more; for vegetables live just as much at the expense of the carbonic acid furnished by volcanos, as at the expense of the carbonic acid furnished by animals themselves.

It is not then for the purpose of purifying the air that these breathe, that vegetables are especially necessary to animals; it is, above all, to furnish them, incessantly, with organic matter quite ready for assimilation; organic matter, which they may burn to their advantage.

There is, therefore, a service necessary, without doubt, but so remote, that it can scarcely be recognized, which vegetables render us, in purifying the air which we consume. There is another service so immediate, that if, during a single year, it were to fail us, the earth would be depopulated; it is that which these same vegetables render us by preparing our nutriment, and that of all the animal kingdom. In this, especially, is found the chain that binds together the two kingdoms. Annihilate plants, and the animals all perish of a dreadful famine; organic nature itself entirely disappears with them in a few seasons.

We have, however, said, that the carbonic acid of the air varies from 4 to 6–10,000th. These variations are very frequent and very easy to observe. Is not this a phænomenon reproaching the influence of animals who introduce this acid into the air, and that of vegetables, which deprive it of it?

No; this phænomenon, you are aware, is a simple meteorological phænomenon. It is with carbonic acid as with aqueous vapour, which forms on the surface of the sea, to become condensed elsewhere, fall again in rain, and be reproduced under the form of vapour.

This water, which is condensed and falls, dissolves, and carries with it carbonic acid; this water, which evaporates, yields up this same gas to the air.

A great meteorological interest would attach to the observation of the variations of the hygrometer, and those of the seasons, or of the state of the sky with the variations of the carbonic acid of the air; but hitherto all tends to show that these rapid variations constitute a simple meteorological event, and not, as had been thought, a physiological event, which, singly considered, would infallibly produce variations infinitely slower than those which are, in fact, observed as much in towns as in the country itself.

Thus the air is an immense reservoir, whence plants may for a long time derive all the carbonic acid necessary for their wants; where animals, during a much longer time still, will find all the oxygen that they can consume.

It is also from the atmosphere that plants derive their azote, whether directly or indirectly: it is there that animals finally restore it.

The atmosphere is therefore a mixture which unceasingly receives and supplies oxygen, azote or carbonic acid, by means of a thousand exchanges of which it is now easy to form a just idea, and the details of which a rapid analysis will now enable us to appreciate.

[To be concluded in our next Number.]

XLIX. *On the Periodical Shooting Stars, and on Shooting Stars in general. By* Miss ZORNLIN[*].

OF the meteorological nature of the periodical shooting stars of August and November, little doubt, I should think, can remain in the mind of any one who has observed and considered the general character of the attendant phænomena. It may, however, be desirable to adduce a few instances in support of this assumption.

In our own island, the November periodical shooting stars have not been witnessed in the same splendour as in other lands possessing a clearer atmosphere, and perhaps a more excessive climate. Phænomena of no small degree of interest, have, however, not unfrequently been exhibited in this country; these, in some cases, not being the less important, from the evidence they afford that the periodical shooting stars cannot be regarded as an isolated meteorological phænomenon.

---

[*] Communicated by the Author.

The most remarkable display of the November shooting stars of which I have met with any record, as occurring in Britain, took place in 1832; and as I believe that the phænomena here referred to have not hitherto been brought forward as connected with the periodical shooting stars, it may not be uninteresting to insert some notice of them in this place. Nor are these accounts less worthy of notice, from the circumstance of their having been recorded before public attention had been directed to the probable periodicity of the phænomenon, by the grand display of 1833. A paper was read before the Royal Society on the 13th of December, 1832, entitled 'An Account of an extraordinary Meteor seen at Malvern, November 12, 1832,' by W. Addison, Esq., F.L.S., "in which the author states that he beheld, from the Malvern Hills, a constant succession of meteors, of various degrees of magnitude and brilliancy. The smaller ones were like those commonly called shooting stars; others were much more brilliant; and notwithstanding the bright moonshine, threw a strong glare upon every object: they also left behind them a long train of very vivid white light. The author witnessed this scene for upwards of an hour, and it was still going on when he left it. At one time he counted forty-eight of these meteors during the interval of five minutes*." In the London Literary Gazette of the same year, we find the following account:—"It appears from the provincial newspapers, in various parts of the country, that very remarkable phænomena were seen, both north and south, on the morning of Tuesday week [November 13th]. Fiery meteors and falling stars (as they are called) issued from the west, and illuminated the heavens in their course, leaving behind them trains of brilliant white. The appearances seem to have been very grand, and to have excited much admiration in the beholders." From these accounts, it must be inferred that the display of the periodical shooting stars on this occasion was very splendid; and also of considerable duration, commencing, as it evidently did, on the evening of the 12th, and extending to the morning of the 13th of November. The phænomenon was also witnessed, at the same time, in considerable splendour, at Geneva, and in other parts of Europe; at Orenburg, and in Arabia.

---

* Abstracts of the Philosophical Transactions, vol. iii. p. 159. See also Phil. Mag. for July 1833. [Third Series, vol. iii. p. 37.]

The November periodical shooting stars have likewise, as is well known, been observed in later years in this country, though in smaller numbers; but what is more to my present purpose, on some occasions, whilst a brilliant display of these meteors has been witnessed in other parts of the globe, meteoric phænomena of different character have been observed in this island. Thus, on the evening of November 12, 1837, a fine coloured aurora, accompanied by some shooting stars, was observed at this place, as well as in many other parts of Britain *; and on the morning of the 13th of November, 1838, I witnessed meteoric phænomena of the grandest description, consisting of three successive and distinct splendid auroras, exhibiting well-defined arches, as also streamers and patches of the most vivid red and green hues. This magnificent display was accompanied by a small number of shooting stars†. Auroras, and other similar meteoric phænomena, are also on record as having occurred in various years on the 12th and 13th of November in other parts of the globe. Thus, on the 13th of November, 1832, when, as already mentioned, shooting stars were observed in great splendour in this country, as also at Geneva, in Arabia, and at Orenburg, remarkable meteoric phænomena, consisting of brilliant luminous columns and bands, were witnessed both to the north and south of the latter place; and on the 13th of November, 1836, an aurora was observed at New York, and other places in the United States. Nor does the occurrence of these periodical meteoric phænomena appear to be confined to very recent times; for we find it recorded, that on November 13, 1755, "The sky was red, and red rain fell in different countries." And again, on the 14th of November, 1765, "Red rain fell in Picardy‡." Nay, the collection of facts from which the above accounts are taken, enables us to trace the occurrence of apparently similar phænomena to a much earlier era; it being recorded that on the 5th or 6th of November, 472, there was "a great fall of black dust, probably at Constantinople, during which the heavens

---

* See an account by Professor Forbes in Phil. Mag., Supplementary Number for January 1838. [Third Series, vol. xii. p. 85.]

† For a notice of the phænomena exhibited on this occasion, I would refer to a paper in the Phil. Mag. for Jan. 1839, [S. 3. vol. xiv. p. 39.] by Mr. W. R. Birt, with whose observations my own, as far as they were carried, closely correspond; though no description could convey an adequate idea of the splendour of the phænomena.

‡ Account of Meteoric Stones, &c. which have fallen from the Heavens, from the earliest period down to 1819. Edin. Phil. Journal, Oct. 1819.

seemed to burn." The latter date, allowing for the difference of style in that age, would form a close approximation to the present 12th or 13th day of the month.

Similar in character to the November shooting stars, are those of August; and, like the former, they also are accompanied by other meteoric phænomena. Thus, on both occasions, lightning has frequently been noticed; and luminous appearances in the clouds were observed by myself on the night of August 11, 1839 *, much resembling those noticed by M. Wartmann at Geneva, on the night of November 12, 1836 †. And in the Sixth Report of the British Association for the Advancement of Science, we meet with an account, communicated by Dr. Traill, of a splendid aurora borealis, which was observed on August 11, 1836. A very remarkable instance of the August periodical meteors, which is mentioned in Colonel Reid's work 'On the Law of Storms,' may be worth citing in this place ‡. The phænomenon occurred during the hurricane at Barbadoes in 1831, and the relation is given in an extract from an account published at Bridgetown in that island, immediately after it occurred. The hurricane had been raging in all its fury, accompanied by incessant lightning. At about three o'clock a.m. on the 11th of August, a temporary cessation in the violence of the wind occurred. "The lightning having also ceased, for a few moments only at a time, the blackness in which the town was enveloped was inexpressibly awful. Fiery meteors were presently seen falling from the heavens; one in particular, of a globular form, and a deep red hue, was observed by the writer to descend perpendicularly from a vast height. It evidently fell by its specific gravity, and was not shot or propelled by any extraneous force. On approaching the earth with accelerated motion, it assumed a dazzling whiteness, and an elongated form, and dashing to the ground in Beckwith Square, opposite the stores of Messrs. U. D. Grierson and Co., it splashed around in the same manner as melted metal would have done, and was instantly extinct: its brilliancy, and the spattering of its particles on the earth, gave it the resemblance of a body of quicksilver of equal bulk." Shortly afterwards, the lightning recurred in terrific grandeur, and the hurricane in increased violence §.

---

* Phil. Mag., December 1839. [S. 3. vol. xv. p. 441.]

† Phil. Mag., September 1837. [S. 3. vol. xi. p. 261.]

‡ Some of the severest storms on record have occurred in the months of August and November. It would form an interesting subject of inquiry, whether such storms have any connexion with the appearance or the non-appearance of the periodical meteoric phænomena at those times.

§ Lieut.-Col. Reid on the Law of Storms, 1st edit., p. 29.

It would be easy to multiply instances of the periodical return both of the November and August shooting stars, but the periodicity of these meteors I consider as granted; and, with the exception of two remarkable instances, neither of which I believe has appeared in any published catalogue, and which I have therefore been induced to insert, my object has been solely to bring forward facts in support of the opinion that the display of the periodical shooting stars cannot be regarded as an isolated meteorological phænomenon.

The nature and origin of the periodical, and indeed of all other shooting stars, appears still to be involved in much obscurity. From the above, and similar facts, it may, however, be inferred, that they are connected with other meteorological phænomena—phænomena which apparently originate in electricity, and which may be considered as atmospheric. We shall therefore be led to seek for their origin in our own atmosphere, and shall not surely be wandering from the mark, if to electricity we refer the production of these, and perhaps of all meteors of similar character.

The visible effects of electricity (whether voltaic or ordinary) are, according to Mr. Faraday, "the evolution of heat, the production of magnetism, chemical decomposition, physiological changes, and lastly, the evolution of light, in the form of a spark." Among the visible effects of electricity in motion, are also included the luminous appearances presented by the aurora borealis. And may not shooting stars originate in electric currents more energetically developed?

According to the law discovered by Mr. Faraday, "the decomposing action of any current of electricity is constant for a constant quantity of electricity;" and as the same philosopher has proved by experiment, "a given quantity of electricity, whether passed in one or in many portions, invariably decomposes the same quantity of water." That a similar constancy will exist in the decomposing action of currents of electricity which may be evolved in the atmosphere, cannot be questioned; and that effects analogous to those elicited by the experiments made by Mr. Faraday on the decomposition of water should be produced in the atmosphere by such electric currents, is more than probable. If, then, we may suppose the decomposition of water into its constituent elements to be in progress in the atmosphere, we must also look for some reaction, some antagonist power to counterbalance such a destructive process, some agent by which the reproduction of water may be effected. And may not electricity, under some circumstances, be evolved to so great a degree in the atmosphere*, as to cause the compression and combination of the elements of oxygen and hydrogen? a combination which would be accompanied by ignition and explosion, and by the formation of water. May not such ignition and explosion produce the luminous appearances called shooting stars?

Processes of this description might be in progress at all elevations in the atmosphere to which oxygen and hydrogen (and perhaps, in some cases, nitrogen also) can extend or be forced in contact; and consequently, wherever aqueous vapour exists to be decomposed, that is, probably, to the utmost limits of our atmosphere; and perhaps these gases may even occasionally be driven by electric currents beyond its ordinary limits. If such an explosion should take place beyond these limits, or even in very elevated strata of the atmosphere, it would necessarily be noiseless; but if within a short distance of the earth's surface, a detonation might be heard. The latter phænomena might also, in some instances, appear of large dimensions, and occasionally (as in the case of the meteor already alluded to at Barbadoes) approach the ground; or even (as in the instance of the large and brilliant meteor which fell on the 13th of November, 1835, and set fire to a barn near Belley, in the Département de l'Ain †,) might sometimes cause the ignition of combustible substances. The apparent balls of fire, occasionally witnessed during thunder-storms, may, perhaps, be of similar or nearly similar origin. And in these meteors we might also find an explanation of the remarkable phænomenon of thunder in a clear and serene sky, alluded to by M. Arago in the *Annuaire* for 1838, and observed by Mr. Addison at Great Malvern on the 4th of August, 1835, when "a remarkable noise was heard at 4 p.m., like a loud clap of thunder, the air at the time being quite free from cloud, and the sun hot and brilliant ‡." Nor is it impossible, that to a somewhat similar cause may be attributed the phænomena of water-spouts, and perhaps also of whirlwinds §; for Captain Beechey, speaking of the former, relates that on one occasion

---

* The remarkable fact, very recently observed, of the production of electricity by the jets of steam issuing from boilers, and the instance it gives of the evolution of electricity upon an enormous scale, during the conversion of water into vapour, (see Phil. Mag., November 1840, S. 3. vol. xvii. p. 370.) tends to confirm and support the views here advanced.

† *Annuaire du Bureau des Longitudes*, 1836.

‡ Report to the British Association for the Advancement of Science, 1839.

§ It is, at least, a singular coincidence, that the remarkable phænomenon of moving pillars of sand, as described by Bruce, in Nubia, occurred on the 14th and 15th of November.

"a ball of fire was observed to be precipitated into the sea;" and Colonel Reid mentions that "many of the descriptions of these whirlwinds speak of visible flame attending them, and occasionally of a remarkable noise." If we suppose the portion of ignited matter to consist solely of oxygen and hydrogen, the result would be pure water; and, when formed in elevated strata of the atmosphere, no trace evident to our senses might remain (unless, indeed, when these meteors should occur in vast numbers, in which case the light reflected by the newly-formed water or vapour, might present the appearance of luminous bands): but if such an ignition and explosion were to take place in lower strata, it might give rise to the showers of rain which fall so suddenly in water-spouts. The atmosphere, however, contains a large proportion of nitrogen, and in its lower strata, carbonic acid, as also various foreign substances, more especially in the vicinity of volcanos, (where, as we shall see in the sequel, these meteors are of very frequent occurrence,) carried up into it by evaporation and other means. And should a combination with such substances take place, red rain, meteoric dust, and perhaps even aerolites might be formed. The ignition of such foreign matter might also account for the various hues of different shooting stars, and also for the trains of sparks occasionally observed.

If we can suppose this hypothesis to be an approximation to the truth, it would unfold to us highly interesting views of processes continually in progress in nature's laboratory:—the perpetual formation and decomposition of water; dissolution and renovation unceasingly occurring in the very constitution of that portion of the terrestrial globe, which, among all the geological revolutions that have taken place on its surface, appears to have suffered the least change—the world of waters*; changes analogous to those observable in all other departments of the natural world; changes by which, in fact, Nature appears to maintain "her health, her beauty, her fertility." And these remarkable meteors might thus be regarded as celestial beacons, shining forth to impart to us information of physiological changes in the very act of occurring, perhaps, in the remotest regions of our atmosphere.

Should such be the origin and office of these meteors, we might, however, expect to find them of continual occurrence, rather than making their appearance as periodical visitors. And, in fact, although they doubtless seem to have their gala nights, M. Quetelet infers, from observations he has made, that in our latitudes about sixteen shooting stars of various sizes may be seen on a dark night in the course of an hour. Nor need we suppose the phænomena to be discontinued during the hours of sun-light. And in the warmer regions of the globe, the phænomenon of shooting stars is, according to M. de Humboldt, of very frequent occurrence. Thus, when off the coast of Africa, and especially near the Canaries, that eminent traveller speaks of "the innumerable multitude of falling stars, which appeared every instant." He adds, "That he never saw them so multiplied as in the vicinity of the volcanos of Quito, and in the part of the Pacific Ocean which bathes the volcanic coasts of Guatemala." He also describes these meteors as being very brilliant, and as usually either leaving trains behind them, which remain luminous twelve or fifteen seconds, or bursting into sparks. And, should the great height above the earth's surface assigned to some of these meteors* seem to militate against the hypothesis here advanced, on the other hand, it cannot but appear that the fact of their frequent occurrence in volcanic regions distinctly leads to the inference that they are not solely cosmical phænomena, but absolutely and intimately connected with the physical constitution of our own planet. And when we consider the vast number of these meteors†, we cannot but feel convinced that they are not mere accidental displays, calculated to alarm the ignorant, and to excite the wonder and curiosity of the learned, but that they actually perform some important though hitherto unperceived office in the œconomy of nature.

---

* The decomposition and formation of water is continually in progress on the earth's surface by combustion and other means; but Nature plans for a globe uninhabited by man; and, with the exception of rare and occasional conflagrations caused by the electric fluid, and perhaps by spontaneous ignition, fire is not kindled on the earth without human agency.

I am well aware that the above attempt at explanation does not account for the periodicity of these meteors, which, in fact, constitutes one of their most remarkable features; but perhaps we can scarcely expect to arrive at any satisfactory conclusion as to the cause of their periodicity, until we become in some measure acquainted with their nature and origin. The facts above adduced prove that the periodical shooting stars are not isolated meteorological phænomena; whilst their evident connexion with the aurora borealis leads us to regard them as of electric origin. If, then, we consider these phænomena as originating in electricity in motion; and if, to use the words of Sir John Herschel, we consider "that the sun's rays are the ultimate source of every motion that takes place on the surface of the earth," (possibly modified not only by the direction in which they strike the earth in its transit through space, but also by the varying rapidity with which the earth itself moves in different parts of its orbit,) we may perhaps regard the sun as mainly influential in their production. And therefore, although meteorological observations must necessarily be of great importance as tending to elucidate the subject*, it cannot but be hoped that astronomy will also lend its powerful aid, and that future observations made on the sun's disc, at, or immediately antecedent to the time indicated by the periodical appearance of these meteors, and compared with the ordinary phænomena presented by its surface, (both in our latitudes and in tropical regions, where shooting stars are of more common occurrence,) may detect some unusual agitation, some apparently peculiar condition in the sun itself at those especial periods, to which may be attributed the periodical occurrence of these meteoric phænomena.

---

* One hundred German miles (466 English miles) or more.—See an article by M. Quetelet from the *Annuaire de Bruxelles*, in Phil. Mag. for September 1837. [Third Series, vol. xi. p. 271.]

† At the rate of 16 per hour, according to M. Quetelet's observations, not less than 140,000 annually; or, if this be considered as too large a proportion, let us take only half that number, and this in a comparatively circumscribed space, and in a temperate parallel of latitude.

Clapham, Nov. 6, 1840. ROSINA MARIA ZORNLIN.

An accidental circumstance has retarded the publication of the above paper until the present period; and, in the interim, two rather important articles have been given to the public on the subject of these meteors; the one forming a portion of Professor Forbes's 'Supplementary Report on Meteorology,' given in the 'Report of the Tenth Meeting of the British Association for the Advancement of Science;' and the other consisting of a paper read before the Royal Astronomical Society, and published in the Monthly Notices of that Society. Although nothing occurs in either of these articles which I had not previously taken into consideration, yet, as I may appear in the former portion of this paper to have passed over some points with too slight a notice, I gladly avail myself of the opportunity thus afforded me of adding a few additional remarks.

The periodicity of the November and August meteors I have considered as granted: Professor Forbes does not appear to regard this point as established; but observes, "that we must use the term periodicity, as applied to these meteors, with caution*." That philosopher, however, does not seem to regard the aurora borealis as connected with, and occasionally appearing as a substitute for the phænomena of the shooting stars; the fact, however, is attested by his own observations in 1837, and by those of others in 1838. He further remarks, that no marked indication was observed in 1839 and 1840. At this place, in both those years, the nights of November 12th were most unfavourable for observation; that of November 12th, 1839, having been uniformly foggy; and that of November 12th, 1840, hazy and dull in the early part of the evening, and subsequently rainy. In 1836, the weather at this epoch was not more propitious; and in 1837, except in the early part of the evening (when the phænomenon before described was witnessed), the sky was overcast throughout the night, the moon being also at the full. On the 10th of August of the present year (1841) we had here a dull evening and a rainy night: on the 9th, however, the phænomenon of shooting stars was witnessed in far more than usual abundance; and among those I observed, although the larger proportion were direct, or moving from N.E. to S.W., three were retrograde, or moving from S.W. to N.E.; and the latter included a splendid meteor, much larger than Venus, which burst into sparks.

---

* The apparently periodical recurrence of the aurora borealis, which, as observed by Captain Beechey near Behring's Straits, made its first appearance for two successive years, 1831 and 1832, on the 26th of August, is not undeserving of notice. A splendid aurora was observed in this country by Mr. S. Hunter Christie on the 25th of August, 1837.—See Seventh Report of the British Association for the Advancement of Science.

The predominating direction of these meteors is another point to which, perhaps, some allusion ought to be made. The hypothesis of Chladni, and that which has been most generally adopted, consists, as stated by Mr. Galloway, "in supposing that, independently of the great planets, there exist in the planetary regions myriads of small bodies which circulate about the sun, generally in groups or zones, and that one of these zones intersects the ecliptic about the place through which the earth passes in November." Besides other difficulties attending this hypothesis, (none of which, it will be found, apply to the hints at explanation I have offered above,) Mr. Galloway remarks, "That bodies moving in groups in the circumstances supposed, must necessarily move in the same direction, and consequently, when they become visible from the earth, would all appear to emanate from one point and move towards the opposite. Now, although the observations seem to show that the predominating direction is from north-east to south-west, yet shooting stars are observed on the same nights to emanate from all points of the heavens, and to move in all possible directions." This difficulty, it will be evident, would not apply to the hypothesis I have ventured to adduce; and, indeed, if these meteors might be regarded as owing their origin to energetic electric currents, it would not be difficult to assign a supposititious reason for the general, although not invariable direction of these meteors contrary to the motion of the earth in its orbit, and also for their greater frequency in tropical regions. This, however, would embrace too wide a field to enter upon at present.

---

* Report of the British Association for the Advancement of Science, 1840, p. 119.

Since the former part of this paper was written, M. Quetelet's memoir, containing his *Catalogue des Principales Apparitions des Etoiles filantes* (which I had not previously seen), has been placed in my hands. From this it appears, that M. Quetelet, as well as other distinguished observers, concur with me in the opinion that the periodical shooting stars are connected with other meteorological phænomena. I also perceive that M. Quetelet cites some of the same instances adduced by me in support of this assumption. The additional facts given in my paper may, however, prove both interesting and confirmatory.

In addition to the periodical appearance of these meteoric phænomena on the 12th of November and the 10th of August, M. Quetelet has pointed out other epochs when similar meteoric phænomena have been observed, recurring with apparent periodicity. The epochs thus indicated are the 2nd of January; the 23rd or 24th of April; from the 15th to the 20th of June; the 18th of October; and the 6th or 7th of December. The increased number of the apparently periodical meteoric phænomena perhaps adds to the difficulties attending the hypothesis of a zone or zones intersecting the ecliptic. To a hypothesis attributing their origin to electric currents, the more frequent recurrence of such phænomena offers no difficulty whatever: for, if electric currents be occasionally thus energetically developed, there appears no reason to infer that this phænomenon may not repeatedly occur, whether periodically or otherwise, and not be restricted to one or two annual displays.

October 6, 1841. R. M. Z.

L. *An Analysis of the Atmosphere of some of the Cornish Mines.*

By M. P. Moyle, *Esq.*[*]

A PREMIUM having been offered for "the best analysis of the air taken at the termination of a *core* of two men, from the extremity of one mine level in granite, and of another in killas; the samples to be as fairly taken as possible, and in measure not less than one gallon each : it is desirable that the ends should be at least fifteen fathoms from any shaft or winse, not more than twenty fathoms above the deepest level of the mine, nor less than 100 fathoms below the adit, &c.," through the Royal Cornwall Polytechnic Society, and that premium having been awarded to the author of this communication, he has considered that an abridged extract from the essays published by that Society in their Transactions for 1839 and 1840, may be acceptable to the readers of the Philosophical Magazine.

---

[*] Communicated by the Author.

The analysis of gases, and more especially their combination or mixture in atmospheric air, being attended with no small difficulty of manipulation, (nor are the precautions requisite for obtaining accurate results of no mean consequence) it may be desirable to premise with a few observations on the different points of consideration so necessary for this purpose, and which are fully detailed in the essays themselves.

A due correction was made in all the following experiments, with the utmost care, for any change of temperature during the progress of the individual experiment.

The necessary allowance for the presence of vapour in the gas was always made according to Dr. Dalton's table. The unaccountable tendency or propensity which gases possess to unite, by overcoming obstacles placed for their security or separation, was never lost sight of; and no experiment was recorded which suffered sufficient interruption to allow of the above circumstance to transpire. In addition to this, the samples of air, although they gave perfect satisfaction as to the security of the vessels in which it was contained, were operated on with little or no loss of time. The pressure and temperature of the atmosphere were always reduced to one common standard, viz. barometer 30 inches, and thermometer 60°; and whenever the lower surface of the air in the jar did not coincide with the exact level of the surface of the liquid in the trough, the density of the included gas was reduced to that of the external atmosphere.

The utmost care was had in the manufacture of the oxygen, hydrogen, nitrous gas, &c. &c., that they might be of the utmost purity. The oxygen was uniformly made from the chlorate of potash in a green glass retort; and although the zinc of commerce was used for the manufacture of the hydrogen, it was invariably purified by passing it immediately before use through a solution of pure potash, for the purpose of removing any carburetted hydrogen or other impurity, and depriving it of any oxygen which it might contain, by subjecting it to the influence of the spongy platina ball, heated to redness; and sulphuretted hydrogen may be removed by the gas being afterwards passed through a solution of chlorate of lime. The nitrous gas or deutoxide of nitrogen was procured by the action of nitric acid in pure metallic copper, collecting the gas sometimes over water, and sometimes over mercury.

The quadrisulphuret of lime was manufactured after the manner recommended by Dr. Dalton.

The samples of air taken from the different loads in the mines were procured by emptying jars of water of their contents, and four-ounce bottles of mercury from one to the other, at the required spots, preserving a small portion of the water or mercury, greasing the corks, driving them as tight as possible, and afterwards sealing them over. Both jars and bottles were kept in an inverted position from this moment to that of their use; and a further proof of their security was generally had by an escape of a few bubbles of air on withdrawing the cork or stopper under water in the pneumatic trough.

The pneumatic trough was filled with perfectly clean rain water, collected from the glass roof of a green-house, and renewed as frequently as opportunities would permit.

Sample 1. One gallon.

Wheal Vor Mine.—Tin in slate. Taken from the 250-fathom level below the surface; fifteen fathoms west of any shaft or winse, and taken a few minutes after firing a hole, with two men in a core; specific gravity of air ·997.

	Oxygen.
Average of three experiments by exposing the gas to the influence of the quadrisulphuret of lime in Hope's eudiometer	18·41
Average of four experiments with pure hydrogen fired by electricity in Volta's eudiometer . . .	18·40
Average of four experiments with nitrous gas . .	18·45
One experiment with heated platina ball over mercury	18·40
Mean . . .	18·416

	Carb. acid.
Average of three experiments with lime-water . .	0·06
Average of three experiments with solution of pure potash	0·07
Mean . . .	00·65

No trace of any other gas, consequently the composition of this sample stands thus:—

Nitrogen	81·519	per cent.
Oxygen	18·416	...
Carbonic acid	0·065	...
	100·000	

Sample 2. One gallon.

Wheal Vor.—As before. 250 fathoms from surface; sixteen fathoms east of any shaft or winse; two men in a core; ten minutes after firing a hole; specific gravity ·993.

	Oxygen.
Average of six experiments with hydrogen	16·7
Average of four experiments with nitrous gas	16·62
Average of two experiments with platina ball	16·75
Mean . . .	16·69

	Carb. acid.
Average of three experiments with lime water . . .	0·075
Average of three experiments with alkaline solution	0·075
Mean . . .	0·075

Composition of this sample:—

Nitrogen	83·24	per cent.
Oxygen	16·69	...
Carbon. acid	0·075	...
	100·005	

Sample 3. One gallon.

Wheal Vor.—As before. 240 fathoms; twenty-four fathoms east of any shaft or winse; two men in a core; taken half an hour after firing a hole; specific gravity ·997.

	Oxygen.
Average of three experiments with hydrogen	19·2
Average of three experiments with nitrous gas	19·0
Average of two experiments by the slow combustion of phosphorus over mercury	18·6
One experiment with platina ball	19·0
Mean . . .	18·95

	Carb. acid.
Average of three experiments with lime water . . .	0·06
Average of three experiments with alkaline solution	0·07
Mean . . .	0·065

A sample of the above air taken in a four-ounce bottle secured by mercury, gave strong indications of both sulphuretted hydrogen and sulphurous acid: quantity not ascertained.

Composition of this sample:—

Nitrogen	80·98 per cent.
Oxygen	18·95 ...
Carbonic acid	0·065 ...
	99·995

Sample 4. One gallon.

Wheal Vor.—As before. 230 fathoms; two men in a core; 22 fathoms from shaft or winse, half an hour after firing a hole; specific gravity ·994.

	Oxygen.
Average of two experiments with hydrogen	17·22
Average of three experiments with nitrous gas	17·36
Average of three experiments with quadrisulphuret of lime	17·30
Average of three experiments with hydrogen on sample taken from bottle secured by mercury, as before	17·20
Average of two experiments from ditto with quadrisulphuret of lime	17·33
Mean	17·282

	Carb. acid.
Average of two experiments with lime water	0·08
Average of two experiments with alkaline solution	0·085
Mean	0·082

	Sulph. hydrogen.
Average of four experiments with a cup of nitric acid standing over mercury, with sample of air taken in a mercurial bottle	0·08 p. ct.

Also, a strong trace of sulphurous acid; therefore composition of this sample,

Nitrogen	82·556 per cent.
Oxygen	17·282 ...
Carbonic acid	0·082 ...
Sulphuretted hydrogen	0·080 ...
Sulphurous acid	strong trace
	100·000

Sample 6. A quart; also mercurial sample.

Wheal Vor, Penhale.—Slate. 36-fathom level; four men at work in a dead end; candle burned with difficulty; specific gravity 0·994.

	Oxygen.
Average of two experiments with hydrogen	14·74
Average of two experiments with nitrous gas	14·77

	Oxygen.
Average of two experiments with hydrogen from mercurial sample	14·77
Average of two experiments with nitrous gas from do.	14·77
Mean . . .	14·76

	Carb. acid.
Average of two experiments with potash solution . . .	0·23
Average of two experiments with potash solution from mercurial sample	0·23
Mean . . .	0·23

A trace of both sulphuretted hydrogen and sulphurous acid in both samples.

Composition of sample:—

Nitrogen	85·01	per cent.
Oxygen	14·76	...
Carbonic acid . .	0·23	...
	100·00	

Sample 7. One gallon.

Great Work.—Granite, tin and copper. 170 fathoms from surface; thirty fathoms from winse or shaft, with two men in a core; taken directly after firing a hole; specific gravity ·991.

	Oxygen.
Average of four experiments with hydrogen	16·99
Average of three experiments with nitrous gas	17·00
Average of three experiments with quadrisulphuret of lime	17·05
Experiment with platina ball	17·00
Mean . . .	17·01

	Carb. acid.
Average of three experiments with lime water	0·09
Average of two experiments with potass solution . . .	0·10
Mean . . .	0·095

Composition of sample:—

Nitrogen	82·895	per cent.
Oxygen	17·010	...
Carbonic acid . .	0·095	...
	100·000	

Sample 8. One gallon.

Great Work.—As before. 160 fathoms from surface; two men in a core; thirty fathoms from winse or shaft; fifteen minutes after firing a hole; specific gravity ·992.

	Oxygen.
Average of two experiments with hydrogen	15·1
Average of three experiments with nitrous gas . . .	15·2
Average of three experiments with quadrisulphuret of lime	15·15
Mean . . .	15·15

	Carb. acid.
Average of two experiments with lime water	0·14
Average of two experiments with solution of potash	0·15
Mean . . .	0·145

Composition of sample :—

Nitrogen	84·705	per cent.
Oxygen	15·15	...
Carbonic acid . . .	0·145	...
	100·000	

Sample 9. One quart.

Great Work.—As before. 150 fathoms from surface; two men in a core; twenty fathoms from winse or shaft, directly after firing a hole; specific gravity ·995.

	Oxygen.
Average of two experiments with hydrogen	16·30
Average of two experiments with nitrous gas . . .	16·40
Average of two experiments with quadrisulphuret of lime	16·47
Average of two experiments with hydrogen from mercurial sample	16·48
Average of two experiments with quadrisulphuret of lime from ditto	16·60
Mean . . .	16·45

	Carb. acid.
Average of two experiments with lime water	0·10
Average of two experiments with potash solution . .	00·98
Mean . . .	0·099

Composition of sample :—

Nitrogen	83·451	per cent.
Oxygen	16·45	...
Carbonic acid	0·099	...
	100·000	

Sample 10. One quart.

Great Work.—As before. 125 fathoms from surface; twenty-three fathoms from winse or shaft; two men in a core; half an hour after firing a hole; specific gravity 0·998.

	Oxygen.
Average of three experiments with nitrous gas . . .	17·56
Average of three experiments with hydrogen . . .	17·50
Average of three experiments with quadrisulphuret of lime	17·56
Average of two experiments with hydrogen in mercurial trough from a mercurial sample	17·54
Average of two experiments with nitrous gas in ditto from ditto	17·56
Mean . . .	17·544

	Carb. acid.
Average of three experiments with lime water . . .	0·10
Average of two experiments with potash solution . .	0·10
Mean . . .	0·10

A trace of both sulphuretted hydrogen and sulphurous acid was observable in the mercurial samples.

Composition of sample:—

Nitrogen	82·356	per cent.
Oxygen	17·544	...
Carbonic acid . . .	0·100	...
	100·000	

Sample 11. One gallon.

Binner Downs.—Copper mine; slate. 104-fathom level; eighteen fathoms from winse or shaft; two men in a core in a rise; specific gravity 0·996.

	Oxygen.
Average of four experiments with hydrogen	16·73
Average of two experiments with nitrous gas	16·80
Average of two experiments with quadrisulphuret of lime	16·77
Average of three experiments with hydrogen in mercurial trough from a mercurial bottle sample	16·76
Average of two experiments with nitrous gas in ditto from ditto	16·76
Mean . . .	16·764

	Carb. acid.
Average of five experiments with lime water and solution of potash	0·09

A strong trace of sulphuretted hydrogen in the mercurial sample. Composition of sample, therefore,

Nitrogen	83·146 per cent.
Oxygen	16·764 ...
Carbonic acid	0·090 ...
Sulphuretted hydrogen	strong trace.
	100·000

Sample 12. One gallon.

Carn Brea.—Copper mine, granite. Immediately after firing a hole; from the 105-fathom level from surface, and twenty-five fathoms from any winse or shaft; two men in a core; specific gravity ·994.

	Oxygen.
Average of two experiments with hydrogen	16·72
Average of three experiments with nitrous gas	16·66
Average of three experiments with quadrisulphuret of lime	16·70
Mean	16·693

	Carb. acid.
Average of four experiments with lime water and solution of potash	0·07

Composition of sample :—

Nitrogen	83·237 per cent.
Oxygen	16·693 ...
Carbonic acid	0·070 ...
	100·000

Sample 13. One gallon.

Carn Brea.—As before. 95-fathom level, machine blowing air; two men in a core; spot wrought only sixteen hours out of the twenty-four; sample taken three quarters of an hour after firing a hole; specific gravity ·992.

	Oxygen.
Average of three experiments with hydrogen	14·50
Average of two experiments with nitrous gas	14·64
Average of three experiments with quadrisulphuret of lime	14·40
Mean	14·51

	Carb. acid.
Average of three experiments with lime water	0·12
Average of three experiments with potash solution	0·14
Mean	0·13

Composition of sample :—

Nitrogen	85·36	per cent.
Oxygen	14·51	...
Carbonic acid	0·13	...
	100·00	

Sample 14. One quart.

Tresavean.—Copper mine, in granite. 156-fathom level; 208 fathoms from surface; two men in a core; directly after firing a hole; sixty-five fathoms from winse or shaft; specific gravity ·993.

	Oxygen.
Average of three experiments with hydrogen	16·20
Average of two experiments with nitrous gas	16·50
Mean	16·35

	Carb. acid.
Average of two experiments with lime water	0·13
Average of two experiments with potash solution	0·13
Mean	0·13

A slight trace of sulphuretted hydrogen.

Composition of sample:—

Nitrogen	83·52	per cent.
Oxygen	16·35	...
Carbonic acid	0·13	...
	100·00	

Sample 15. One gallon.

Wheal Ann.—Tin mine, in granite. A rise in the 80-fathom level; sixteen fathoms from any shaft or winse; two men in a core; no gunpowder used; specific gravity of air ·995.

	Oxygen.
Average of six experiments with hydrogen and quadrisulphuret of lime	16·72

	Carb. acid.
Average of four experiments with lime water and solution of potash	0·08

Composition of sample:—

Nitrogen	83·20	per cent.
Oxygen	16·72	...
Carbonic acid	0·08	...
	100·00	

Sample 16. One gallon.

Wheal Ann.—As before. 100-fathom level; forty-five fathoms from winse or shaft; machine and water-fall forcing air; end of a core of two men; no gunpowder used for some time; specific gravity ·997

	Oxygen.
Average of seven experiments with hydrogen, nitrous gas and quadrisulphuret of lime	18·22
	Carb. acid.
Average of five experiments with lime water	0·07

Composition of sample:—

Nitrogen	81·71	per cent.
Oxygen	18·22	...
Carbonic acid . . .	0·07	...
	100·00	

Sample of the above taken in a mercurial bottle gave the following means:—

Oxygen	18·34	per cent.
Carbonic acid . . .	0·07	...

Sample 17. Four gallons.

Consols.—Copper mine, in slate. No other report forwarded but that they were taken from the bottom levels. The proper directions having been forwarded for the taking of the samples, it is presumed the necessary precautions were adopted as they came to hand in an inverted position, &c. &c.; specific gravity ·997.

	Oxygen.
Average of four experiments with hydrogen	17·93
Average of two experiments with nitrous gas	17·82
Average of three experiments with quadrisulphuret of lime	17·88
Average of two experiments with phosphorus	17·60
Average of two experiments with platina ball	17·70
Mean . . .	17·78
	Carb. acid.
Average of three experiments with lime water	0·06
Average of three experiments with potash	0·06
Mean . . .	0·06

Composition of sample:—

Nitrogen	82·16	per cent.
Oxygen	17·78	...
Carbonic acid . . .	0·06	...
	100·00	

Sample 18. Four gallons.

Consols.—As before. Sample obtained under similar circumstances to the last; specific gravity ·997.

	Oxygen.
Average of three experiments with hydrogen . . .	18·41
Average of three experiments with quadrisulphuret of lime	18·41
Average of two experiments with nitrous gas . . .	18·45
Mean . . .	18·425

	Carb. acid.
Average of three experiments with lime water . . .	0·06
Average of two experiments with potash solution . .	0·07
Mean . . .	0·065

Composition of sample:—

Nitrogen	81·52	per cent.
Oxygen	18·42	...
Carbonic acid . . .	0·06	...
	100·00	

Summary of foregoing experiments:—

Average quantity of oxygen per cent.	17·067
Average quantity of carbonic acid	0·085
Average quantity of nitrogen	82·848
	100·000

The results of the foregoing experiments clearly demonstrate the impure state of the atmosphere which a miner is compelled to breathe for about one-third of the period of his existence (generally eight hours at a time out of the twenty-four). There cannot be a doubt, that where much gunpowder is used often pernicious gases are generated, the detection of which will not be easy in samples taken with water, as I have failed in almost every instance in finding a trace of such, by the nicest tests. That sulphurous acid is produced, there cannot be question, as it may be found invariably in the water preserved in the jars, when the samples have been taken immediately after the explosion of a hole, as well as in the air well preserved in a bottle by mercury, and very speedily experimented on in a mercurial trough.

The minutiæ of preparing the different tests, and the precautions adopted in the foregoing experiments, are fully detailed in the above-mentioned Transactions, and it is therefore considered unnecessary to recapitulate them here.

LI. *Remark on Primitive Radices. By the* Rev. R. MURPHY*.

HAVING had the honour of receiving from M. Jacobi his 'Canon Arithmeticus, sive tabulæ, quibus exhibentur pro singulis numeris primis, vel primorum potestatibus infra 1000, numeri ad datos indices et indices ad datos numeros pertinentes, Berolini, 1839,' I sought in the tables for some law relative to primitive radices. Being desirous of completing a work, entitled 'New Forms for the calculation of Logarithms,' which is nearly ready for the press, I could not bestow sufficient time to investigate the absolute generality of the following properties; yet even as empiric forms easily recollected, and verified in every case in those tables, and also in every case where I have taken small primitive radices, they will be found useful; at the same time the attention of mathematicians will, I hope, be drawn to the demonstration of the generality of these forms (or the contrary), though belonging to what is usually considered a dry subject, "The Theory of Numbers."

In page 6, we find a table, "Numeri primi, quorum 10 radix primitiva est," viz. 7, 17, 19, 23, 29, 47, 59, &c. to 2543; and the table from which this is deduced is (with some exceptions) carried as far as 9901. The following prime numbers, to which 10 is primitive radix, may be *culled* from those in Burckhardt's table, viz. 7, 17, 47, 97, 167, 257, 367 (497 is not prime), 647 (817 is not prime), (1007 is not prime), 1217, 1447, 1697 (1967 is not prime). Hence every prime number of the form $10n^2 + 7$ has 10 for a primitive radix, as far as can be verified by these tables.

As this property, if general, must depend on some relation between the numbers 7 and 10, I remarked that 7 was the only prime number less than 10, to which the latter was the primitive radix; accordingly, I tried in several instances the more general formula $an^2 + p$, where p is a prime number less than a number, a to which a is a primitive radix (as $3n^2 + 2$, $5n^2 + 3$, &c.); from the results it would appear to be a general property, that under those circumstances $an^2 + p$ $\left(\text{when prime, and } p \text{ greater than } \frac{a}{2}\right)$ will have a as a primitive radix. From the cause I have mentioned, I have not bestowed that attention to these properties I could have wished; the attention of others will now be called to them; in the mean time they may stand as empiric formulæ.

London, October 1841. ROBERT MURPHY.

---

* Communicated by the Author.

LII. *On the Perchlorate of the Oxide of Æthule, or Perchloric Æther.* *By* Clark Hare, *and* Martin H. Boye[*].

THE energetic properties of perchloric acid, and its stability, compared with the other compounds of chlorine with oxygen, led us to the belief that this acid might be combined with the substance which performs the part of a base in that class of organic salts which are generally designated by the name of *æthers*, and for which Berzelius, in consequence of his theoretical views, has adopted the name of oxide of æthule. For this purpose, a concentrated solution of perchlorate and sulphovinate of barytes, in equivalent proportions, was subjected to distillation. The sulphovinate of barytes may be considered as a double sulphate of barytes and the oxide of æthule; and we anticipated that, when heat was applied, a double decomposition would take place between the latter and the perchlorate of barytes. So long as the salts remained in solution no reaction occurred; but as soon as they became solid, in consequence of the distillation of the water, a reciprocal decomposition ensued, and a sweet æthereal liquid distilled into the receiver. *This liquid* is the *perchlorate of the oxide of æthule.*

As this substance is extremely explosive, in order to prepare it with safety it is necessary to operate on small quantities. We have employed from seventy to ninety grains of crystallized sulphovinate of barytes, with an equivalent proportion of perchlorate of barytes[†]; but we would recommend, especially on the first performance of the experiment, the employment of considerably smaller quantities. The salts should be intimately mixed in a mortar, and placed in a small retort attached to a refrigerator containing ice, and a receiver similarly cooled. The retort is to be heated in an oil-bath, in which a thermometer is suspended, so as to indicate the temperature. A wooden screen, furnished with openings covered with thick plate-glass at such intervals as to afford a full view of the different parts of the apparatus, should be erected in front of it, and strings passed round the screen and attached to a bar traversing on a pivot, and supporting an Argand spirit-lamp, by which heat is communicated to the oil-bath, so as to enable the flame of the lamp to be removed from or applied to the apparatus, according to the indications of the thermometer, without exposing the person of the operator. After the heat has reached 212° Fahr., below which the salts employed do not react on each other, it should be raised very gradually, and the distillation finished below 340° Fahr. Under these circumstances but little danger is to be apprehended from the retort; but the æther in the receiver must be treated with the greatest caution, since it has exploded in our hands in attempting to remove it with a pipette from the stratum of water which covers it. This water, therefore, should be removed by the cautious use of strips of blotting-paper, moistened at the end, and introduced into the tube employed as a receiver.

---

[*] From the Transactions of the American Philosophical Society, vol. viii.; having been read before the Society, December 4th, 1840.

[†] The amount of barytes in the perchlorate should be ascertained by an experiment, as it retains water with great tenacity. It may be worth while to mention, that the perchlorate of potassa cannot be substituted for the perchlorate of barytes, since the sulphovinate is decomposed without acting on it. We were equally unsuccessful in an attempt to procure the æther by the distillation of perchlorate of barytes and concentrated sulphovinic acid.

To avoid the danger attendant on the management of the æther in its pure state, it may be received in strong alcohol, since it is not explosive when dissolved in alcohol. If the experiment be performed with seventy grains of sulphate of barytes, from one to two drachms of absolute alcohol will be found sufficient for this purpose. By the addition of an equal volume of water, the æther may subsequently be separated from the solution in small quantities for the purpose of examination; but in this case a loss of æther is sustained by the decomposing influence of the water employed.

The perchlorate of æthule obtained in this way is a transparent, colourless liquid, possessing a peculiar, though agreeable smell, and a very sweet taste, which, on subsiding, leaves a biting impression on the tongue resembling that of the oil of cinnamon. It is heavier than water, through which it rapidly sinks. It explodes by ignition, friction, or percussion, and sometimes without any assignable cause. Its explosive properties may be shown, with but little danger, by pouring a small portion of the alcoholic solution into a small porcelain capsule, and adding an equal volume of water. The æther will collect in a drop at the bottom, and may be subsequently separated by pouring off the greater part of the water, and throwing the rest on a moistened filter, supported by a wire. After the water has drained off, the drop of æther remaining at the bottom of the filter may be exploded either by approaching it to an ignited body, or by the blow of a hammer. We are induced to believe, that in explosive violence it is not surpassed by any substance known in chemistry. By the explosion of the smallest drop, an open porcelain plate will be broken into fragments, and by that of a larger quantity, be reduced to powder. In consequence of the force with which it projects the minute fragments of any containing vessel in which it explodes, it is necessary that the operator should wear gloves, and a close mask, furnished with thick glass plates at the apertures for the eyes, and perform his manipulations with the intervention of a moveable wooden screen[*].

In common with other æthers, the perchlorate of æthule is insoluble in water, but soluble in alcohol; and its solution in the latter, when sufficiently dilute, burns entirely away without explosion. It may be kept for a length of time unchanged even when in contact with water: but the addition of this fluid, when employed to precipitate it from its alcoholic solution, causes it partially to be decomposed. Potassa, dissolved in alcohol, and added to the alcoholic solution, produces immediately an abundant precipitate of the perchlorate of that base; and, when added in sufficient quantity, decomposes the æther entirely. It would appear, therefore, impracticable to form either perchlorovinates or perchlorovinic acid.

We have subjected the perchlorate of æthule to the heat of boiling water with explosion or ebullition.

It may be observed, that this is the first æther formed by the combination of an inorganic acid containing more than three atoms of oxygen with the oxide of æthule, and that the chlorine and oxygen in the whole compound are just sufficient to form chlorohydric acid, water, and carbonic oxide with the hydrogen and carbon.

The existence of a compound of the oxide of æthule with an acid containing *seven* atoms of oxygen, led us to attempt to combine, by the same method, this base with nitric acid. For this purpose we subjected a mixture of sulphovinate and nitrate of barytes to the same treatment as described above; but the reaction, even when conducted with the greatest possible care, is destructive, hyponitrous æther and gaseous matters being the principal products obtained. Nor were we more successful in our attempts to procure a sulphurous or hyposulphuric æther by the same process.

LIII. *Note on the Theory of Light.* *By* Professor POWELL[†].

IN my recently published work 'On the Undulatory Theory as applied to the Dispersion of Light,' &c., I have given abstracts of the various investigations related to the main subject which have been pursued by several mathematicians, and especially by M. Cauchy, so far as they have come to my knowledge; so various, however, are the channels through which such researches are made public, that some of them, especially those which appear on the Continent, often escape attention. In this way I now find I have omitted to notice some of the later investigations of the distinguished mathematician just named.

---

[*] Having suffered severely, on several occasions, from the unexpected explosion of this substance, we would earnestly recommend the operator not to neglect the precautions mentioned above.

[†] Communicated by the Author.

M. Cauchy has favoured me with a letter, in which, acknowledging the receipt of a copy of my work, he adds a brief mention of these later investigations, to which I had not referred in my volume, with a request that I would take an opportunity of supplying my readers with this further information. As it may be *some time* before a work on abstract science reaches a second edition, perhaps the best course I can pursue is to offer a very brief statement of the points in question through the medium of this Journal.

It appears then, that subsequently to the researches of which I have given abstracts, M. Cauchy published in August 1836, at Budweiss, a lithographed memoir entitled *Mémoire sur la Théorie de la Lumière,* &c., which bears much on the subject of the general theory, and the phænomena to which it relates, as considered in my tract.

The main heads of this investigation are as follows:—

1. The general equations of motion of the æther: 2nd. Colour, or the dispersion: 3rd. The motion of light penetrating to a small depth into the interior of opake bodies: 4th. The transition from the formulæ obtained under the third head, to those which represent vibratory motion in general in an æthereal fluid: 5th and 6th. The case of media in which the propagation of light takes place in the same manner in every direction, whether round any point, or round every axis parallel to a given straight line: 7th. The propagation of plane waves in transparent bodies.

The consequences have been further followed out by the author in other memoirs published since.

In the second volume of the *Comptes Rendus,* 1836 (1st semestre), p. 365, M. Cauchy has observed that the intensity of the light penetrating to a small depth x in the interior of an opake body, decreases in proportion to a negative exponential of the form ε^{-cx}.

Exponentials of the same kind are found entering into many of the formulæ included in the memoir of August 1836; and setting out from these formulæ, he shows (p. 84 of that memoir) how in a coloured glass the thickness necessary to produce the extinction of a luminous ray may vary with the nature of the colour.

Among the formulæ of § 7. we may distinguish the equations (44.) (9.) and (51.), from which follow (as the author observes, p. 95) the laws of rectilinear, circular and elliptic polarization; and it may be remarked that the equation (9.), relative to a transparent medium, viz.

$$\zeta = \mathrm{A} \cos(k z - s t + \lambda) \quad \eta = \mathrm{B} \cos(k z - s t + \mu)$$
$$\zeta = \mathrm{C} \cos(k z - s t + \nu)$$

is deduced from the general equations which represent the infinitely small motions of a system of molecules that is from the equations (16.) of the memoir on Dispersion, for obtaining which the author has made no particular assumption, and has no occasion to make any term vanish. He also observes (p. 83) that we cannot in general reduce the equations (9.) to those which represent rectilinear polarization, and in which λ, μ, ν are equal.

The equations (16.) of the memoir on the Dispersion are the differential equations of motion in their most general form, corresponding to those of art. 59. in my tract.

The reader will immediately perceive the relation which these researches bear to the general subject. These last results accord exactly with mine obtained by a different method, as well as with those of the late Mr. Tovey, by whose recent death the science of this country has sustained so great a loss.

Oxford, Oct. 3, 1841.

LIV. *Results of some Experiments in Electricity and Magnetism.* By WILLIAM PETRIE, *Esq.**

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

BEING desirous of obtaining certain information relating to electricity and magnetism, which I failed to find in various works on these subjects at the time published, I was induced to make experiments, from which the following are the concise statements, showing the results, and which may, perhaps, possess sufficient interest to find a place in your Journal.

It is commonly stated, as by Lenz, that the conducting power of wires varies as the section, and inversely as the length; but this only refers to a particular case, namely, when the amount of electricity which the battery can circulate when the current is quite unimpeded (such as when the circuit is made with a very short thick wire), is very great compared with what does pass, on account of the great length and small section of the wire experimented with.

---

* Communicated by the Author.

Adopting, for brevity, the following notation :—

L = length of wire.

S = sectional area of wire.

n = a multiple, or simply a number.

Q = quantity of electricity which the battery can circulate, when the passage of the current is quite unimpeded; that is, the absolute power of the battery.

q = that portion of Q, which passes through the wire experimented with, when it is included in the circuit.

e = a constant, while L and S are constant; being 1, when such a measure is taken for Q, that, on Q being reduced to 1, $q = \frac{Q}{2}$.

My object has been to obtain the law of conduction, giving the values of q for all values of Q as well as of L and S.

In a wire of constant L and S, by varying Q, I find

$$q = \frac{e . Q}{e + Q} \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad \text{(formula A).}$$

The quantity (q) of electricity which (from a given Q) will pass through a length L, may be considered as a new *available quantity*, or Q, for passing through another piece of wire afterwards included in the circuit *. Hence for a wire of constant S and of $n \times$ the length of another, we have (from formula A) the new

$$q = \frac{e^n . Q}{e^n + n . e^{n-1} . Q}, \text{ or, the new } q = \frac{e . Q}{e + n . Q} \; . \; . \; . \; \text{(B}$$

When a copper wire having $S = \frac{1}{100}$ square inch, and $L = 58$ feet, completes the circuit of a battery in such action, that Q is equal to the electricity circulated by the combination of 0·1 grain of pure zinc per second, then q was found to be $\frac{1}{2}$ Q, or the electricity arising from the combination of 0·05 grain of zinc per second.

Now by applying this numerical datum, and the fact that increasing S and Q together increases q in the same ratio, by applying this to the above formula B, and reckoning L in feet, S in square inches or fractional parts of a square inch, and representing Q and q by grains of zinc used per second, we may deduce the following formulæ :—

$$L = 580 . S . \frac{Q - q}{Q . q},$$

---

* This reasoning will equally apply although the electric influence be propagated by vibrations in any medium, or even by any simultaneous affection or change of state in the particles of the conducting substance, but the convenient analogy of a current renders the subject much clearer.

$$S = \frac{L}{580} \cdot \frac{Q \cdot q}{Q - q},$$

$$Q = 580 \cdot S \cdot \frac{q}{580 \cdot S - L \cdot q},$$

$$q = 580 \cdot S \cdot \frac{Q}{580 \cdot S + L \cdot Q}\ *$$

which appears to accord with some results arrived at by Ohm, which I have subsequently seen; though he has not deduced formulæ of quite the same nature.

These formulæ are useful to solve many practical questions, such for instance as relating to the size of conducting wires, and power of batteries necessary to transmit a given quantity of electricity through a given distance, as in telegraphs; and in determining the ratio of the section to the length of helix wires in forming electro-magnets of various dimensions and proportions, so as to obtain a maximum of effect, which depends chiefly on the number of coils × the quantity of electricity in circulation.

The constant 580 in the formulæ applies only to copper wire unannealed; to adapt the formulæ to different metals we must vary the constant in the direct ratio of their conducting powers. The above calculations and experiments refer only to electricity of the intensity of a single cell of Daniell's constant battery. It is probable (judging from an experiment with electricity of *two* cells' intensity) that an increase in the intensity has the same effect on the transmission of the electricity, as a proportionate increase in the conducting power of the wire, as Ohm had previously deduced.

The ordinary form of galvanometer, by the deflection of the needle in a helix placed so as to be in the magnetic meridian, cannot determine the comparative amounts of two currents of electricity by any calculation from the degrees of deflection, unless previous data be obtained by other experiments, as the amount of deflective force exerted by any given current at different arcs of deflection (when balanced by terrestrial magnetism) is not as the tangents of these arcs, as would be the case if the deflective action of the helix on the poles of the needle were constant, for all positions of the needle, in amount as well as direction; but depends on the shape of the helix and the proportional length of the magnet placed within; because the action depends on the law, that *every portion of an electric current constantly tends to deflect a magnetic pole, in a direction at a right angle to the current itself, and also at a right angle to the straight line from that pole to the current, and with a force inversely as the square of the distance of the pole from that portion of the current.*

---

* These formulæ were tested by the following experiments:—A length of copper wire (220 feet) was led about a room, double, that is, the returning half running close and parallel to the other part of the wire without touching: the doubled wire dipped into pairs of mercury cups at certain intervals; any of these could be connected or disconnected in an instant, by inserting or removing a bit of connecting wire, and thus the current was transmitted through various lengths of wire successively. The power of the battery was also varied by increasing the number of united cells, or diminishing the strength of the acid. A magnet galvanometer, included in the circuit, showed the quantity of electricity passing, either through the wire or without it.

In the case of a square helix and a magnet nine-tenths of the average exterior and interior length of a side of the helix, the tangential force arising from the deflection by the helix varies with the angle of deflection, as here stated.

Degrees of the arc	0°	5°	10°	20°	30°	45°	70°	90°
Tangential de-flective force	1·	0·74	0·56	0·36	0·236	0·112	0·02	0·00

I found a useful and accurate form of galvanometer for measuring and comparing the power of galvanic batteries, or currents which are not very feeble, to be, a helix and a thin flat magnet, so placed on an axis as to be deflected vertically when a current is transmitted, and weights applied in any way so as to retain the magnet in a horizontal position: the weights should act not far from the axis, so as to make the deflecting force more sensible by its greater leverage. The helix wire should be thick and as short as possible; three coils are enough, so as not to impede the current to be measured.

One cell on Daniell's constant principle, one foot high, and two inches and a half broad, with a zinc rod inserted nine inches in the liquid, and charged with a strong solution of sulphate of copper, and 7 × diluted ordinary sulphuric acid, at about 65° of Fahrenheit, and with the circuit quite completed, evolves a quantity of electricity due to the consumption of 0·04 grain of zinc per second. For cells of such a size, cylinders of wood prepared as in Mullins's battery can easily be turned less than one-tenth of an inch thick and ten inches long; they do not appear to impede the current much more than the gut used by Daniell, and are more durable and easy to manage.

When the cells are near 32° temperature the sulphate of zinc as it is formed consolidates on the zinc rod; although the zinc be well amalgamated and quite pure, and the solution quite fresh, the electric current acts by starts instead of being perfectly uniform, as it is at 60° or even at 50°.

Experiments on the Shape of Magnets.

Eight steel bars were made of nearly the same hardness and quality, being six inches long, half an inch broad, and of various thicknesses, from 0·5 inch to 0·02; they were all magnetized by the single touch, being repeated until they would become no stronger; the power was measured by the force exerted at the extremity to turn into the magnetic meridian, when placed at right angles to it; this force, which we will call *fn*, was ascertained by calculation from the formula

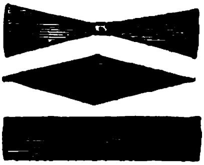
$$fn = W \cdot \frac{N^2 L}{824{\cdot}000}. \left\{\begin{array}{l} \text{W being the magnet's weight.} \\ \text{N its number of single vibrations per minute.} \\ \text{L the total length of the magnet in inches.} \end{array}\right.$$

The *fn* of all these magnets was the *same*, within the errors of experiment. Similar experiments were made with steel bars of 18, 6 and 2 inches in length, all being 0·5 inch broad; the *fn* of each was in the *exact ratio of their length*, as was partially ascertained by Captain Kater[*] and Coulomb.

Similar experiments were made with bars of 3, 0·5 and 0·25 inches in breadth, all being six inches long; the ratio of their *fn* was nearest that of the *square root of the breadth* (the smallest being too strong and the largest too weak; probably the ratio of the square roots would have been correct had each been rubbed with a magnet of corresponding breadth).

Similar experiments were made with bars of 0·5, 0·25 and 0·15 inch square, all being six inches long; the ratio of their *fn* was between that of the square and cube root of the breadth of their sides, being nearest the square root.

The relative *fn* of magnets of the accompanying forms, is as follows: their length was four inches, and breadth 0·8.



- - - 4	They were magnetized in the direction of the shade lines.
- - - 7	
- - - 12	

Effect of approximating Magnets.

Two bar magnets, six inches long, 0·5 inch broad, and 0·05 inch thick, were placed parallel with similar poles adjacent flatwise, one over the other at various distances, and their magnetic power ascertained by vibrations, according to the preceding formula; their power diminished as they approached, in the ratio here shown:—

---

[*] [Capt. Kater's paper on this subject will be found in Phil. Mag., vol. lix. p. 359.—Edit.]

Distance of magnets apart.	Magnetic power.
inches.	*f n.*
Infinite	1·
3·	0·970
1·	0·907
0·8	0·885
0·5	0·844
0·4	0·825
0·3	0·798
0·2	0·765
0·15	0·74
0·1	0·725
0·05	0·7.

But the loss was only temporary, as they recovered their former power immediately that they were separated. [Their distances apart are reckoned from the middle of the thickness of each; so that when they are said to be at a distance 0·05, they are in actual contact.]

A number of hardened steel sheet magnets, 8 inches long, 0·8 wide, and about 0·03 thick, were placed in a light framework of wood, one over another at equal distances, as in the preceding experiment, and the whole mass slung by a thread of parallel silk fibres, and the magnetic power ascertained by vibrations; this pile of magnets was kept one foot high by using fewer magnets when they were placed further apart*. The power of each individual magnet was diminished as they approached, in the ratio here shown:—

Distance of magnets apart.	Magnetic power.
inch.	*f n.*
Infinite	1·
1·	0·95
0·8	0·93
0·5	0·83
0·4	0·78
0·3	0·7
0·2	0·6
0·15	0·52
0·1	0·42
0·05	0·28 †.

---

[*] Only forty-eight magnets were used; therefore, when they were placed very close, the pile could not be made one foot high: but the effect of increasing the height of the pile, the distance between the magnets remaining the same, was ascertained; and so the effect of a pile one foot high was calculated from experiments with one of a less height.

[†] The results given in the second column, are always comparative with the aggregate power of the same magnets when placed far apart.

But it is very trying to have two-thirds of the magnetic power *temporarily* suppressed by mutual proximity, and they would probably soon lose permanently a portion of it by any slight concussion.

The experiments were gone over three times, and the mean results taken. The magnets were placed at their closest distance before beginning, so that further permanent loss of magnetism might not take place during the experiments. I believe the third place of decimals in the first table cannot be trusted.

From the foregoing table and observation following, it appears that power is not gained by too great an approximation of plates of a compound magnet, and the thinner the plates the better.

W. Petrie.

LV. *On a Theorem given in the Philosophical Magazine for August.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE "new theorem" which is applied in the August Number of your Magazine to determine the development of cos $n\theta$ in descending powers of cos θ, may be at once deduced from one of the fundamental formulæ in finite differences, as follows:—the formula referred to is

$$\Delta^n u_x = u_{x+nh} - n u_{x+\overline{n-1}h} + \frac{n\,\overline{n-1}}{1\,.\,2} u_{x+\overline{n-2}h} - \&c.$$

$$\text{or } (-1)^n \Delta^n u_x = u_x - n u_{x+h} + \frac{n\,\overline{n-1}}{1\,.\,2} u_{x+2h} - \&c.,$$

where u_x is any function of x, and $h = \Delta x$.

Now let u_x be of m dimensions in x, and $= (a-x)(b-x)(c-x) \ldots (l-x)$, then it is evident, that if $n > m$, $\Delta^n u_x = 0$

$$\therefore\ 0 = u_x - n u_{x+h} + \frac{n\,\overline{n-1}}{1\,.\,2} . u_{x+2h} - \&c.,$$

In this equation put $x = 0$,

$$\therefore\ 0 = u_0 - n u_h + \frac{n\,\overline{n-1}}{1\,.\,2} . u_{2h} - \&c.,$$

which is the theorem to be proved.

With respect to the use of this theorem in expanding cos $n\theta$

according to descending powers of cos θ (where n is a positive integer), it seems extremely doubtful whether anything would be gained by its introduction in an elementary work; the method given in the ordinary treatises on trigonometry depending on the logarithmic series, is on every account preferable.

In the same article in your Number for August there is given a proof that $\Delta^n x^m = 0$ when $m < n$.

If the author of it will only turn to art. 885 in Lacroix, I am convinced that he will regret having offered his new demonstration. Believe me, yours sincerely,

Trinity College, Cambridge, J. E.
Aug. 24, 1841.

LVI. *On a Mode of deducing the Equation of* Fresnel's *Wave. By* Sir William Rowan Hamilton, *LL.D., P.R.I.A., Member of several Scientific Societies at Home and Abroad, Professor of Astronomy in the University of Dublin, and Royal Astronomer of Ireland**.

THE following does not pretend to be the *best*, but merely to be *one* way, of deducing the known equation of Fresnel's wave, from a known geometrical construction. It requires only the first principles of the application of algebra to the geometry of three dimensions, and does not introduce any of the geometrical properties of the auxiliary ellipsoid employed, except those which are immediately suggested by the equation of that ellipsoid. It has, therefore, in the algebraical point of view, a certain degree of directness, although it might be rendered easier and shorter by borrowing more largely from geometry.

1. The known construction referred to is thus enunciated by Sir John Herschel, in his Treatise on Light, *Encyclopædia Metropolitana*, article 1017. "M. Fresnel gives the following simple construction for the curve surface bounding the wave in the case of unequal axes, which establishes an immediate relation between the length and direction of its radii. Conceive an ellipsoid having the same semiaxes a, b, c; and having cut it by any diametral plane, draw perpendicular to this plane from the centre two lines, one equal to the greatest, and the other to the least radius vector of the section. The loci of the extremities of these perpendiculars will be the surfaces of the ordinary and extraordinary waves."

2. The coordinates of the wave being $x\ y\ z$, and those of the ellipsoid X Y Z, we have the six equations,

---

* Communicated by the Author.

$$\frac{X^2}{a^2} + \frac{Y^2}{b^2} + \frac{Z^2}{c^2} = 1, \quad . \; . \; . \; . \quad (1.)$$

$$\frac{X\,dX}{a^2} + \frac{Y\,dY}{b^2} + \frac{Z\,dZ}{c^2} = 0, \quad . \; . \quad (2.)$$

$$x\,X + y\,Y + z\,Z = 0, \quad . \; . \; . \; . \; . \quad (3.)$$

$$x\,dX + y\,dY + z\,dZ = 0, \quad . \; . \; . \quad (4.)$$

$$X\,dX + Y\,dY + Z\,dZ = 0, \quad . \; . \; . \quad (5.)$$

$$x^2 + y^2 + z^2 = X^2 + Y^2 + Z^2; \quad . \; . \; . \; . \quad (6.)$$

between which we are to eliminate X, Y, Z, and the ratios of their differentials.

3. The equations (1.) and (2.) are satisfied by assuming

$$X = a \sin\theta \cos\phi, \quad Y = b \sin\theta \sin\phi, \quad Z = c \cos\theta; \quad (7.)$$

and then the equation (3.) gives

$$\tan\theta = \frac{-c\,z}{a\,x \cos\phi + b\,y \sin\phi}; \quad . \; . \; . \; . \quad (8.)$$

while the comparison of the two values of $\tan\theta \frac{d\phi}{d\theta}$, deduced from (4.) and (5.), gives

$$\frac{a\,x \cos\phi + b\,y \sin\phi - c\,z \tan\theta}{a\,x \sin\phi - b\,y \cos\phi} = \frac{a^2 \cos\phi^2 + b^2 \sin\phi^2 - c^2}{(a^2 - b^2) \sin\phi \cos\phi}; \quad (9.)$$

and the equation (6.) becomes

$$(x^2 + y^2 + z^2)(1 + \tan\theta^2) = (a^2 \cos\phi^2 + b^2 \sin\phi^2) \tan\theta^2 + c^2. \quad (10.)$$

It remains therefore to eliminate θ and ϕ between the three equations (8.) (9.) (10.).

4. Substituting for $\tan\theta$, in (9.) and (10.), its value given by (8.), we easily obtain

$$A \tan\phi + B \operatorname{cotan}\phi = C; \quad . \; . \; . \; . \; . \; . \quad (I.)$$

$$A' \tan\phi + B' \operatorname{cotan}\phi = C'; \quad . \; . \; . \; . \; . \; . \quad (II.)$$

if we put for abridgement

$$A = (c^2 - b^2)\, a\,b\,x\,y;$$
$$B = (a^2 - c^2)\, a\,b\,x\,y;$$
$$C = (b^2 - c^2)\, a^2 x^2 + (c^2 - a^2)\, b^2 y^2 + (b^2 - a^2)\, c^2 z^2;$$
$$r^2 = x^2 + y^2 + z^2;$$
$$A' = r^2 (b^2 y^2 + c^2 z^2) - c^2 b^2 (y^2 + z^2);$$
$$B' = r^2 (a^2 x^2 + c^2 z^2) - c^2 a^2 (x^2 + z^2);$$
$$C' = -2 (r^2 - c^2)\, a\,b\,x\,y.$$

And eliminating ϕ between the equations (I.) and (II.), we find:

$$(A B' - A' B)^2 + (A C' - A' C)(B C' - B' C) = 0; \quad (III.)$$

a form for the equation of the wave, which we have now only to develope and depress.

5. Expanding it first under the form

$$W_8 + W_{10} + W_{12} = 0,$$

in which W_8, W_{10}, W_{12} are, respectively, homogeneous functions of x, y, z, of the 8th, 10th, and 12th dimensions, we soon discover that these three functions have a common factor, of the 8th dimension, namely, $c^2 z^2 r^2 R^2$, in which

$$R^2 = C^2 + 4\, a^2 b^2 (c^2 - a^2)\, (c^2 - b^2)\, x^2 y^2,$$

C having the same meaning as in (I.), so that

$$R^2 > 0, \text{ if } c^2 > b^2 > a^2, \text{ or if } c^2 < b^2 < a^2,$$

conditions which we may suppose to be satisfied. And rejecting, as evidently foreign to the question, this common factor $c^2 x^2 r^2 R^2$, the known equation of the wave results, under the form

$$u_0 + u_2 + u_4 = 0, \quad \ldots\ldots\ldots \quad \text{(IV.)}$$

in which

$$u_0 = a^2 b^2 c^2,$$

$$u_1 = -\{a^2 (b^2 + c^2)\, x^2 + b^2 (c^2 + a^2)\, y^2 + c^2 (a^2 + b^2)\, z^2\},$$

$$u_2 = (x^2 + y^2 + z^2)\, (a^2 x^2 + b^2 y^2 + c^2 z^2).$$

6. The foregoing investigation is taken from a manuscript Report which I had the honour of drawing up in July 1830, when, in conjunction with the late and present Provosts of Trinity College, Dublin, I was appointed to examine the first communication of Professor MacCullagh to the Royal Irish Academy, since published in the second part of the sixteenth volume of the Transactions of that body. A far more concise and elegant deduction of the same known equation of the wave from the same geometrical construction, depending, however, a little more on the geometrical properties of the ellipsoid, has since been communicated by Professor MacCullagh himself, and is published in the second part of the seventeenth volume of the Transactions of the same Academy. Others have published other demonstrations.

My own mode of deducing the equation of the wave from the principles of Fresnel, without any reference to the ellipsoid above referred to, may be seen in the 'Third Supplement' to my Theory of Systems of Rays contained in the first part of the last-mentioned volume.

Observatory of Trinity College, Dublin,
October 13, 1841.

P.S. Since writing out and sending off the foregoing paper, I have had opportunity to refer to Fresnel's own deduction of the same equation of his wave from the same geometrical construction, entitled "Calcul très simple qui conduit de l'équation d'un ellipsoide à celle de la surface des ondes." (*Mém. de l'Acad. des Sci. de l'Inst. Royale de France*, tom. vii., page 137.) It is much simpler than mine, and nearly coincident with that of Professor MacCullagh, but seems to have been overlooked by both of us.

LVII. *Remarks on* M. Mossotti's *Theory of Molecular Action*[*].

SOME years since a paper by M. Mossotti, on the forces which regulate the internal constitution of bodies, attracted a good deal of attention among scientific men. A translation of it appeared in the third part of the 'Scientific Memoirs.' The author's object was to develope the views of Franklin and Æpinus with respect to electricity. It is well known, that in the form which the latter of these philosophers gave to the single fluid hypothesis, a mutual repulsion is supposed to exist among the material molecules of which bodies are composed, as well as among the particles of the electrical æther; while these particles are supposed to be attracted by the material molecules. Assuming that all the forces required by the hypothesis vary directly as the mass and inversely as the square of the distance, is it possible to establish stable equilibrium among the molecules of which bodies are composed? This is the question which M. Mossotti proposed to determine.

A priori, stable equilibrium certainly appears to be possible; for, as the author remarks, if the molecules of matter surrounded by their atmospheres attract each other when at a distance, and exercise a repulsive force when brought nearer together, there must be some intermediate position of stable equilibrium. But although M. Mossotti's general view may be correct, I believe it will be found that his analysis is erroneous. As the subject of molecular attraction possesses at the present day a good deal of interest, I trust that I may be permitted to develope at some little length my objections to M. Mossotti's investigation. For this purpose it will be necessary to follow his analysis as far as the reduction of the problem to equations.

Let f be the coefficient of the repulsion between any two atoms of the æther; g the coefficient of the attraction between an atom of the æther and a material molecule; and γ the coefficient of the repulsion between two material molecules.

Again, let q be the density of the æther at a point $x\ y\ z$, and ϵ the elastic force or pressure at the same point, referred to the superficial unit. Let ϖ be the density at a point $\xi\ \eta\ \zeta$ of a molecule supposed to be of certain though very small dimensions.

All the forces of the system may be represented by the partial differential coefficients of the following definite integrals:—

---

[*] Communicated by the Author.

$$\mathrm{F} = \iiint \frac{f q' \, d x' \, d y' \, d z'}{\{(x'-x)^2 + (y'-y)^2 + (z'-z)^2\}^{\frac{1}{2}}},$$

$$\mathrm{G} = \iiint \frac{g \pi \, d \xi \, d \eta \, d \zeta}{\{(\xi - x)^2 + (\eta - y)^2 + (\zeta - z)^2\}^{\frac{1}{2}}},$$

$$\Phi = \iiint \frac{g q' \, d x' \, d y' \, d z'}{\{(x' - \xi)^2 + (y' - \eta)^2 + (z' - \zeta)^2\}^{\frac{1}{2}}},$$

$$\Gamma_\gamma = \iiint \frac{\gamma \pi_\gamma \, d \xi_\gamma \, d \eta_\gamma \, d \zeta_\gamma}{\{(\xi_\gamma - \xi)^2 + (\eta_\gamma - \eta)^2 + (\zeta_\gamma - \zeta)^2\}^{\frac{1}{2}}}$$

(γ subscript refers to the 5th molecule).

These integrals being taken between suitable limits, we have for the equilibrium of the æther the equations

$$\left.\begin{aligned}
\frac{d \varepsilon}{d x} &= - q \frac{d \mathrm{F}}{d x} + q \Sigma \frac{d \mathrm{G}}{d x} \\
\frac{d \varepsilon}{d y} &= - q \frac{d \mathrm{F}}{d y} + q \Sigma \frac{d \mathrm{G}}{d y} \\
\frac{d \varepsilon}{d z} &= - q \frac{d \mathrm{F}}{d z} + q \Sigma \frac{d \mathrm{G}}{d z}
\end{aligned}\right\} ; \quad \dots\dots\dots\dots \quad (1.)$$

and for that of the centre of gravity of a molecule,

$$\left.\begin{aligned}
\iint \varepsilon \, d \eta \, d \zeta &= \iiint \pi \frac{d \Phi}{d \xi} \, d \xi \, d \eta \, d \zeta \\
&\quad - \Sigma \iiint \pi \frac{d \Gamma_\gamma}{d \xi} \, d \xi \, d \eta \, d \zeta \\
\iint \varepsilon \, d \xi \, d \zeta &= \iiint \pi \frac{d \Phi}{d \eta} \, d \xi \, d \eta \, d \zeta \\
&\quad - \Sigma \iiint \pi \frac{d \Gamma_\gamma}{d \eta} \, d \xi \, d \eta \, d \zeta \\
\iint \varepsilon \, d \xi \, d \eta &= \iiint \pi \frac{d \Phi}{d \zeta} \, d \xi \, d \eta \, d \zeta \\
&\quad - \Sigma \iiint \pi \frac{d \Gamma_\gamma}{d \zeta} \, d \xi \, d \eta \, d \zeta
\end{aligned}\right\} \quad \dots\dots\dots \quad (2.)$$

In both these systems of equations Σ refers to the various molecules whose equilibrium we are considering; in (1.) it extends to all the molecules; in (2.) to all but one.

The double integrals in (2.) are to be extended to the whole surface of the molecule under consideration, and the triple integrals to its whole volume. Such are M. Mossotti's equations. Let us begin by examining the meaning of the several terms of those of the second system. The last terms of these three equations represent the total actions (respectively parallel to the axes of co-ordinates) exerted by all the molecules of the system but one, upon that one. Consequently the other six terms must refer to the action of the æther upon the molecule under consideration. Accordingly the second terms express the attraction (according to the assumed law of the inverse square of the distance) exerted by the whole body of æther upon the molecule in question. But this, according to the hypothesis, comprises the whole action of the æther upon the molecule. What then do the first terms represent? They represent the components parallel to the axes of the total pressure exerted by the æther over the surface of the molecule. Hence it appears that the idea of fluid pressure enters into the formation of our equations. But, on the molecular hypothesis of the constitution of bodies, what is fluid pressure but a form of molecular action? Hence the first terms of our equations, as well as the second, refer to molecular action between the æther and the material particles. We are thus reduced to the following alternative:—either the molecular action expressed in the first terms is included in that expressed in the second, in which case the first terms are superfluous, and the equations are incorrect; or, beside the forces given in the hypothesis, we must take into account certain other forces, whose nature is wholly unknown, namely, those by which the pressure on the molecules is produced. As the object of the hypothesis from which we set out is to explain the nature of molecular action, it is clear that we are not at liberty to introduce the agency of unknown molecular forces. Thus the only conclusion we can adopt is that M. Mossotti's second system of equations is not in accordance with his hypothesis, and that the latter does not admit of being modified so as to produce an agreement. Let us now examine the first system of equations, those, namely, which refer to the equilibrium of the æther. They are, in reality, only the ordinary equations of fluid equilibrium, which are generally put in the following form:—

$$\frac{dp}{dx} = \rho \mathrm{X} \quad \frac{dp}{dy} = \rho \mathrm{Y} \quad \frac{dp}{dz} = \rho \mathrm{Z} \ldots\ldots\ldots\ldots\ldots\ldots \quad (3.)$$

where p is what M. Mossotti denotes by ε; ρ is equivalent to his q, and X, Y, Z, are the forces impressed upon the fluid. If we consider with some degree of attention the signification of these equations, we shall find that they express the conditions of the equilibrium of an element of the fluid, taken as a continuous mass. The circumstances under which we are justified in considering a congeries of molecules as a continuous mass, are well known, and need not here be discussed. To return, the equilibrium of the element $d\,x\,d\,y\,d\,z$ of the fluid is maintained by the respective equality of the impressed moving forces $X\rho\,d\,x\,d\,y\,d\,z$, $Y\,\rho\,d\,x\,d\,y\,d\,z$, $Z\,\rho\,d\,x\,d\,y\,d\,z$, to the differences of pressure over the opposite faces of the element, or to $\frac{d\,p}{d\,x}\,d\,x\,.\,d\,y\,d\,z$; $\frac{d\,p}{d\,y}\,d\,y\,.\,d\,z\,d\,x$; and $\frac{d\,p}{d\,z}\,d\,z\,.\,d\,x\,d\,y$. Now this pressure is nothing but the result of molecular action between contiguous particles. Hence it is clear that the expressions X. Y. Z do not include all the forces which act upon the particles of the fluid: they exclude precisely the molecular forces.

But if we examine the terms on the right-hand side of (1.), we shall find that they include *all* the forces, of whatever kind, which act upon the particles of the æther. The conclusion from this is obvious, and coincides with that which we derived from the consideration of (2.).

In fact the errors in both cases arise from the same source, namely, that after considering explicitly all the forces of the system, the author introduces the idea of fluid pressure, itself the result of a particular mode of molecular action.

If this were a question to be decided by a reference to authority, we might remark, that in the various memoirs which have of late years been published on the molecular view of the undulatory theory of light, the luminiferous æther is considered simply as a congeries of molecules; and further, that where, as in a memoir by Poisson, in those of the Institute for 1830, and in some of Cauchy's, the pressure of fluids or the tension of solids is introduced in connexion with molecular action, they appear as results derived from it, and not as co-ordinate principles.

The preceding observations contain little that is not familiar to most of those who take any interest in mathematical physics, but perhaps the celebrity of M. Mossotti's memoir may give them some degree of interest, and at any rate they may serve to point out the necessity of distinct views of the connexion between the theory of molecular action and the ordinary principles of equilibrium.

R. L. E.

September 24, 1841.

LVIII. *Some Observations on* Dr. Brett *and* Mr. Smith's *Experiments on the alleged Conversion of Carbon into Silicon*[*]. *By* Dr. SAMUEL M. BROWN.

To Richard Phillips, Esq.

SIR,

AS Dr. Brett and Mr. Smith have made the Philosophical Magazine the medium of publication for their "Experiments on the alleged Conversion of Carbon into Silicon," I request you to do me the honour of giving the same publicity to these few and brief observations on their letter, while I gladly acknowledge the liberal style of investigation in which the gentlemen in question have undertaken and executed their task.

Your Correspondents specify, on the very threshhold, that the "paracyanogen employed by them was produced by the decomposition of hydrocyanic acid;" and this is sufficient to render almost all their experiments of no value. I do not, indeed, understand the statement. If it be meant that their paracyanogen was prepared by the spontaneous decomposition of anhydrous hydrocyanic acid, they must prove that paracyanogen can be so produced at all; for M. Gay-Lussac found that hydrocyanic acid is wholly resolved into ammonia and a brownish-black matter, inferred to be "un azoture de carbone," which cannot be paracyanogen if ammonia be the only other product. If, again, their paracyanogen were a product of the decomposition of aqueous hydrocyanic acid, I am ready to show that it was a low hydrate of paracyanogen, which I never made any experiments upon; but in such a case the burden of proof lies on them. Moreover, howsoever the subject of their experiments was prepared, (and in a matter affecting so vital a truth or untruth as this is they ought surely to have been more specific,) if it were not prepared exactly in the same way as my own, their results can have no other relation to mine than that of parallel lines, incapable of meeting and merging indeed, but by no means diverging and contradictory. Accordingly, nothing can be said of their experiments till, at the middle of p. 299, they determine to avoid every objection by employing paracyanogen prepared from bicyanide of mercury, and make one with what seems to be the substance I used; one experiment.

Here I find no sufficient evidence in the single sentence, in which the authors describe the preliminary process and its product, that the former was rightly performed, or that the latter was true paracyanogen; and I do know that this process is so difficult of performance, that it was not till after long practice that my assistants, Messrs. Craig and Thomas Brown, acquired facility in working it. Solubility in sulphuric acid is no test; blackened half-charred cyanide of mercury, paracyanide of mercury, and most ex-organic carbonaceous substances, are all dissolved by that menstruum as well as paracyanogen. It appears that "the residue was black and coherent;" it should have been extremely incoherent. I do not think Mr. Smith employed the same material as I used in this experiment, which is the only attempt at a literal repetition of any of mine in the whole list on free paracyanogen. The paracyanogen I worked with was all prepared in the hammer-iron paracyanogen-tube, which is described in my paper on the production of silicon. Will neither Dr. Brett nor Mr. Smith get one made?

---

[*] Philosophical Magazine, Oct. 1841, p. 295.

It appears that the next set of experiments was "On the formation of compounds of silicon with copper, iron and platinum by the reaction of these metals on paracyanogen" (p. 300); but they were made with paracyanogen to which I have already excepted, and that is enough. In addition to that quite fatal objection, the first is equal to nothing according to Dr. Brett himself; the second and third, by Mr. Smith, are not performed according to my directions; and in all the three the investigation is far from being rigid. May I be allowed to state my respectful conviction, that the authors have not striven as they might? My experiments cost me eighteen months' incessant labour and many a failure. Even so late as last November and December I tried to effect the alleged transformation before a celebrated physician and chemist six weeks in vain; but at last succeeded, or, as I must say now, appeared to succeed.

For the next experiments (p. 301), on the ferrocyanide of potassium, let such as are interested in the subject judge whether they be sufficient repetitions of mine. I may mention, in passing, that the two apparatus described in this part of your Correspondents' letter, viz. an iron tube closed with an iron plug, and a gun-barrel protected by luting and well secured at the orifice, were the very instruments that foiled me last November and December. There must be free enough exit for nitrogen; I used large crucibles of hammer-iron with loose lids and porous lutes. I do not allude to the first experiment of this set (p. 301), for it is original on the part of Mr. Smith. This brings us down to p. 304.

The last experiment but two (p. 304) just confirms Mr. Johnstone's observation, already confirmed by myself, that ignigenous paracyanogen gives off cyanogen when heated! The last but one is wholly exceptionable on account of the material; and consequently, the last paragraph but one (p. 305) is not to the point. The last of all is a feeble trial, made on a wrong subject, and attended by no significant result.

In conclusion, this is all I can oppose to the authority of Dr. Brett and Mr. Smith. I trust it is enough to preserve the right-minded scepticism of chemists on the subject at issue, which is either so momentous as to be worth a world of cautious inquiry, or so unimportant as not to merit even condemnation. Meanwhile, I beg your leave to take this opportunity of offering a few suggestions on the right repetition of my experiments, which I confess to be necessary on account of the extreme condensation of my memoirs; a quality, which must be attributed to my inexperience on one hand, and a settled aversion to many words on the other.

If the apparent transformation be real, it is manifest that the rationale must be very subtle to find, however simple it may yet be found to be, and that it is very likely that the success of the processes may depend on contingencies which are the least obvious. I often failed; at length far oftener succeeded; and have described the operations in successful cases, trusting that I have included the essential circumstances of success, but knowing very well that I may not have done so. Hence the necessity of copying the very letter of the text; and, hence, too, my humble right of suggesting the propriety of not concluding too hastily against my special positive results. Chemists, who are willing to work, will not easily be deluded by silicated vessels, dirty crucibles, or foul potassa. In chemical investigation it is very difficult to discover new truth; but for every one who feelingly knows that it is so, it is also difficult to be very much misled.

As it is evident that, so far as I was led to perform these experiments by hypothetical insight, true or imaginary, the likelihood of success depended on the correctness of the view of the constitution of paracyanogen exhibited in the paper on that substance, scrupulous care must be taken not to infringe on that constitution by any experimental accident, such as ever so slight access of air during the prolonged heatings; inasmuch as for every equivalent of carbon that may be surreptitiously drawn off, say in the form of carbonic oxide, three equivalents are lost for conversion, and one less of silicon can be produced. I have wasted a great deal of paracyanogen in this way: and it was only last week that I hit upon a lute capable of hindering the paracyanide of a certain metal from being wholly changed into carbonic oxide, nitrogen, and metallic oxide during a protracted subjection to the red heat of iron in a porcelain crucible, after having made a great many trials in vain.

Paracyanogen is like carbon, boron, and silicon, in presenting two distinct forms, the unignited and the ignited; with all the intermediate conditions. In the former state it gives nitrogen away at the incipient red heat of iron, though slowly and without force enough to overcome any considerable pressure; while in the latter it requires at least the white heat. On this account ignited paracyanogen may be mistaken for ignited carbon, and an operation said to have failed, which would have succeeded if it had been finished. Paracyanogen is in every way a tantalizing thing to make experiments upon.

My white heat for copper is the temperature at which the metal is white to my eye, and yet solid; when it is swelled out to the utmost verge of solidity, and is about to fall down liquid. This, too, needs management. As to the fusion of sulphate of lime, it is needless to speak of every contingency. To know the accidental from the essential is the problem of scientific authorship.

In fine, the alleged conversion of carbon into silicon is before the world, at least unencumbered by presumptuous explanations, and not disgraced by anything like a challenge. If others fail to procure the same results, I shall be sorry; but so long as my observations continue to lead me on to others of a more and more practical kind, I shall be very well content to have my first publication regarded as a plenary mistake, and will refrain from writing any more till it shall have become impossible not to succeed in the performance of my operations. I have, Sir, the honour to be,

Your obedient Servant,

SAMUEL MORISON BROWN.

3 Dartmouth Row, Blackheath,
near London.

LIX. *Description of an Electric Thermometer.* *By* E. SOLLY, *Jun., Esq.*

To Richard Taylor, Esq.

MY DEAR SIR,

I AM induced to send you the following account of a little thermo-electric arrangement, believing that it may be interesting to some of your readers; for although there is little or no novelty in the principles on which its action depends, I am not aware that it has been before practically employed.

I had for some time experienced considerable inconvenience in conducting certain experiments requiring a long-continued and uniform degree of heat, from the difficulty of regulating the temperature of my furnace, and the constant uncertainty whether everything was proceeding satisfactorily during my absence from the laboratory. I had in consequence often thought of the possibility of so arranging a little thermo-electric apparatus that it might serve as an index of the rate of combustion and consequent heat of the furnace, by the deflection of a galvanometer at a distance from the source of heat. A small thermo-electric battery might be so placed that the one series of joints or solderings should be constantly exposed to the heated surface of the furnace; but a serious obstacle presented itself to any contrivance of this kind, which was the difficulty of keeping the alternate joints of the battery cool: a current of electricity would doubtless be evolved in consequence of the difference of temperature existing between the two sides of the battery, but of course as the heat would gradually traverse from the hotter to the cooler side, it would greatly diminish and modify the results, and thus present false indications of temperature; whilst even if it were possible to keep the one side of the battery cool, either by water or by any other means, yet the value of the deflection of the galvanometer would be always uncertain, as the difference between the two sides of the battery could never be ascertained unless the exact reduction of temperature thus caused were correctly known.

After one or two unsuccessful attempts to overcome this objection, I laid aside the battery and substituted in its place a single pair of metallic elements, which I found gave abundance of power, and was not liable to the defect which the use of the battery involved.

A piece of copper wire, one twenty-fourth of an inch in diameter, and of sufficient length to reach from the furnace to my ordinary sitting-room, was joined by twisting the ends to a similar wire of soft iron, the ends of both having been previously well cleaned with sand-paper. The two wires were then secured in a convenient manner by small nails to the walls of the rooms they had to pass through, care being taken that they were not anywhere in contact with each other, except at the two extreme points of junction; the one of these was so placed in the flue of the furnace that it was completely exposed to the action of the hot air and smoke at that part where the flue left the body of the furnace, whilst the other joint was in my room in contact with a thermometer, and surrounded with cotton, so as to render it as little as possible liable to sudden changes of temperature. The copper wire was then divided about a foot from the joint thus protected; the two ends of the wire were connected with the extremities of a galvanometer coil, and the apparatus was complete.

A metallic circuit was thus made consisting of two elements, the one being the iron-wire, and the other the copper-wire, including the additional length of copper wire in the coil of the galvanometer. The one joint or point of contact was of course always far hotter than the other, and would necessarily remain so, so long as the fuel in the furnace continued to burn, and would be dependent on the rate of combustion in the furnace; whilst the other junction would always remain very nearly at the temperature of the air, and its variations could be readily known by the thermometer in contact with it. A current of electricity was thus generated, proportioned to the difference of temperature between the two joints, and a deflection of the galvanometer was caused, which increased when the furnace became hotter, decreased when it cooled, and at all times indicated accurately the changes of temperature taking place, thus giving me a thermometer which indicates, without my moving from the table, the exact rate of combustion going on in the furnace, which is fifty yards distant from the indicator. I believe that it is commonly supposed that weak thermo-electric currents cannot be well made to traverse small wires of any length, and this is probably the reason why this beautifully manageable power has been so little employed for practical uses. I have received so much satisfaction from the arrangement just described, that I am convinced it would be found a very useful indicator of temperature in stoves, flues, and hot pipes, in many situations where a common thermometer is inapplicable.

The cost of such an apparatus must necessarily be more expensive than any thermometer, but then it must be remembered that it does far more than an ordinary thermometer, giving us the means of knowing the temperature of a stove or furnace at a distance, giving us indications of the least change or variation in the source of heat, with even greater certainty and distinctness than a thermometer; and besides, showing these changes so rapidly that we know whether it is becoming hotter or colder, before a thermometer placed on the outside of the stove indicates any change. I have observed, on comparing the thermo-electric with an ordinary thermometer placed on the iron plate forming the top of the furnace, that if the ash-pit door were closed, or the draught in any other way diminished, the deflection of the galvanometer was immediately reduced, whilst the external thermometer continued to rise for some little time; and that the indications given by the galvanometer of the augmentation or decrease of temperature in the furnace, always preceded the same indications from the mercurial thermometer on the top.

I am, dear Sir, yours very truly,

London, Oct. 20, 1841. EDWARD SOLLY, Jun.

LX. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from p. 325.]

Feb. 24, 1841.—A PAPER, entitled "Description of parts of the Skeleton and Teeth of five species of the genus Labyrinthodon, from the new red sandstone of Coton End and Cubbington Quarries; with remarks on the probable identity of the Cheirotherium with that genus of extinct Batrachians," by Richard Owen, Esq., F.G.S., F.R.S.

In a paper read on the 20th of January, Mr. Owen described the peculiarities in the structure of the teeth of the Labyrinthodon (see *ante*, p. 315); and having been favoured by Dr. Lloyd, since that paper was written, with the loan of all the reptilian remains obtained from the new red sandstone of Warwick and Leamington, deposited in the Museums of those towns, and having been liberally permitted by the Committees of the Institutions to examine the teeth by the microscopic test, he gives, in this paper, a minutely detailed description of the fragments submitted to his examination, and points out their relative connexion to each other, and the laws by which he has been enabled to determine that they all belong to the genus Labyrinthodon, and [are] confirmatory of the Batrachian nature of the Wurtemberg fossil.

The specimens which Mr. Owen has examined are referable to five species, to which he has applied the names,—1. *Labyrinthodon salamandroides*, 2. *L. leptognathus*, 3. *L. pachygnathus*, 4. *L. ventricosus*, and 5. *L. scutulatus*; and he describes successively the characters exhibited by the bones assignable to the 2nd, 3rd and 5th species.

Labyrinthodon leptognathus.—The remains which Mr. Owen considers as portions of this species, consist of fragments of the upper and lower jaws, two vertebræ, and a sternum. They were found in the sandstone quarries at Coton End, near Warwick.

The portions of the upper jaw show that the maxillary or facial division of the skull was broad, much depressed and flattened, resembling the skull of the gigantic Salamander and of the Alligator; and the outer surface of the bones was strongly sculptured, as in the Crocodilian family, but of a relatively larger and coarser pattern. The fragment described contains the anterior moiety of the single row of small teeth, or 30 sockets, and the base of one of the great anterior tusks. The bases of the serial teeth project directly from the outer wall of the shallow socket, there being no alveolar ridge external to it. The large anterior fang is three times the size of the first of the serial teeth, and the size of these gradually diminish as they are placed further back; the length of the common-sized being about two lines, and the greatest breadth one-third of a line. The apical two-thirds of each tooth is smooth, but the basal third is fluted, and anchylosed to the outer wall of the socket. The breadth of the upper jaw, opposite the middle of the dental series, was two inches six lines; in proceeding backwards the jaw gradually expands to three inches, and in proceeding forwards narrows, but in a less degree towards the anterior extremity, and then slightly widens or inclines outwards on account of the large tusks. Where the upper jaw is entire, a portion next the median suture, four lines in breadth, is separated from the maxillary bone by a longitudinal harmonia, and corresponds with the position of the nasal bone in the Crocodile. On comparing the structure of the cranium of the Labyrinthodon with the Batrachian condition of the same part, Mr. Owen shows that an important difference will be found to exist. In both the caducibranchiate and perennibranchiate species, the upper maxillary bones do not extend horizontally over the upper surface of the skull, but leave a very wide interval between the maxillary and nasal bones; and the palatal processes of the former contribute as little to form the floor of the nasal cavity: in the Crocodiles, on the contrary, the palatal processes of the maxillary bones extend horizontally inwards, and meet at the middle line of the roof, forming an unbroken floor to the nasal cavity. In the Labyrinthodon the superior maxillary bones, as already shown, extend inwards to the nasal bone, constituting with it a continuous roof to the nasal cavities; but the palatal processes, instead of reaching to the middle line, as in the Crocodiles, are very narrow, as in the Batrachia. The osseous roof of the mouth is principally composed of a pair of broad and flat bones, analogous to the divided vomer in Batrachia, but of much greater relative extent, approaching, in this respect, those of the Menopome, and defending the mouth with a more extensive roof of bone than exists in any Lacertian reptile: "physiologically, therefore," observes Mr. Owen, "the Labyrinthodon, in this part of its structure, comes nearest to the Crocodile; but the structure itself, morphologically, is essentially Batrachian." In the Menopome and gigantic Salamander, a row of small teeth extends transversely across the anterior extremity of the vomerine bones: and the occurrence in the Labyrinthodon of a similar row, consisting in each palatine bone of three median small teeth and two outer larger ones, marks most strongly its Batrachian nature; and from the outermost tooth a longitudinal row of small and equal-sized teeth is continued backward along the exterior margin of the palatine bone. The whole of this series of palatal teeth is nearly concentric with the maxillary teeth.

In Lacertine reptiles the examples of a row of palatal teeth are rare, short, and situated towards the back of the palate, upon the pterygoid bones, as in the Iguana and Mosasaur. In Batrachia the most common disposition of the palatal teeth is a transverse row placed at the anterior part of the divided vomer in Frogs, the Menopome and gigantic Salamander, and at the posterior part in certain toads. In the Amphiume*, on the contrary, the palatal teeth form a nearly longitudinal series along the outer margin of the palatine bones. The Labyrinthodon, as already shown, combines both these dispositions of the palatal teeth. The posterior palatine apertures are more completely circumscribed by bone than in most Batrachians, occupying the same relative position as in the Iguana. The posterior margin only of one of the anterior apertures is exhibited in this specimen, but from its curve Mr. Owen infers that the two apertures were not confluent, as in the Crocodile, the Frog, or the Menopome, but that they were distant, as in the Iguana.

From the physiological condition of the nasal cavity Mr. Owen is disposed to believe that the Labyrinthodon differed from the Batrachians and resembled the Saurians, in having distinct posterior nasal apertures surrounded by bone, and that its mode of respiration was the same as in the higher air-breathing reptiles. In the shedding and renewal of the maxillary and the transverse palatal teeth, Mr. Owen shows that the process took place alternately in each row, as in many fishes, whereby the dental series is always kept in an efficient state.

The author then describes a portion, sixteen inches long, of the left ramus of an under jaw from the Warwick sandstone, and considered to belong to the same species as the bone just described. It is slender and straight, and the symphysial extremity is abruptly bent inwards, and it presents, Mr. Owen says, almost as striking a Batrachian character as any of the bones just mentioned. The angular piece is of great breadth, extending on both sides of the jaw, and is continued forward to near the symphysis, forming the whole of the inferior part of the jaw, and extending upon the inner as far as upon the outer side of the ramus, the inner plate performing the function of the detached os operculare in the jaw of Saurians. The dentary bone is supported upon a deep and wide groove along the upper surface of the angular piece, which also projects beyond the groove, so as to form a strong convex ridge on the external side of the jaw, below the dentary piece. This character, which in the large bull-frog (*Rana pipiens*) is confined to the posterior part of the maxillary ramus, is in the Labyrinthodon continued to near the anterior extremity. The teeth are long and slender, gradually diminishing in size towards the anterior portion of the jaw, and the fragment presents a linear series of not less than fifty sockets, placed alternately a little more internally; and at the anterior inflected part of the jaw is the base of the socket of a large tooth. The anterior portion of the jaw being broken off, it is uncertain if the serial teeth were continued externally to the anterior tusk, a remarkable ichthyic character noticed in another species of Labyrinthodon.

The sockets of the teeth are shallower than in the upper jaw; the

---

[* Dr. Harlan's dissection of the *Amphiuma means* will be found in Phil. Mag., First Series, vol. lxiii. p. 325.—Edit.]

outer wall is more developed than the inner, and the anchylosed bases of the teeth more nearly resemble, in their oblique position, those of existing Batrachia. Mr. Owen then describes the microscopic structure of the teeth, and he shows that, between the apex and the part where the inflected vertical folds of the cement commence, the tooth resembles, in the simplicity of its intimate structure*, that of the entire tooth of ordinary Batrachia and most reptiles; and in the lower or basal half of the tooth the structure described in the previous memoir commences, and gradually increases in complexity. From the long and slender character of this ramus, Mr. Owen shows that the length of the head, as compared with the breadth, approximates more nearly to Crocodilian proportions than to the ordinary Batrachian ones; but that among existing Batrachia it resembles most nearly the Amphiume.

A dorsal vertebra from Coton End, which is next described, presents still further evidence of the Batrachian nature of the Labyrinthodon, in having concave but not deep articular cavities at the extremities of the body, a condition now known among existing reptiles only in the Gecko, and in the lower or perennibranchiate division of Batrachians. The body of the vertebra is elongate and subcompressed, with a smooth extended, but not regularly curved surface, terminating below in a slightly produced, longitudinal, median ridge; and it exhibits the same exceptional condition in the Reptilian class as do the vertebræ of existing Batrachians, in having the superior arch or neurapophysis anchylosed with the centrum. From each side of the base of the neural arch extends obliquely, outwards and upwards, the remains of a thick and strong transverse process; and from their strength and direction Mr. Owen gathers indications of a necessity for an expanded respiratory cavity, and that they supported ribs.

A symmetrical bone, resembling the episternum of the Ichthyosaurus, is also described. It consists of a stem or middle, which gradually thickens to the upper end, where cross-pieces are given off at right angles to the stem, and support on each a pretty deep and wide groove, indicating strongly the presence of clavicles, and thus pointing out another distinction from Crocodiles, in which clavicles are wanting.

In concluding the description of these remains of the *Labyrinthodon leptognathus*, Mr. Owen says, that they prove the fossil to have been essentially Batrachian, with striking and peculiar affinities to the higher Sauria, leading, in the form of the skull and the sculpturing of the cranial bones, to the Crocodilian group, and in one part of the dental structure, in the form of the episternum, and the biconcave vertebræ, to the Ichthyosaurus; while in the bony palate there is a deviation from the Batrachian character, and a resemblance to the Lacertian type. Another marked peculiarity in this fossil is the anchylosis of the base of the teeth to distinct and shallow sockets, by which it is made to resemble the Sphyræna and certain other fishes. From the absence of any trace of alveoli of reserve for the

* See *antè*, p. 317.

successional teeth, Mr. Owen believes the teeth were reproduced, as in many fishes, especially the higher Chondropterygii, which formed the *Amphibiæ natantes* of Linnæus, in the soft mucous membrane which covered the alveolar margin, and subsequently became fixed to the bone by anchylosis, as in the Pike and Lophius. No remains of the locomotive organs of the *L. leptognathus* have yet been found.

Labyrinthodon pachygnathus.—In detailing the remains of this species, consisting of portions of the lower and upper jaws, an anterior frontal bone, a fractured humerus, an ilium with a great part of the acetabulum, the head of a femur, and two unguial phalanges, Mr. Owen dwells on further Batrachian characters and certain peculiarities of structure, and shows the points in which it agrees with the *L. leptognathus*. A portion, nine and a half inches long, of a right ramus of a lower jaw is first described; and in addition to the characters common to it and the fragment of the lower jaw of the *L. leptognathus*, in the structure of the angular and dentary pieces, the author shows that the outer wall of the alveolar process is not higher than the inner, as in Frogs and Toads, the Salamanders and Menopome, in all of which the base of the teeth is anchylosed to the inner side of an external alveolar plate. The smaller serial teeth are about forty in number, and gradually diminish in size as they approach both ends, but chiefly so towards the anterior part of the jaw. The sockets are close together, and the alternate ones are empty. The great laniary teeth were apparently three in each symphysis, and the length of the largest is considered to have been one and a half inch. A section through the base of the anterior tusk above the socket exhibits the structure described in Mr. Owen's first memoir; but a section of the second tusk, also taken above the socket, exhibited a very simplified modification of the labyrinthic arrangement, presenting a disposition closely analogous to that at the base of the teeth of the Ichthyosaurus. The apical half of the tusks has a smooth and polished surface, and the pulp-cavity is continued, of small size, into the centre of this part of the tooth. In the serial teeth, which in other respects, except size, correspond with the preceding description of the tusks, the central pulp-cavity is more quickly obliterated, but the alveoli are large, moderately deep and complete: the texture of the teeth is dense and brittle. The base of each tooth is anchylosed to the bottom of its socket, as in Scomberoid and Sauroid fishes; but the Labyrinthodon possesses, Mr. Owen says, a still more ichthyic character in the continuation, preserved in this specimen, of a row of small teeth anterior and external to the two or three larger tusks. A double row of teeth thus occasioned does not exist in the maxillary bones, either superior or inferior, of any Batrachian or Saurian reptile; in Mammalia it has been noticed only in the upper jaw of the hare and rabbit, and in Fishes only in the lower jaw.

A fragment of the superior maxillary bone is also described, and its chief deviation from the Crocodilian type of structure is the continuation of the palatal plate of the intermaxillary bone for about an inch to the outer side of the base of the external plate or process; while in the Crocodiles the external wall of the intermaxillary bone is united by the whole of its outer margin with the maxillary, and is thence continued along the whole outer contour of the intermaxillary bone. Now in the Labyrinthodon the intermaxillary bone presents the same peculiar modification of the Batrachian condition of this bone as in the higher organized Batrachia, the palatal process of the intermaxillary extending beyond the outer plate both externally and, though in a less degree, internally, where it forms part of the boundary of the anterior palatal foramen, whence the outer plate rises in the form of a compressed process from a longitudinal tract in the upper part of the palatal process; it is here broken off near its margin, and the fractured surface gives the breadth of the base of the outer plate, stamping the fossil with a Batrachian character conspicuous above all the Saurian modifications by which the essential nature of the fossil appears at first sight to be marked.

In the anterior frontal bone, Mr. Owen says, there are also indications of Crocodilian structure. Its superior surface is slightly convex, and pitted with irregular impressions; and from its posterior and outer part it sends downwards a broad and slightly concave process, which the author considers the anterior boundary of the orbit. This process presents near its upper margin a deep pit, from which a groove is continued forwards; and in the corresponding orbital plate of the Crocodile there is a similar but smaller foramen.

From these remains of the cranium of the *Labyr. pachygnathus*, it is evident, Mr. Owen states, that the facial or maxillary part of the skull was formed in the main after the Crocodilian type, but with well-marked Batrachian modifications in the intermaxillary and inferior maxillary bones. The most important fact which they show is, that this Sauroid Batrachian had subterminal nostrils, leading to a wide and shallow nasal cavity, separated by a broad and almost continuous palatal flooring from the cavity of the mouth; indicating, with their horizontal position, that their posterior apertures were placed far behind the anterior or external nostrils; whereas in the air-breathing Batrachia the nasal meatus is short and vertical, and the internal apertures pierce the anterior part of the palate. Mr. Owen therefore infers that the apparatus for breathing by inspiration must have been present in the Labyrinthodon as in the Crocodile; and hence still further, that the skeleton of the Labyrinthodon will be found to be provided with well-developed ribs, and not, as in the existing Batrachia, with merely rudimentary styles. Since the essential condition of this defective state of the ribs of Batrachia is well known to be their fish-like mode of generation and necessary distention of the abdomen, Mr. Owen likewise directs attention to the probability that the generative economy of these fossil reptiles may have been similar to that of existing crocodiles.

A fragment of a vertebra presents analogous characters to the vertebra of the *L. leptognathus* previously noticed.

Of the few bones of the extremities which have come under Mr. Owen's inspection, one presents all the characteristics of the corresponding part of the humerus of a toad or frog, viz. the convex, somewhat transversely extended articular end, the internal longitudinal depression, and the well-developed deltoid ridge. The length of the fragment is two inches, and the breadth is thirteen lines. The ridges are moderately thick and compact, with a central medullary cavity. In its structure as well as in its general form, the present bone agrees with the Batrachian, and differs from the Crocodilian type.

Again, in the right ilium, about six inches in length, and in the acetabulum, there is a combination of Crocodilian and Batrachian characters. The acetabular cavity is bounded on its upper part by a produced and sharp ridge as in the frog, and not emarginate at its anterior part, as in the crocodile. Above the acetabulum in the frog the ilium gives off a broad and depressed process, the lower extremity of which is separated from the acetabulum by a smooth concave groove, both of which are wanting in the crocodile, there being only a slight rising of the upper border of the acetabulum. These characters, however, are well developed in the Labyrinthodon: but the process, instead of being depressed is compressed, and its internal extremity is pointed and bent forwards, representing the rudiment of the long anterior process of the ilium in the *Batrachia anoura*; but it does not attain in the Labyrinthodon the parallel of the anterior margin of the acetabulum, and the bone terminates in a thick truncated extremity a few lines anterior to the acetabulum; an essential feature of resemblance to the Crocodiles and difference from the Batrachians. But the most marked difference in this fossil from the crocodile is the length of the ilium posterior to the acetabulum, in which it agrees with the analogous portion of the frog and other tailless Batrachia; while, on the contrary, there is an agreement with the Crocodilian type in the mode of articulation to the vertebral column. In the frog a transverse process of a single vertebra abuts against the anterior extremity of the produced ilium. In the crocodile the transverse processes of two vertebræ are thickened and expanded, and joined to a rough, concave, articular surface occupying the inner side of the ilium, and a little posterior to the acetabular cavity. In the Labyrinthodon is a similar well-marked, rough, elongated, concave, articular surface, divided by a non-articular surface, and destined for the reception of the external extremities of two sacral ribs. The Labyrinthodon likewise agrees with the crocodile in the lower part of the acetabulum being completed by the upper extremity of the pubis, the anterior and inferior part of the ilium offering an obtuse process at the posterior part of the lower boundary of the acetabular cavity.

As the fragment of the ilium was discovered in the same quarry as the two fragments of the cranium and the portion of the lower jaws, Mr. Owen thinks they may have belonged to the same animal; and if so, as the portions of the head correspond in size with those of the head of a crocodile six or seven feet in length, but the acetabular cavity with that of a crocodile twenty-five feet in length, then the hinder extremities of the Labyrinthodon must have been of disproportionate magnitude compared with those of existing Saurians, but of approximate magnitude with some of the living anourous Batrachia. That such a reptile, of a size equal to that of the reptile whose remains have just been described, existed at the period of the new red sandstone, Mr. Owen says, is abundantly manifested by the remains of those singular impressions to which the term Cheirotherium has been applied. Other impressions, as those of the *Cheirotherium Hercules,* correspond in size with the remains of the *Labyrinthodon Salamandroides,* which have been discovered at Guy's Cliff. The head of a femur from the same quarry in which the ilium was found, is shown to correspond in size with the articular cavity of the acetabulum. The two toe-bones, or terminal phalanges, are stated to be strictly Batrachian, presenting no trace of a nail, and from their size are referred to the hind-feet of the *L. pachygnathus.*

Thus, observes Mr. Owen, all these osseous remains from the Warwick and Leamington sandstones agree in their essentially Batrachian nature, and, in this interesting conclusion, with the fossils of the German keuper; and he concludes this portion of the memoir with some observations respecting the so-called Cheirotherium footsteps. He has long believed that they were the foot-prints of a Batrachian, and most probably of that family which includes the toad and frog, on account of the difference of size in the fore and hind extremities; but, in consequence of the peculiarities of the impressions, he has always considered that the animal must have been quite distinct in the form of its feet from any known Batrachian or other reptile. Now then, he observes, we have in the Labyrinthodon also a Batrachian reptile, differing as remarkably from all known Batrachia and from every other reptile in the structure of its teeth: both the footsteps and the fossils are, moreover, peculiar to the new red sandstone; and though the generic name Labyrinthodon may be susceptible hereafter of being expanded to the appellation of a family, yet, he asks, may it not be justifiable to consider the term Cheirotherium as one of the synonyms of Labyrinthodon?

Labyrinthodon scutulatus.—The remains, to which this specific designation has been applied by the author, composed a closely and irregularly aggregated group of bones imbedded in sandstone, and manifestly belonging to the same skeleton; they consist of four vertebræ, portions of ribs, a humerus, a femur, two tibiæ, one end of a large flat bone, and several small osseous, dermal scuta. The mass was discovered in the new red sandstone at Leamington, and was transmitted to Mr. Owen by Dr. Lloyd in the summer of 1840.

The vertebræ present biconcave articular surfaces similar to those of the other species. In two of them, the surfaces slope in a parallel direction obliquely from the axis of the vertebræ, as in the dorsal vertebræ of the frog, indicating an habitual inflexion of the spine, analogous to that in the humped back of the frog. The neurapophyses are anchylosed to the vertebral body. The spinous process rises from the whole length of the middle line of the neurapophysial arch, and its chief peculiarity is the expansion of its elongated summit into a horizontally flattened plate, sculptured irregularly on the upper surface. A similar flattening of the summit of the elongated spine is exhibited in the large atlas of the toad. The body of the vertebræ agrees with that of the *L. leptognathus*. The humerus is an inch long, regularly convex at the proximal extremity, and expanded at both extremities, but contracted in the middle. A portion of a somewhat shorter and flatter bone is bent at a subacute angle with the distal extremity, and resembles most nearly the anchylosed radius and ulna of the Batrachia.

The femur wants both the extremities; its shaft is subtrihedral and slightly bent, and its walls are thin and compact, including a large medullary cavity. The tibiæ are as long, but thicker and stronger than the femur. They had lost their articular extremities, but exhibited that remarkable compression of their distal portion which characterizes the corresponding bone in the Batrachia: they likewise have the longitudinal impression along the middle of the flattened surface. The length of the more perfect shaft is 2 inches 1 line. The precise nature of the broad flat bone, Mr. Owen had not determined.

With respect to the osseous dermal scuta, Mr. Owen remarks, that though they form a striking instance of the Crocodilian affinities of the Leamington fossil, yet as these detached superficial bones are the most liable to be separated from the fragmentary skeleton of the individual they once clothed, the negative fact of their not having been found associated with the remains of the Labyrinthodon in other localities proves nothing in regard to a difference of dermal structure between the Leamington and Warwick species. Indeed no anatomist, he says, can contemplate the extensive development and bold sculpturing of the dermal surface of cranial bones in the *Labyrinthodon pachygnathus* and *L. leptognathus* without a suspicion, that the same character may have been manifested in bony plates of the skin in other parts of the body. Admitting for a moment this structure to be proved, to what extent, asks Mr. Owen, does it affect the claims of the Labyrinthodon to be admitted into the order of Batrachians, in which every known species is covered with a soft, lubricous and naked integument, without scales or scutæ? In reply, he says, that the skin is the seat of variable characters in all animals; and, apart from the modifications of the osseous and dental systems, and other intimate organs, is apt to mislead the naturalist who is in quest of the real affinities of a species: and he instances the Trionyx, as an example of a soft-skinned animal among Chelonian reptiles. Lastly, Mr. Owen shows, that, previously to the discovery of the fossils described in this memoir, the only Batrachian remain which had been found in beds anterior to the epoch of the Molasse is the fragment of a skull, on which Prof. Jaëger founded his *Salamandroides giganteus*.

March 10.—A paper "On the Geological Structure of the Northern and Central Regions of Russia in Europe," by Roderick Impey Murchison, Esq., P.G.S., and M. E. de Verneuil, V.P.G.S. of France, was commenced.

ZOOLOGICAL SOCIETY.

Feb. 9.—A continuation of the descriptions of Mr. Cuming's shells, by W. J. Broderip, Esq., was read, for which we refer to No. 97 of the Society's Proceedings. The following remarks by the author, and letter from Sir D. Brewster, were appended to the description of *Bulinus velatus.*

In a great number of the beautiful land-shells of the Philippine Islands, collected by Mr. Cuming, and herein-before described, the pattern, upon immersion in water or other fluid, becomes entirely obliterated till evaporation restores the colours to all their pristine brilliancy. In the species now before us, the very reverse is the result of immersion. The external whitish porous epidermis which veils the shell when dry, suffers the bright colours to shine out when immersed in water. *Bul. velatus* is described above, as it appears on immersion, and before it becomes dry: but in the latter state the beauties of the shell are shrouded, and the colour of the sutural bands, peeping out between interstices in the epidermis, gives to these bands a moniliform appearance.

I sent to Sir David Brewster, as the highest authority on such subjects, four or five species of those land-shells from which the pattern disappears upon immersion; but I have not as yet forwarded to him any upon which the colours come out when so treated. Sir David has been so obliging as to send me the following letter, which I now lay before the Society:—

"Dear Sir,—I beg to return you my best thanks for the very interesting specimens of land-shells from the Philippine Islands, which you have been so kind as to send me. The disappearance of the white pattern by immersion in water or any other evaporable fluid, and its subsequent reappearance when the shell is dry, are phenomena perfectly analogous to those of *hydrophanous opal, tabasheer,* and other porous substances.

"The phenomenon in the land-shells is still more beautiful when we examine them by *transmitted* light. The pattern which is *white* by *reflected* light, is *dark* by *transmitted* light, and *vice versâ.* This is particularly beautiful in the *Helix pulcherrima,* where the ground of the *white* pattern is almost *black* by reflected light, and of a light *reddish* colour by transmitted light, the pattern which is *white* by reflection having a *dark red* colour by transmitted light.

"In all these shells, the difference of structure by which the pattern is produced, does not exist in the shell, but in the *epidermis,* and hence the pattern may be wholly obliterated by removing the epidermis. It appears to me, from very careful observations, that the epidermis consists of two layers, and that it is only the upper layer which is porous wherever the pattern is white. These *white* or porous portions of the epidermis differ from the other parts of the upper layer only in having been deprived of, or in never having possessed, the element which gives transparency to the membrane, in the same manner as hydrophanous opal has become *white,* from the expulsion of its water of crystallization.

"When the shell is immersed in water or any other fluid, the fluid enters the pores of the white epidermis, and having nearly the same refractive power as the epidermis, no light is reflected at the separating surface of the water and the pores which contain it, so that the light passes through the membrane, which thus loses its white appearance. When the water escapes from the pores by evaporation, or is driven from them by heat, the membrane again reflects white light from the numerous surfaces of its pores.

"As the colouring matter resides in the shell itself, its peculiar colour is seen through the epidermis as distinctly where it is porous as where it is not porous, when the porous portion has been rendered transparent by the absorption of a fluid.

"If we apply oil or varnish to the white pattern, we may obliterate it permanently, or we may change it into a pattern entirely different from the original one.

"If these observations appear to you to have any interest, you are welcome to make any use of them you please.

"I am, dear Sir, ever most faithfully yours,
"D. Brewster."

It will be observed, that Sir David Brewster points out how the application of oil or varnish to the white pattern may obliterate it permanently; such a case has already happened more than once: persons who have become possessed of some of the species whose patterns are lost on immersion, not content with their natural beauties, and unaware of their peculiarity, have had recourse to art, and by applying oil or varnish, have spoiled their specimens,—a proper punishment for trying to mend nature.

LONDON ELECTRICAL SOCIETY.

Oct. 19, 1841.—A Translation by the Secretary, of "Observations on the Electrical Effects of the Gymnotus," by Professor Schœnbein, was read.

The author having related at some length the nature and results of certain experiments he made with the Gymnotus of the Adelaide Gallery, proceeds to examine the reasons which have induced some philosophers to attribute its electrical powers to the peculiar physical or mechanical construction of the electric organ, and adduces a mass of facts in proof that this organ bears very little analogy to Volta's pile. For instance, it consists of a combination of substances possessing naturally but a very feeble electromotive power, and yet the fish will give shocks equal in force to those obtained from a very large Leyden jar charged at its maximum, or from 200 pairs of a highly excited voltaic battery. Again, the creature dwells in a conducting medium which completes at all times the circuit between the poles of its organ, and yet no passage of electricity occurs but at its will; it is precisely as if it possessed a means of insulating or uninsulating the apparatus at pleasure; although physiologists have not detected any arrangement fitted for such a purpose. The fish too can regulate the intensity of its discharges at will; this is especially evident when a comparison is made between the effects produced by touching it with the human hand, or through the medium of a metallic conductor. The power evidently depends upon an intimate union, of which we know not the nature, between the vital powers and the physical functions of this organ; for, like all vital organization, the power is exhausted by repeated efforts.

"An Account of Experiments with a Water Battery." By H. M. Noad, Esq., Memb. Elect. Soc.

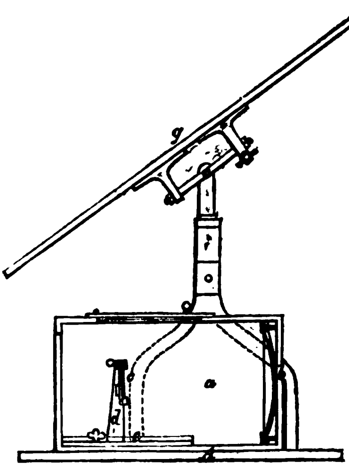
The water battery employed by the author of this paper consisted of 500 pairs, and produced the effects which are known to bear so close an analogy with those from the electrical machine. Among the results, the deposition of carbon, when the electrodes were placed in the flame of a candle, was very characteristic of the peculiar action under these circumstances. The nature of the deposit was first noticed by M. Gassiot.

The Secretary then laid before the Society Mr. Weekes's Monthly Register of the electrical condition of the atmosphere for September.

LXI. *Intelligence and Miscellaneous Articles.*

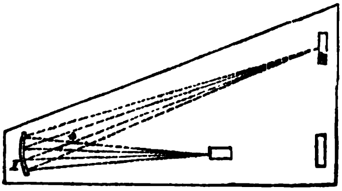
NOTICE OF BEARD'S PATENT FOR IMPROVEMENTS IN APPARATUS FOR OBTAINING LIKENESSES AND REPRESENTATIONS OF NATURE, &c.

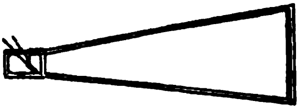
THE drawing shows various views of the apparatus, the nature of which is written on the drawing, and the same parts are referred to by the same letters of reference. *a* is a rectangular box, inside of which and at the end thereof, there is fixed a concave reflector, which may be of glass or metal, as is well understood in making reflectors for telescopes. This reflector is placed with the reflecting surface facing the other end of the box, which has an opening corresponding to the size of the reflector. *c* is a light frame fixed by a support, *d*, to a piece of wood or other material, *e*, with which it slides on the bottom of the box in a direction to and from the face of the reflector and lengthwise of the box; this frame is intended to carry the prepared material on which the impression is to be made

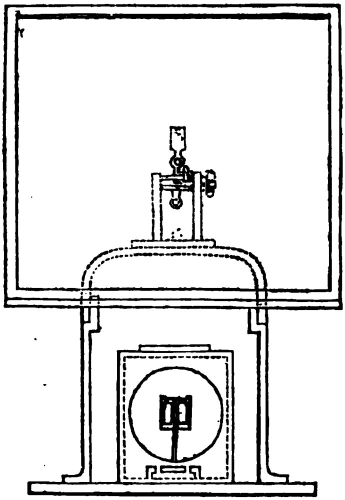


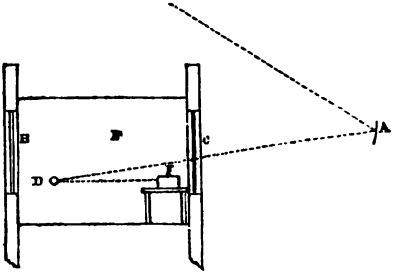
by the reflected image; the material may be retained in the proper position against the frame by a small spring, *f*, pressing against the material on the back; and between the spring and the frame, *c*, the prepared surface is slid. A small door is made on the top of the box for the purpose of observing the focal image, and the frame is slided till properly adjusted to the focus. The box, *a*, should be placed on a table or other support, at such height that the centre of the reflector may be about as high as that part of the person which is intended to be in the middle of the picture. When the reflecting apparatus is to be used, the person whose likeness is to be taken should be placed in a chair, to which some suitable support for the head is attached, to enable him to remain perfectly still, and the refleoting aparatus should then be placed with the open end immediately opposite to the person. A trial surface is then to be put against the frame to receive the reflected image, by which means its correct placing will readily be judged of by looking through the hole in the box, *a*, and the focus will be adjusted by sliding the piece, *e*; the trial surface is then to be removed, and the prepared surface is to be placed in the frame and allowed to remain as long as required to form the image, and a little practice will readily enable the operator to judge when the required effect is obtained. The size of the reflecting apparatus I use, is as follows: the box, *a*, inside fifteen inches long, eight and a half inches high, and eight inches wide, and the interior is black; reflector seven inches clear diameter, and twelve inches focus; the prepared surface on to which the picture is to be formed is two and a half inches long, by two inches wide, but these dimensions may be varied. In using the apparatus above described, when the daylight is very bright, I prefer that the inclined glass roof, A, of the room should be glazed with blue glass, or otherwise to soften the bright rays of light, in order that the person sitting to have a likeness taken may be as near as possible to the glass roof; but when there is a bright sunshine, I use a large looking-glass or reflector. I prefer to use a large concave reflector to collect the rays of light from the sun and throw it on the person sitting.

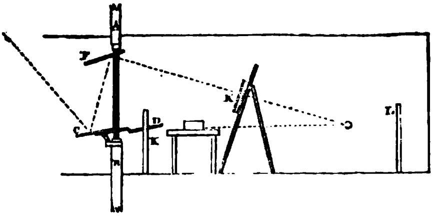
I place the reflecting apparatus out of the centre of the large reflector, B, generally behind the large reflector, by having a hole through such large reflector towards the outer edge thereof; and in making such large reflector, B, I find that it may be done by forming a frame of wood to the figure or shape required, and then covering the surface with small squares of looking-glass of five or six inches square; this is a cheap and convenient way of making such a reflector on a large size for the purpose, but I do not confine myself thereto, the object being to obtain a large reflector to collect the light and direct it strongly on a person or other object, from which it is desired to obtain a likeness or image on to a prepared surface. In preparing plates with silver surfaces to render them more suitable for receiving impressions or images thereon, and then have the surface protected from change, I take sheets of copper with a surface of silver, such as is now commonly used according to the process known as Daguerreotype, and place two such plates together, with their silver surfaces touching, those surfaces having been carefully wiped clean with cotton, and aided, if necessary, with dilute sulphuric acid; and I pass such two plates five or six times between a pair of smooth hardened rollers. I then anneal the plates by heating them to a low red heat and permit them to cool. Then I again subject them to the acid process, and then I pass them, silver face to face, between the rollers, the rollers being set slightly closer together as the plates will have become somewhat thinner, and I repeat such rolling and annealing till I find the silver surfaces are highly polished and equal in appearance all over the silver surfaces. They will then be ready for the next process, which consists in taking a small bunch or tuft of cotton, and dipping it into dilute nitric acid, and then into tripoli, and very lightly rubbing the silver surface of a plate, then quickly and carefully rubbing off the acid and tripoli with dry cotton: in these rubbings a circular motion is to be observed. I then take a surface of velvet, having dusted on it some impalpable powder of charcoal from a muslin bag, and rub the silver surface of the plate in a direction transversely of the length of plate, when the required surface will be obtained, and is ready to undergo the iodine process, as is well understood; but I prefer that the iodine should not be used separately, but that it should be combined with nitric acid and water, or with bromine, or with both, or with bromic acid; and I perform this operation in the following manner: I place a square glass vessel somewhat larger than the plate to be operated on, in a box made of wood, with a cover with an opening at each end to allow of a plate of glass to slide closely across, so as to permit of as little escape of vapour as possible, the plate glass slide being somewhat more than twice the length of the box, and in one part thereof, towards one end, an opening is formed through the glass large enough to receive the metal plate and yet not to allow it to drop through; by this means the slide can bring the plate of metal directly over the box, in order that the vapours as they arise may come in contact with the silver surface, which is placed downwards, and in a few seconds it will be ready to be put into the reflecting apparatus, to receive an impression, or it may be put into a suitable dark case for holding several such plates ready for use. In combining iodine with nitric acid and water, I put equal parts of iodine, nitric acid, and of water, and in combining bromine therewith, I combine an equal part with each of the other three, or I omit to use nitric acid, and use sulphuric acid and water, or I omit the use of acid and simply combine iodine and bromine in the box above mentioned, and the required vapours will quickly operate on the metal plate, the other portion of the glass slide closing the box, when no plate is being operated on. And I have only further to remark, that when the impression has been obtained, the plate is to be operated on by the mercury, and the fixing and washing processes, in like manner to the treatment well understood when treating plates which have, according to the process of Daguerreotype, received an impression or image by the aid of a camera, all which is well known, and in use, and forms no part of the invention. In taking impressions or obtaining images by the aid of the apparatus above described, it is desirable to use a screen behind the person sitting to have a likeness taken, and for this purpose a plate of ground plate glass or other semi-transparent surface, through which the shadow of the person may pass; and at the back of such screen I have a frame capable of being set nearer to or further from the back of the screen, and in this frame I have a white surface capable of receiving the shadow so passed through the semi-transparent screen, and by causing such white surface to come nearer to or further from the screen, I obtain varying effects of light and shade in the picture produced. And I find it of advantage sometimes to use a brown, blue, or black back surface, particularly when taking images of plaster busts or other white objects; for it will be evident that although I have thus far confined the description to the taking likenesses from human beings, any other object having the apparatus placed suitably in front of it, will be received on the reflector, and reflected to the prepared surface, in like manner to that of a person. And I would also remark, that in using a large concave reflector to collect the light as above explained, I find it desirable to stretch across the front thereof, a surface of tissue paper varnished with boiled oil, such as is used for tracing paper, as it has the effect of scattering the light and producing a better effect in the picture.











Repertory of Patent Inventions, N.S., vol. xv. p. 13.

ON THE POLISHED ROCKS OF FONTAINBLEAU.

BY M. DUROCHER.

In the *Comptes Rendus de l'Académie des Sciences*, for July 12, 1841, we find a "Notice on the traces of polishing which the diluvium has left on the sandstones of Fontainbleau," by M. Durocher, of which the following is a translation.

In the places where the diluvium of the valley of the Seine has extended, following the general direction from east-south-east to west-north-west, those marks of polish which are seen so frequently and so distinctly in Scandinavia and in the Alps, had not hitherto been observed on the surface of the rocks. This is probably occasioned by the soft nature of the rocks which form the Paris basin, and their mechanical disaggregation under the influence of exterior agents. It is the same in the countries to the south of the Baltic, on which the diluvium of the north has formed thick deposits of the fragments of the Scandinavian rocks: as only schists or limestones are found in them, it is not possible that the marks produced by the passing of the currents should have been preserved to our times; and yet their existence cannot be called in question; for in boring a well through the diluvial soil in Prussia, on reaching the solid rock it was found polished and grooved as in Sweden and Finland: the deposit which covered it had served as the means of preservation. The absence of these traces in many places which have been the theatre of diluvial phænomena, is therefore to be attributed to atmospheric action.

Amongst the formations which constitute the Paris basin, the sandstones alone appear to me capable of preserving any remains of diluvial impressions: in an excursion which I recently made to Fontainbleau, I indeed recognised that these marks were not entirely effaced. In the forest of Fontainbleau there are but very few spots where this can be observed; the principal cause which hinders this, is, that the solid sandstone seldom appears at the surface in great areas. It is almost everywhere covered with a vegetable crust, and unfortunately in the small number of cases where it may be seen, it is nearly always disaggregated and reduced to sand. As to the blocks which are found heaped up in such great numbers and prodigious size, it is difficult to discern upon their surface the marks of the currents of water which displaced them, and gave them their various and fantastical forms. It is only in the spot well known by the name of the *Gorges de Franchard*, that I was clearly able to recognise the traces of the passage of great masses of water.

I will endeavour to notify, as precisely as possible, the points where I observed it most distinctly. In following the road which leads from the Cross of Souvray (on the road of Ury) to Franchard, when we reach the spot where this road makes a slight deviation to the north-east, we must take the road which leads to the Rochers-des-hautes-plaines, and turn at the third path to the right: this crosses a small plain covered with heath, and leads straight to the defile of Franchard. It opens into the principal valley by a slightly inclined entrance, and forms, as it were, a cutting in the line of rocks which border the valley on this side. It is by descending this way, that the traces of polish may be observed on both sides of this species of defile, but chiefly on the right side. There are very large blocks there, and also masses of flattened rock, which seem to be a part of the sandstone layers, or to have been but little moved from their position. Upon examining their surface, we see that it is polished, and even presents indications of broad furrows, the resemblance of which to the diluvian furrows of the Alps or of Scandinavia, it is impossible to mistake. A circumstance which precludes any doubt that the marks are owing to the action of masses of water, is, that they are directed according to the declivity of the defile, and fall towards the interior of the central valley.

Upon examining the surface quite near, we see no fine grooves as we do on the granites and gneiss of the Alps; nor is there any reason to be surprised at this, because these sandstones, not being formed of grains, which are strongly agglutinated together, were not adapted for receiving and retaining very fine grooves. The polish is also visible on the largest blocks of sandstone; there are several flattened, which are rather sunk in the earth or lie above it on their broadest side, which is nearly horizontal and rugged, whilst the superior surface is polished; by a close examination, we find that the polish is not owing to the friction of these masses against each other or against the earth, but to the action of the waters.

On the north anticlinal of the valley of Franchard, on the side opposite to that I have just mentioned, we find another little defile at the spot where the mass of blocks called Roche-qui-pleure is situated: there the polish is still visible, but only upon some blocks having the position already described; several amongst them show on their sides circular and rounded cavities, which appear to me to have been caused by a whirling of waters; they seem to have followed the declivity of the land in its present configuration, and to have been precipitated towards the middle of the valley. It would have been interesting to see the disposition of the traces at the very bottom of the valley; but it is impossible, for the ground is covered with sand and vegetable earth: there are very few blocks; the greater number of them are in heaps, crowning the two crests.

This valley of Franchard may be compared to a cutting hollowed by the waters in the high plain of sandstone which forms the western part of the forest, and the level of which is very much higher than that of the town of Fontainbleau. The mean direction is from the east 10° south, to the west 10° north; this is the general direction of the numerous lines of hills covered with blocks, and disposed in the form of bands extended in the same direction; the most prominent type of this disposition is the small chain which extends from the Sablons to near Milly, and the different rings (*anneaux*) of which bear the names of Rocher-de-bon-Ligne, of the Salamander, of Trape-charette, &c.

As M. Elie de Beaumont has so judiciously observed, these bands take the direction W.N.W., which the Seine follows before receiving the Loing at Saint Manvert, and which it again takes beyond Melun. It is probable that at the diluvian epoch the current followed the same line from the E.S.E. to the W.N.W., that it must have covered the whole extent now occupied by the forest, and that it hollowed out those broad furrows, the ridges of which are crowned by blocks. The sandstone of Fontainbleau being formed of friable and of more cohering layers, the former were doubtless undermined by degrees by the action of the waters; and then the solid layers, being no longer upheld at their base, must have been broken up, and have produced those vast heaps which give such wild and picturesque aspects to this forest.

[On the subject of the preceding notice of M. Durocher, see abstracts of communications made to the Geological Society by Prof. Agassiz, Dr. Buckland, and Mr. Lyell, p. 565, 569—590, of the preceding volume (S. 3. xviii.) of the Philosophical Magazine; and some remarks by Mr. Murchison and M. de Verneuil, in their paper on the Geology of Russia, a notice of which will appear in a future Number: also Dr. Locke's notice of diluvial or glacial scratches on rocks in America, noticed in the Annals and Magazine of Natural History, vol. vii. p. 524; and M. Sefström's Memoir on the Furrows of the Scandinavian Mountains in the SCIENTIFIC MEMOIRS, Part ix. —EDIT.]

ON THE COMPOSITION OF THE BRAIN OF MAN. BY M. FREMY.

It results from the researches of M. Fremy that the brain of man is formed of a considerable quantity of water and of matter insoluble in æther, which he describes as albuminous matter.

The portion soluble in æther is formed chiefly of three substances:

1st. The white matter discovered by Vauquelin, in which M. Fremy has detected very decided acid properties, and which he calls cerebric acid.

2ndly. A liquid fatty matter, which has all the properties and composition of the olein of human fat, analysed by M. Chevreul.

3rdly. Cholestrin discovered in the brain by M. Couerbe. There are besides found in the brain variable and very small quantities of oleic acid, margaric acid, cerebrate of soda, and albuminous matter; to obtain these results, the author cuts the brain into small pieces, boils it repeatedly in alcohol, and allows it to remain for some days in this liquid. The object of this operation is to remove the water contained in the brain, and to coagulate the albumen; the cerebral mass has then lost its elasticity, and may be submitted to pressure; the alcohol retains traces only of cerebric acid, which may be separated by the filter; the brain is then to be treated with boiling alcohol, till it ceases to dissolve any further portion; the liquors are to be evaporated, and the residue treated with boiling absolute alcohol, which removes the olein, the cerebric acid, the cholestrin and the oleic and margaric acids; the albuminous matter and cerebrate of soda do not dissolve. On the cooling of the alcohol the cholestrin and cerebric acid are deposited; these are to be separated by cold æther, which readily dissolves the cholestrin and leaves the cerebric acid. In order to purify the cerebric acid, it must be boiled with slightly acidulated alcohol, in order to decompose the cerebrate of soda, which is always mixed with cerebric acid. Cold alcohol holds the olein, and the oleic and margaric acids: it is to be rendered slightly alkaline by ammonia and evaporated, and by these means the olein is deposited when the liquor has acquired a certain degree of concentration, whilst the oleate and margarate of ammonia remain in solution.

The albuminous matter and the cerebrate of soda, which have resisted the action of the absolute alcohol, are in their turn treated with boiling alcohol, acidulated with hydrochloric acid, which decomposes the cerebrate of soda. The cerebric acid set free dissolves very readily in alcohol; there then remains a coloured matter, albuminous in its nature, which contains sulphur, but never phosphorus.

After having thus determined the composition of the fatty matters of the brain, M. Fremy examined the substances which M. Couerbe has described by the names éléencéphol, céphalote and stéaroconote: and he states that the first is formed of olein and cerebrate of soda; that the second contains olein and cerebrate of soda, associated with traces of albumen; and that the last is merely a mixture of albumen and cerebrate of soda.

In analysing the brain in different states and of different ages, M. Fremy found that the quantity of free fatty acids was variable; and that it even sometimes increased when the fatty matters were left in a closed bottle. He discovered the cause of this curious phænomenon by referring to the observations of M. Chevreul on the fat of carcases, and to the memoir of MM. Pelouze and Boudet, in which they mention the spontaneous saponification of palm oil. He further observed, that it was the albuminous matter of the brain which had the property of eventually converting the olein into oleic acid. Lastly, he found by analysis, that all the fatty bodies occur in the white substance of the brain, and that the gray portion contained only traces of it; if therefore the anatomy of the brain were to be represented in a chemical point of view, it may be said that the substance which forms, so to speak, the frame of the brain, is originally gray, and that it is the fatty matter which infiltrates and spreads in the interior of this gray matter, that forms the white zones which constitute the white portion of the brain.—*Journal de Pharmacie*, tom. xxvii. p. 766.

ACTION OF PEROXIDE OF IRON ON POTASH. BY M. FREMY.

The evident analogy which exists between iron and manganese, leads to the opinion that similar compounds of them will sooner or later be formed.

This idea induced M. Fremy to suppose that it might be possible to form salts in which an oxygenated compound of iron would act the electro-negative part, and which would thus correspond with the compounds of manganic and oxymanganic acid with bases; and the following experiments are conceived by M. Fremy to demonstrate that iron can give rise to compounds produced under the same circumstances as the manganates, and possessing a striking analogy with that species of salts.

When a mixture of peroxide of iron and potash is kept for some time at a strong red heat, a brown mass is obtained, which, treated with water, gives a solution of a very fine violet-red colour; this compound is very soluble in water, but a large quantity of water eventually decomposes it: it becomes insoluble in water which is very alkaline, but gives a brown precipitate, which dissolves readily in pure water, and yields a solution of a fine purple colour. It appears to be much less permanent than manganate of potash; under certain circumstances it decomposes at common temperatures into oxygen gas, which escapes, and peroxide of iron which precipitates; and a solution of free potash remains, which is perfectly colourless. At 212° Fahr. it undergoes similar decomposition instantaneously: all organic substances decompose it; consequently it is impossible to filter the solution.

This compound may be more readily prepared, and in a few minutes, by calcining, at a high temperature, a mixture of nitre, potash and peroxide of iron; or by heating a mixture of peroxide of potassium and peroxide of iron. This compound was also formed in the humid way by passing a current of chlorine gas into a very concentrated solution of potash, holding peroxide of iron in suspension.

The fact stated seems to indicate the existence of a body more highly oxygenated then peroxide of iron. M. Fremy could not, however, isolate any such compound; for when the solution is treated with an acid, and the potash is saturated, oxygen is disengaged and peroxide of iron is precipitated; if the acid is in excess, the peroxide of iron is dissolved, and a salt of this oxide is formed.—*Journal de Pharmacie*, tom. xxvii. p. 97.

PREPARATION OF GUAIAIC ACID. BY M. THIERRY.

To prepare this acid M. Thierry dissolves the pure guaiacum resin of commerce in the requisite of alcohol, sp. gr. (56 c.); the tincture is to be filtered, and three-fourths of the spirit used are to be distilled; when the operation is over there remains in the retort a yellowish liquor, above a quantity of resin; when cool the liquor is to be filtered.

This liquor is acid, and is to be saturated with barytes water, with which it forms a soluble salt; the liquor is to be evaporated to half in a water-bath, and the guaiacate obtained is to be decomposed by dilute sulphuric acid; and it is better that a little of the salt should remain undecomposed, rather than excess of acid should be used; for the latter during the concentration of the guaiaic acid reacts upon and decomposes a portion of it; if, however, any should have been employed, a little barytes water may be used to precipitate it; when the sulphate of barytes has been separated the solution is to be evaporated in a water-bath to the consistence of a syrup; the residue is to be put into a matrass, and the mixture is to be shaken repeatedly for some time; the æther dissolves the guaiaic acid, but does not act upon the extractive matter; the liquor is to be decanted, and when the æther is evaporated, the guaiaic acid is deposited in irregular mammillated masses on the sides of the vessel. The acid thus obtained is not however quite pure; it contains some resinous matter, and in order to free it from this it must be sublimed very cautiously.

This acid is obtained in the state of fine needles; it is soluble in water, in alcohol and in æther; it differs from benzoic and cinnamic acid in their being slightly soluble in water, whereas guaiaic acid is perfectly soluble in it.

M. Thierry has not yet analysed this acid, and he states that the substance noticed by M. Righini as guaiaic acid in the *Journal de Chémie Médicale*, 1836, is not truly this acid, but a resinous matter which has been saponified by magnesia, and which is precipitated when no longer held in solution by an alkali.—*Journal de Pharmacie*, tom. xxvii. p. 382.

LONDON INSTITUTION.

The Lectures for the season will commence on Monday, November 8th, when Mr. Grove (whose appointment to the Chair of Experimental Philosophy in this Institution we have already recorded) will deliver the first lecture of a Course on *Magnetism*. On Thursday, November 11, Mr. Brayley will begin a Course on *Meteorology*; and on Thursday, January, 6, 1842, Professor Grove will commence another Course, the subject of which will be *the Physical Elements of the Ancient Philosophers*. The series will also include lectures *on Manufactures*, by Mr. E. Cowper, of King's College; *on Rome*, by Dr. Vaughan; *on Painting*, by Mr. Haydon; *on Music*, by Mr. Gauntlett; *on the Fossil Remains of Extinct Animals*, by Professor Ansted, of King's College; *on Numismatics*, by Mr. J. Williams; *on America*, by Mr. Buckingham; and *on Shakspere*, by Mr. C. C. Clarke. Four Conversazioni will also be held, on the evenings of Wednesdays, January 19, 1842, February 16, March 16, and April 20.

Mr. Brayley's Course on METEOROLOGY will consist of Four Lectures; designed, principally, to illustrate the more recent observations and discoveries, and to review the present state of our knowledge respecting Igneous Meteors and Meteorites.

SCIENTIFIC BOOKS.

CONCHOLOGIA SYSTEMATICA, or complete System of Conchology: in which the Lepades and Mollusca are described and classified according to their Natural Organization and Habits; illustrated with 300 highly finished Copper Plate Engravings, by Messrs. SOWERBY, containing above 1500 figures of Shells. By LOVELL REEVE, F.Z.S., Member of the Cuvierian Society of Paris, &c.

The UNDULATORY THEORY, as applied to the Dispersion of Light. By the Rev. BADEN POWELL, M.A., F.R.S., F.G.S., Savilian Professor in the University of Oxford.

A MANUAL OF ELECTRICITY, MAGNETISM AND METEOROLOGY. By DIONYSIUS LARDNER, D.C.L., F.R.S., &c.

A LIST of the GENERA of BIRDS, with their Synonyma, and an indication of the typical species of each genus, by GEORGE ROBERT GRAY. 2nd edit. 8vo.

A New Process for purifying the Waters supplied to the Metropolis by the existing Water Companies, rendering each water much softer, preventing a fur on boiling, separating vegetating and colouring matter, destroying numerous water-insects, and withdrawing from solution large quantities of solid matter, not separable by mere filtration. By THOMAS CLARK, Professor of Chemistry in the University of Aberdeen. 2nd edit. 8vo.

An Illustrated Catalogue of British Plants, arranged according to the Natural Orders, with references to Smith, Hooker, Lindley, Macreight, &c. By C. E. SOWERBY, A.L.S. To be continued monthly.

An Essay on Single Vision. By JOHN THOMAS WOODHOUSE, M.D., Fellow of Gonville and Caius College, Cambridge.

A Practical Treatise on the Manufacture and Distribution of Coal-Gas; its Introduction and progressive Improvement; illustrated by Engravings from working drawings, with general estimates. By SAMUEL CLEGG, Jun., Civil Engineer.

Preparing for Publication.

The GEOLOGY AND MINERALOGY OF ENGINEERING; comprehending the elements of the sciences of Œconomic Geology and Mineralogy applied to the Arts. By E. W. BRAYLEY, Jun., Fellow of the Linnæan and Geological Societies; Associate of the Institution of Civil Engineers; Corresponding Member of the Royal Geological Society of Cornwall, &c.

METEOROLOGICAL OBSERVATIONS FOR SEPT. 1841.

Chiswick.—Sept. 1. Very fine. 2. Slight fog: very fine: clear. 3. Very fine: showery: heavy rain. 4. Stormy and wet. 5. Cloudy and fine: rain: cold fog. 6. Dense fog: hazy: foggy at night. 7. Foggy: rain. 8. Fine. 9. Overcast. 10, 11. Foggy: very fine. 12, 13. Very hot for the period of the season. 14. Dry haze: very fine. 15. Very fine: rain at night. 16—20. Very fine. 21. Hazy: very fine. 22. Rain: very fine. 23. Heavy rain. 24. Cloudy: rain. 25. Showery. 26. Showery: stormy with rain at night. 27. Fine: lightning and very heavy rain at night. 28. Rain: boisterous. 29. Boisterous: clear at night. 30. Boisterous, with rain: clear and fine.

Boston.—Sept. 1. Foggy: rain yesterday P.M. 2. Fine. 3. Rain: heavy rain P.M. 4. Cloudy: stormy, with rain P.M. 5. Cloudy: rain P.M. 6. Fine. 7. Cloudy: rain P.M. 8. Cloudy. 9. Fine. 10. Cloudy: thermometer 74° three o'clock. 11. Cloudy. 12. Fine: thermometer 80° half-past eleven o'clock A.M. 13. Fine: thermometer 74° three o'clock P.M. 14. Fine. 15. Cloudy. 16. Fine: rain A.M. 17. Fine. 18. Foggy. 19—21. Cloudy. 22. Cloudy: rain A.M. 23. Rain A.M. 24. Cloudy: rain early A.M. 25. Fine. 26. Cloudy: rain early A.M.: rain P.M. 27. Fine. 28. Stormy: rain early A.M. 29. Stormy. 30. Cloudy: rain early A.M.: rain P.M.

Applegarth Manse, Dumfries-shire.—Sept. 1. Fair till P.M., then rained. 2. Continued rain P.M. 3. Fair and fine. 4. Fair and fine, but cloudy A.M. 5. Fair and fine. 6. Fair and fine: hoar-frost A.M. 7. Cloudy A.M: rain P.M. 8. Wet A.M.: cleared up. 9. Wet nearly all day. 10. Wet throughout. 11. Cloudy A.M.: wet P.M. 12. Hot sun: fiery wind: thunder. 13. Clear: fiery wind: thunder. 14. Showery A.M.: wet P.M.: thunder. 15. Cloudy, but fair: thunder. 16. One shower. 17. Fair and clear. 18, 19. Fair and fine. 20, 21. Fair and fine, though windy. 22. Shower in the afternoon. 23. Fair and fine. 24, 25. Showers. 26. Wet A.M.: cleared and was fine. 27. Fair but threatening. 28. Heavy showers. 29. Heavy rain all day: thunder. 30. One or two slight showers.

Sun shone out 26 days. Rain fell 15 days. Thunder 5 days. Frost, hoar, 2 days.

of the Horticultural Society at Chiswick, near London; by Mr. Veall *at Boston, and by* Mr. Dunbar *at Applegarth Manse, Dumfries-shire.*

Days of Month. 1841. Sept.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8¾ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dumfries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8¾ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
○ 1.	30·068	30·092	29·991	29·49	29·78	29·68	59·7	72·8	52·7	72	36	53	64	40½	NW.	S.	calm	S.	...	...	·23	...	59
2.	29·896	29·843	29·752	29·37	29·55	29·49	60·2	67·7	51·3	73	48	57	60	53	E.	S.	calm	S.	...	...	...	...	57
3.	29·744	29·691	29·439	29·20	29·57	29·60	65·3	69·8	54·7	72	50	56	59½	49	SSE.	SW.	calm	WNW.	...	·48	·10	...	58
4.	29·546	29·775	29·444	28·88	29·58	29·61	54·7	73·4	52·7	55	41	60·5	54	46½	W var.	W.	SW.	NNE.	·536	·32	·84	...	57
5.	29·874	29·823	29·753	29·36	29·68	29·68	53·2	57·6	47·2	56	36	51	59	36	S.	E.	calm	NW.	·402	·04	·52	...	54
6.	29·762	29·725	29·704	29·17	29·65	29·63	52·5	56·3	42·8	61	37	48	58	33½	S.	S.	calm	SSW.	·019	...	·15	...	49
7.	29·800	29·734	29·582	29·31	29·51	29·37	55·7	58·7	45·8	62	49	50	56	34½	S.	S.	calm	ESE.	...	·17	...	0·37	50
☾ 8.	29·800	29·951	29·717	29·16	29·45	29·74	60·7	62·0	52·6	70	51	60	58½	48½	W.	SW.	W.	WSW.	·183	...	·13	...	54
9.	30·076	30·019	29·959	29·40	29·80	29·70	59·3	68·3	56·2	68	56	60	61	39½	E.	S.	calm	SE.	...	...	...	...	55
10.	30·066	30·002	29·985	29·31	29·73	29·71	63·8	67·4	59·2	74	55	64·5	61	56	WSW.	SW.	calm	SE.	...	...	...	...	59
11.	30·122	30·052	29·976	29·47	29·81	29·88	59·8	69·4	57·2	78	54	60	61½	55	W.	NE.	calm	SSE.	...	...	...	...	60
12.	30·012	29·923	29·598	29·36	29·80	29·75	68·4	74·7	60·6	84	56	70	81	55	E.	SE.	calm	ESE.	...	...	...	...	64
13.	29·932	29·843	29·815	29·30	29·80	29·79	68·3	78·5	61·0	79	59	66·5	80	58	E.	E.	S.	E.	...	...	...	...	64
14.	29·892	29·803	29·589	29·29	29·78	29·75	66·8	75·3	62·0	78	55	65	60	59½	ENE.	SE.	calm	SE.	...	...	...	1·02	65
● 15.	29·966	29·886	29·847	29·28	29·68	29·62	67·7	71·4	62·6	66	55	63	66	57	S.	S.	calm	SE.	...	·06	...	...	65
16.	29·910	29·876	29·820	29·27	29·63	29·70	65·2	73·0	60·4	71	41	62	65	56½	S.	S.	calm	SW.	·063	·01	...	...	62
17.	30·036	29·967	29·913	29·40	29·80	29·85	58·3	69·6	50·6	69	41	57	65	52	W.	W.	calm	S.	...	...	·15	...	57
18.	29·964	29·903	29·892	29·43	29·86	29·88	54·7	67·7	49·8	72	40	56	64	42	S.	S.	calm	E.	...	...	...	...	56
19.	30·070	30·037	29·972	29·52	29·95	30·00	60·5	66·2	50·7	74	54	58	66	43	ENE.	E.	calm	E.	...	...	...	...	54
20.	30·184	30·109	30·056	29·60	30·09	30·10	62·5	68·7	60·0	71	60	62	65½	50½	E.	SE.	E.	E.	...	...	...	...	61
21.	30·018	29·969	29·700	29·54	30·06	29·90	60·3	70·0	59·7	70	56	59	59½	50	ENE.	E.	E.	ESE.	...	·10	...	0·05	61
☽ 22.	29·692	29·627	29·613	29·20	29·70	29·55	59·4	67·2	58·2	70	52	58·5	59	49	SE.	S.	E.	E.	·087	·30	...	...	61
23.	29·688	29·617	29·578	29·14	29·47	29·47	55·7	66·4	55·4	66	49	56	62½	50	S.	S.	calm	E.	·602	·23	·44	...	59
24.	29·578	29·526	29·480	29·05	29·43	29·40	58·3	62·2	56·0	64	50	57	57½	52	SSE.	SW.	calm	NE.	·366	·25	·04	...	53
25.	29·468	29·481	29·398	28·92	29·29	29·26	57·5	63·3	53·6	65	50	57	60	48	S.	SW.	N.	ENE.	·158	·30	·19	...	57
26.	29·504	29·480	29·422	28·94	29·29	29·30	58·2	63·6	53·7	64	50	54	60	48	SE.	S.	calm	E.	·080	·27	·03	...	56
27.	29·548	29·567	29·471	28·97	29·40	29·30	56·8	62·4	54·0	67	54	55	62	39	W.	W.	calm	ESE.	·327	·57	·02	...	57
28.	29·404	29·381	29·310	28·75	29·00	28·94	58·2	64·7	56·5	64	56	59	61½	51½	S.	S.	S.	S.	·438	·15	·39	1·95	58
29.	29·252	29·267	29·167	28·52	28·70	28·90	61·2	66·8	58·0	65	54	59	58	47	S.	S.	W.	SSW.	·088	·35	...	...	59
○ 30.	29·248	29·413	29·172	28·63	29·00	29·10	58·7	64·4	56·0	65	44	57	61	51	S var.	SW.	calm	SW.	·366	·11	·20	...	58
Mean.	29·804	29·746	29·670	29·20	29·594	29·594	60·1	67·3	51·7	68·83	49·66	58·3	62·2	45·3					Sum. 3·715	3·71	3·43	3·39	Mean. 58

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[THIRD SERIES.]

DECEMBER 1841.

LXII. *First Sketch of some of the principal Results of a Second Geological Survey of Russia. Communicated by* Roderick Impey Murchison, *Esq., F.R.S., President of the Geological Society.*

To the Editor of the Philosophical Magazine.

Dear Sir,

IT was my earnest wish to have complied earlier with your request when I left this country, to send you from the spot some account of my distant wanderings; but the desire to avoid communicating early conceptions which might be modified by subsequent observation, induced me to stay my pen until I could offer something worthy of a place in the Philosophical Magazine. The short sketch which follows was written at Moscow near the close of the journey, and is, with some very slight alterations, the translation of a letter addressed to M. Fischer de Waldheim, the venerable and respected President of the Society of Naturalists of that metropolis. Since then, besides the official report to the Minister of Finance, the Count de Cancrine, I have submitted to His Imperial Majesty, a tabular view of all the formations in Russia, accompanied by a general map and a section from the Sea of Azof to St. Petersburgh. These documents, which will be engraved in the course of the winter, are to be considered only as the prelude to a long memoir with full illustrations of the organic remains, mineral structure and physical features of the country, which will be laid before the Geological Society of London, as soon as, with the assistance of my fellow-labourers, I shall have prepared the materials for the public eye. In the mean time the friends of science must be happy to learn, that the Emperor, his ministers and officers have powerfully and kindly contributed to these results by every possible aid and support which geologists could receive. Desirous that this inquiry should be rendered as perfect as circumstances will admit, His Imperial Majesty has graciously authorized the Minister of Finance, the Count de Cancrine, to permit Count Keyserling to visit this country during the winter, to cooperate with myself, whilst General Tcheffkine, the chief of the Staff of the Mining Corps, and so well known to many of my English friends, has obtained permission for Lieut. Koksharoff to be among us for a season, to complete his studies, and acquire a correct knowledge of those *British* strata with which the deposits of our ancient allies and kind friends have been compared.

I remain, dear Sir, yours most faithfully,

RODERICK IMPEY MURCHISON.

16 Belgrave Square,
Nov. 5, 1841.

Letter to M. Fischer de Waldheim, Ex-President of the Society of Naturalists of Moscow.

(*Translation.*)

MY DEAR SIR, Moscow, Oct. 8, 1841.

As you have taken a lively interest in the success of the geological expedition which I have just completed, accompanied by my friends M. de Verneuil, Count de Keyserling, and Lieutenant Koksharoff, I hasten to communicate to you some of its chief results; and I do so with real pleasure, because in requesting you to present them to the Society of Naturalists of Moscow, I acquit myself of a duty towards a distinguished body which has done me the honour of placing my name in the list of its foreign members.

The wide extension in the North of Russia of the Silurian, Devonian and Carboniferous Systems, as proceeding from the last year's survey, by the same observers and our friend the Baron A. de Meyendorf, is already known to you from the abstracts of memoirs communicated to the Geological Societies of London and Paris. Our principal objects this year were,—1st. To study the order of superposition, the relations and geographical distribution of the other and superior sedimentary rocks in the central and southern parts of the empire. 2nd. To examine the Ural Mountains, and to observe the manner in which that chain rises from beneath the horizontal formations of Russia. 3rd. To explore the carboniferous region of the Donetz, and the adjacent rocks on the Sea of Azof.

Our last year's survey had pretty nearly determined the limits of the great tract of carboniferous limestone of the North of Russia. On this occasion we have added to its upper part that remarkable mass of rock which forms the peninsula of the Volga near Samara, and which, clearly exposed in lofty, vertical cliffs, and charged with myriads of the curious fossils *Fusilina*, constitutes one of the striking features of Russian geology.

The carboniferous system is surmounted, to the east of the Volga, by a vast series of beds of marls, schists, limestones, sandstones and conglomerates, to which I propose to give the name of "Permian System," because, although this series represents as a whole, the lower new red sandstone (*Rohte todte liegende*) and the magnesian limestone or *Zechstein*, yet it cannot be classed exactly (whether by the succession of the strata or their contents) with either of the German or British subdivisions of this age. Moreover the British lithological term of lower new red sandstone*, is as inapplicable to the great masses of marls, white and yellow limestones, and gray copper grits, as the name of old *red* sandstone was found to be in reference to the schistose black rocks of Devonshire.

To this "Permian System" we refer the chief deposits of gypsum of Arzamas, of Kazan, and of the rivers Piana, Kama and Oufa, and of the environs of Orenbourg; we also place in it the saline sources of Solikamsk and Sergiefsk, and the rock salt of Iletsk and other localities in the government of Orenbourg, as well as all the copper mines and the large accumulations of plants and petrified wood, of which you have given a list in the 'Bulletin' of your Society (anno 1840). Of the fossils of this system, some undescribed species of *Producti* might seem to connect the Permian with the carboniferous æra; and other shells, together with fishes and Saurians, link it on more closely to the period of the Zechstein, whilst its peculiar plants appear to constitute a Flora of a type intermediate between the epochs of the new red sandstone or "trias" and the coal-measures. Hence it is that I have ventured to consider this series as worthy of being regarded as a "System."

The overlying red deposits which occupy a great basin in the governments of Vologda and Nijni Novogorod, have not as yet been found to contain any organic remains except minute *Cyprides* and badly preserved *Modiolæ*; but when we take into consideration their thickness, geological position, and mineral characters, we are disposed to think that they may at some future day be identified with a portion of the "Trias" of German geologists. I am strengthened in this opinion by Count Keyserling's discovering, during our tour at Monte Bogdo, certain fossils which are unknown in other parts of Russia, but which are associated with the *Ammonites Bogdoanus* already described by Von Buch, and which that distinguished geologist refers to the type of the muschelkalk.

---

* See Silurian System p. 54.

True lias does not exist in Russia, as Von Buch had decided from an examination of fossils sent to him, but the Jurassic or oolitic series is divisible into two stages. The lowest of these, which is much more developed than the upper, never occupies any considerable tract of country, being either distributed in patches, or hidden by newer accumulations. From the eastern flanks of the Ural chain in the 64° of N. latitude to the Caspian Sea, it preserves nearly the same mineral and fossil characters. This formation represents the inferior and middle oolite. The ferruginous sands, calcareous grits, and black schists of the Moskwa are of this age; and also those beds which we examined last year on the Volga between Kostroma and Kinshma, at Makarief upon the Unja, as well as those shales and sands which we have seen this year in many other localities, particularly between Arzamas and Simbirsk, between Syzran and Saràtoft, at Saragula, and on the river Ilek near Orenbourg.

The upper oolitic group occurs in several situations along the Donetz, where it was first recognized by Major Blöde. It is calcareous, often oolitic, of light yellow colour, and contains many *Trigoniæ*, *Nerineæ*, &c., which enable us to compare it with the upper Jura of the Germans, or Portland and Coral rag division of my own country.

The cretaceous system, though composed of very different beds of marls, white chalk, sands and grits (sometimes green), offers for the most part the fossils of the white chalk of Europe, such as the *Inocerami* (Catillus), *Belemnites mucronatus*, *Ostræa vesicularis*, *Terebratula carnea**.

Above the cretaceous system, we have not been able to discover in any part of Russia, except in the Crimæa, the "nummulite limestone" which there sets on, and acquires a great importance in its range through Georgia, Egypt, and the Mediterranean basin.

The equivalents of the lower tertiary formations (Eocene of Lyell) seem to exist in one part only of your country (S. of Saràtoft). On the other hand, the middle and upper tertiaries (Miocene and Pleiocene) cover large surfaces on the Lower Volga, in Podolia, Volhynia, and also along the shores of the Sea of Azof and the Black Sea, where the youngest of these strata, very much resembling the "upper crag" of Norfolk, are beautifully displayed.

I have not time to enter upon the numerous and interesting phænomena of the Ural Mountains, the examination of which occupied us nearly three months. We there studied alternately the wonders of the gold alluvia, the sites of the entombment of your great mammalia, and sought for the causes of the astonishing metamorphism of the sedimentary rocks of that chain. For an explanation of the last class of phænomena, the works of Humboldt and Gustaf Rose must always be consulted. I will on this occasion simply say, that far from being *primitive*, as was supposed, this chain, with the exception of its eruptive masses, is entirely composed of *Silurian*, *Devonian* and *Carboniferous* rocks, more or less altered and crystallized, but in which nevertheless we have been able to recognize in a great number of localities my own *Pentamerus Knightii*[*], and many fossils which clearly define the age of the other strata. These rocks, though much broken up, are arranged in parallel bands, the mean direction of which in the North Ural is from N. and by W. to S. and by E., whilst in the South Ural, trending N. and S., they assume a fan-shaped arrangement, spreading out towards the southern steppe of the Kirghis, where, interlaced with porphyries and other trap-rocks, they are often converted into the far-famed jaspers of this region.

---

* After this letter was written, we found in the collection of Professor Eichwald at St. Petersburgh, a fine specimen of Exogyra and other fossils in a green sandstone from the Lower Volga, sent to him from a locality well known to us, which leaves little doubt of the existence also of a true representative of our greensand.—R. I. M.

Still less can I now pretend to treat of the great carboniferous region of the Donetz; for without entering into details concerning this southern tract, so valuable to the future interests of Russia, I cannot render it the justice which it merits. Still I may say to you as a geologist, that its numerous beds of coal (bituminous and anthracitic), with its grits and shales, are completely subordinate to the mountain limestone series, and represent in no sense the coal-fields of Great Britain, Belgium, and France.

In concluding, however, I must tell you of a very interesting discovery we made in returning from Taganrog to Petersburgh. Count Keyserling took the line of Voroneje and the Don, and M. de Verneuil and myself that of Koursk, Orel and the river Oka, and on meeting at Moscow our results completely agreed.[†] It was, as you know, generally believed up to this moment, that central Russia presented a regular succession from older to younger deposits as you proceeded from north to south. This is not the case. A great axis of Devonian rocks or old red sandstone, having a width of at least 120 miles, rises in the heart of the country around Voroneje and Orel, and stretches to the W. N. W., in which direction it probably connects itself with deposits of the same age in Lithuania and in Courland. This discovery seems, indeed, to have an intimate relation to one which we made in entering Russia early in the spring, near to Schavli in Lithuania, of much red ground and a band of upper Silurian rocks. In fact it also explains the cause of the great difference which exists between the deposits of the carboniferous basin of the Donetz and those of your Moscow region, now proved to constitute a vast *basin*. For as the two seas, in which these deposits were accumulated from high antiquity, were separated by the ancient lands in question, so must we infer that the conditions and nature of their shores, their rivers, their currents and bottoms (on which of course the nature of marine deposits depend), must have been essentially different.

---

[*] Silurian System, p. 615.

[†] Colonel Helmersen, so distinguished for his geographical and geological researches in Russia, also examined the tract near Orel in the course of the summer, and had come to the same conclusions as our party. I was however unacquainted with his opinion when I wrote this letter.—R. I. M.

This discovery also proves the symmetry of the opposite edges of the *Moscow basin*; since in advancing from the governments of Tula and Kaluga on the south, we see the same ascending order as that which we before described in the Waldai Hills on the north. In both tracts the Devonian or old red rocks, with *Holoptychius Nobilissimus*, and many fishes and shells of that system well known in the British Isles*, pass under the lowest strata of the carboniferous æra, and serve as a base line to those thin beds of poor coal associated with *Unio sulcatus* and *Productus gigas* (*hemisphericus*, Sow.), which are at present the subject of new researches on the part of the Russian Government.

The enormous space we traversed and examined, in all between 13 and 14 thousand miles, might well astonish you, if I did not assure you, that the arrangements for this journey, undertaken under the auspices of the Minister of Finance, Count de Cancrine, were admirably prepared by General Tcheffkine, whose clear directions, united to that spirit of hospitality which characterizes all Russians, and above all the inhabitants of the Ural and Siberia, rendered every enterprise feasible, and enabled us to overcome every obstacle.

I shall communicate to you at a later date, and before our large memoir is prepared, the general table of the order of superposition of all the formations of Russia, with sections†.

Accept, dear Sir, the assurance of the affection and esteem of your devoted servant,

RODERICK IMPEY MURCHISON,
President of the Geol. Society of
London.

To His Excellency M. Fischer de Waldheim.

---

* See Silurian System, p. 599.

† These documents, which were laid before His Imperial Majesty in MSS., are now in the hands of the engraver.

LXIII. *On the Theory of Storms, with reference to the Views of* Mr. Redfield. *By* ROBERT HARE, *M.D., Member of the American Philosophical Society, Professor of Chemistry in the University of Pennsylvania*[*].

1. MR. REDFIELD'S idea, that tornadoes and hurricanes are all whirlwinds, involves some improbabilities.

2. It requires that during every hurricane there should be blasts of nearly equal force coinciding with every tangent which can be applied to a circle. Thirty-two ships, equidistant from the axis of gyration and from each other, should *each* have the wind from a different point of the compass with nearly equal force. The only modification of which this view of the case admits, is that resulting from the progressive motion which tends to increase the velocity of the wind on the side on which this motion concurs with that of the whirl, and to produce upon the opposite side a corresponding diminution. Moreover, as respects any one station, the chance would be extremely unfavourable that the same hurricane should twice proceed from the same quarter: and yet in the course of time it would be felt, at any station, to proceed from many different directions, if not from every point of the compass.

3. Mr. Redfield has alleged, that he observed proofs of gyration in the effects of the New Brunswick tornado; but I think that the survey of Bache and Espy shows that it would not be consistent with the facts to suppose such a motion, unless contingently; and that it could only be a casual effect of the currents rushing towards the axis of the tornado[†].

4. Being of opinion that calorific expansion is inadequate to explain the afflux of wind towards the equator, it is alleged by Mr. Redfield, "that the space previously occupied by the atmosphere so left behind, is, by the centrifugal action of the earth's rotation, constantly supplied from higher latitudes."

5. I presume that the meaning of this allegation is, that the centrifugal force communicated to the air at the equator by the diurnal revolution of the earth, lessening the gravity of the air thus affected, causes it to rise, and give place to those portions of the atmosphere which, existing where the diameter of the earth is less, have less rotary motion. Admitting an afflux to arise in this way, could it have any other effect than that of accumulating air over the equator, compensating by quantity and altitude for the loss of weight arising from a greater centrifugal force pertaining to that region? But, on

---

[*] Communicated by the Author.

[†] See fifth volume of the American Philosophical Transactions.

the other hand, if we attribute the ascent of the air at the equator to heat, the theory of calorific circulation will account for the continuance of the process.

6. In ascribing the prevalence of westerly winds in the upper regions of the atmosphere to the deflection of the trade winds by our mountains, Mr. Redfield's explanation harmonizes with the theory of Halley. In fact, is it not reasonable, that, as the water accumulated by these winds in the Gulf of Mexico is productive of a gulf-stream, there should be an *aërial* accumulation and current corresponding with that of the aqueous current, which is designated by the name above-mentioned?

7. But not perceiving that the trade winds cannot be explained without the agency of temperature, Mr. Redfield, in the following paragraph, rejects the influence of heat:—

8. "To me it appears that the causes of the great storms may be considered to indicate with entire certainty the great law of circulation in our atmosphere; and that the long-cherished theory, which is founded on calorific rarefaction, must give place to a more natural system of winds and storms, founded mainly upon the more simple conditions of the great laws of gravitation."

9. It would seem from this paragraph as well as others, that Mr. Redfield admits of no other cause of atmospheric currents besides that of gravitation. But in the absence of calorific and electrical reaction, what other effect could gravitation have, unless that of producing a perfect state of inert quiescence?

10. It is remarkable that the author, after ascribing the trade winds to momentum, as the antagonist of gravitation, loses sight of it in this summing up of the causes of atmospheric currents!

11. If, as Mr. Redfield alleges, the minuteness of the altitude of the atmosphere, when compared with its horizontal extent, be an objection to any available currents being induced by calorific rarefaction, wherefore, for the same reason, should not momentum, or any other cause diminishing or counteracting the influence of his chosen agent, gravity, be equally inefficient?

12. Assuming that the motion of the air in hurricanes is always gyratory, Mr. Redfield considers gyration as a *cause* of these terrible meteors! How far his language on this subject is reasonable or consistent, may be seen from the following paragraph, which I quote from one of his essays, published in Silliman's Journal for 1834, vol. xxv. page 125:—

13. "Notwithstanding these general and determinate ho-

rizontal movements, the equal distribution of the atmosphere over the surface of the globe, which results from gravitation, tends to prevent any very rapid or violent motion in any specific direction, and consequently to prevent violent and destructive winds. But owing to the tendency of all fluid matter to run into whirls or circuits, when subject to the influence of unequal or opposing forces, a rotative movement of unmeasured violence is sometimes produced. This peculiar movement, which in its most active state is sometimes distinguished by the name of tornado or hurricane, assumes every possible variety of position, appearance, velocity and extent, and is the only known cause of violent and destructive winds or tempests."

14. Agreeably to this paragraph, gravitation, in lieu of being, as previously alleged, the main basis of winds and storms, tends to produce that equal distribution of the atmosphere over the surface of the globe, on which I have insisted.

15. But if neither gravity, nor calorific expansion, nor electricity be the cause of winds, by what *are* they produced?

16. He alleges that the fluid matter has a tendency to run into whirls or circuits, when subject to the influence of unequal or opposing forces, and that in this way a rotative movement of unmeasured violence is sometimes produced.

17. If this were true, evidently whirlpools, or vortices of some kind, ought to be as frequent in the ocean, as, agreeably to his observation, they are found to be in the atmosphere. The aquatic gulf-stream, resulting from the impetus of the trade winds, ought to produce as many vortices in its course as the aërial currents derived from the same source, especially as in the ocean the great laws of gravitation have full liberty to act without any important interference from calorific changes, to which the advocates of the agency of such changes in producing wind will not ascribe much efficacy, where non-elastic liquids are in question.

18. There are few vortices or whirlpools in the ocean, because there are in few cases descending currents towards which the surrounding waters are concentrated. Of course, vertical currents cannot arise from any imaginable cause.

19. The conflicts of "*opposing or unequal forces*" do not produce curvilinear motion, unless there be a successive deflection, as in the case where it results from centripetal force, or the influence of gravity upon a projectile. If one of two *directly opposite* forces be less than the other, retardation will ensue, and a lateral current or currents carrying off the excess of momentum. If currents encounter each other *obliquely*, a diagonal current will result. I doubt if a whirlpool ever

takes place without a centripetal force resulting from an hiatus.

20. But the author has not informed us how these unequal or opposing forces are generated in the atmosphere. Without any assigned cause, he appeals to "certain opposite and unequal forces by which a rotative movement of unmeasured violence is produced." This rotative movement, although alleged, as above, to be an *effect*, is stated immediately afterwards to be "the only known cause of violent and destructive winds or tempests."

21. In a memoir on the causes of tornadoes and water-spouts, and in some subsequent communications published in the Transactions of the American Philosophical Society, and republished in Silliman's Journal, various facts and arguments were mentioned, tending to prove that the proximate cause of the phænomena of a tornado is an ascending current of air, and the afflux of wind from all points of the compass to supply the deficiency thus created.

22. In this mode of viewing the phænomena, no difference of opinion exists between Bache and Espy and myself, however we may differ respecting the cause of the diminution of atmospheric pressure within the track of a tornado, which gives rise to the ascending current.

23. I adduced several facts, upon the authority of the skilful survey made by those gentlemen, proving that the effects were in some cases inconsistent with the existence of a *whirl*; and I mentioned one which could not be explained without attributing it to a gyratory force. I was led to consider gyration as a contingent, not an essential feature in the meteors in question.

24. It appeared reasonable to suppose that the confliction of confluent streams of air rushing towards an axis moving progressively, might be productive of a whirling motion. The contortion of six feet of the upper part of a brick chimney upon the lower portion, so as to cause the corners of either portion to project over the sides of the other, was deemed inexplicable without ascribing it to gyratory force. Subsequently, however, it occurred to me that this fact was more likely to be the result of a *local* than of a *general* whirl; since in the latter case, the chimney could not have been twisted as described, without being precisely at the centre of the whirlwind. That such could have been its position, appears to me extremely improbable; and had it been so situated, as the whirlwind was estimated to be moving progressively at the rate of seventeen miles per hour, it is to me incomprehensible how the portion which was dislocated could have escaped an overthrow. Evidently, although twisted upon its base while

concentric with the gyration, it would in one second of time have been twenty feet upon the windward side of it, consequently subject to the tangential force of the whirlwind. I adduced this, as well as other facts, to prove that in tornadoes and hurricanes there are local whirls, causing bodies which are of a nature to favour electrical discharge to be particularly affected; a fact which is admitted by Mr. Redfield, was considered by Espy and Bache, as well as myself, to be irreconcilable with the idea that a general whirling motion is essential to tornadoes. I allude to the circumstance, that when several trees were prostrated one upon the other, the uppermost was found to have fallen with the top directed towards the point towards which the meteor was moving; while the direction in which the lowermost trees were found to have fallen indicated that they were overthrown by a force in a direction precisely the opposite of that which had operated upon those above-mentioned.

25. Mr. Redfield has not made any effort to show how the trees could have been piled upon each other, as described, but, on the contrary, admits that a whirlwind would blow oppositely, on opposite margins of the whirl. As this appears to me quite evident, I cannot understand how the opposite forces belonging respectively to the different sides of the whirlwind, can be made to bear successively upon one spot, so as to cause trees to fall in diametrically opposite directions.

26. Another fact, irreconcilable with a general whirling motion, was adduced by Messrs. Espy and Bache. One of the four posts, upon which a frame building was supported, was first moved towards the tornado, as it advanced; in the next place as it moved away, so as to make two furrows in the ground. In the interim the frame was protected by a larger building, which intervened between it and the tornado. I am utterly unable to understand how the transient tangential forces of a whirlwind blowing oppositely, on the opposite margins of its track, could thus move the post in question, so as to make two distinct furrows in the ground indicating two successive impulses, in directions of which one was at right angles with the other. Mr. Redfield admits that "the confused directions of fallen bodies is distinctly recognized by all the parties to this inquiry." Conceding that amid this confusion he has been enabled, by a survey, to show that the directions in which certain trees fell are consistent with their having been subjected to a whirlwind, it does not demonstrate gyration to be an essential feature of tornadoes. It is sufficiently accounted for by considering it as a fortuitous consequence of the conflict of currents rushing into a rarefied vortex.

27. Mr. Redfield adopts the singular determination of not noticing the "insuperable difficulties" of the hypothesis which he has undertaken to set aside. The advocates of the disputed hypothesis are not aware of any such difficulties: is it correct to allege their existence without mentioning the facts and arguments which justify this allegation? Without repeating here the evidence and the reasoning which I have already published on this subject, I will advert to one fact which is utterly irreconcilable with Mr. Redfield's "rotary theory." I allude to the statement of a most respectable witness, that while the tornado at Providence was crossing the river, the water, which had risen up as if boiling within a circle of about 300 feet, subsided as often as a flash of lightning took place. Now supposing the water to have risen by a deficit of pressure resulting from the centrifugal force of a whirl, how could an electrical discharge cause it to subside?

28. I have already, I trust, sufficiently shown that the abortive explanation which Mr. Redfield dignifies with the title of "his theory of rotary storms," amounts to no more than this: that certain imaginary, nondescript, unequal and opposing forces produce atmospheric gyration; that these gyrations, by their consequent centrifugal force, create about the axis of motion a deficit of pressure; and hence the upward force displayed by tornadoes and hurricanes. I cannot give to this alleged theory the smallest importance, while the unequal and opposing forces upon which it is built remain in perfect obscurity, the author having disclaimed both the agency of heat and electricity.

29. But admitting a whirlwind to be produced, not by a deficit of pressure about the axis, but by unequal and opposing forces acting externally in any competent way whatever, it is perfectly evident that any deficit of pressure about the axis consequent to the resulting centrifugal force, could only cause a descending aërial current, while it could not tend in the slightest degree to carry solids or liquids aloft.

30. It must be obvious that the stratum of air on the earth's surface, partaking of the circular motion, must also partake of the centrifugal momentum, and of course would have a disposition the very inverse of that which would cause them to rush towards the axis; while heavier bodies being surrounded by the rarer mediums consequent to the whirl, would have their gravity less counteracted than usual. I cannot help thinking, that as respects the application of his "rotary theory" to explain tornadoes, these arguments will amount to a "*reductio ad absurdum.*"

31. Mr. Redfield infers that the whirlwinds, of which he as-

sumes the existence, have a property which he alleges to be observable in "all narrow and violent vortices, viz. a spirally involute motion quickened in its gyrations as it approaches towards the centre of the axis or whirl."

32. But is it not evident, that if any mass of matter be made to revolve by unequal and opposing forces, or by any other than those resulting from the centripetal force, caused, as already described, by an ascending current, the gyration will not quicken in proportion as the gyrating matter may be nearer the centre, but on the contrary will be slower as the distance from the axis may be less? It appears to me, that the only case in which gyration is found to quicken in proportion as the matter involved approaches the vortex, is that which results from the confluence caused by an ascending or descending concentric current. So far therefore as Mr. Redfield's observations confirm the idea that the whirling motion in tornadoes quickens towards the centre, it tends to confirm the opinions which he combats, and to refute those which he upholds.

33. To conclude whether or not the efforts which I have made, to show that the phænomena of tornadoes and hurricanes arise from an electrical discharge by convection, be justifiable, I think it will be conceded that any theory of storms which overlooks the part performed by electricity must be extremely defective.

34. Both by Messrs. Espy and Redfield the influence of this agent in the phænomena of nature is entirely disregarded, although with the storms, which have been especially the subject of their lucubrations, thunder, lightning, and convective discharge are most strikingly associated.

35. I will conclude with subjoining the following propositions, inferences, or allegations; which are so evident to my mind, that I am at a loss to understand that they have not had a similar influence upon the minds of all who are conversant with the science of electricity.

36. Our experiments make us familiar with two processes of electrical discharge. In one of these electricity passes in the form of sparks or flashes; in the other it may be conveyed without any perceptible evolution of light, by the alternate or successive contact of intervening bodies with the excited surfaces, as, for instance, by means of pith-balls, pendula, or a blast of air. The former process has been designated by Faraday as disruptive, the latter as convective discharge.

37. The disruptive process being exemplified by lightning,

the magnificent apparatus of nature by means of which this awful phænomenon is displayed, may be supposed competent to produce convective discharge upon a scale of proportionable magnitude, as exhibited in tornadoes and hurricanes.

38. As bodies oppositely electrified attract each other, *à fortiori*, attraction must always exist between any bodies sufficiently electrified for an electric discharge to take place between them. This law may be illustrated by means of an instrument called Cuthbertson's electrometer. Hence the rising of water within the track of a tornado, and its subsidence on the passage of lightning, as observed by Mr. Allen near the city of Providence, Rhode Island, may be considered as resulting from the alteration of convective with disruptive discharge*.

39. By this observation of Mr. Allen, attraction is shown to have existed between an electrified stratum of air coated by clouds, and the oppositely electrified water of a subjacent river. It is reasonable to infer, that attraction, originating in the same way, operating upon the denser stratum of the atmosphere in the vicinity of the earth by counteracting gravitation, may cause that rarefaction by which houses are burst or unroofed,

---

* "The most interesting appearance was exhibited when the tornado left the shore, and struck the surface of the adjacent river. Being within a few yards of this spot, I had an opportunity of accurately noting the effects produced on the surface of the water.

"The circle formed by the tornado on the foaming water was about three hundred feet in diameter. Within this circle the water appeared to be in commotion, like that in a huge boiling caldron; and misty vapours, resembling steam, rapidly arose from the surface, and entering the whirling vortex, at times veiled from sight the centre of the circle, and the lower extremity of the overhanging cone of dark vapour. Amid all the agitation of the water and the air about it, this cone continued unbroken, although it swerved and swung around, with a movement resembling that of the trunk of an elephant whilst that animal is in the act of depressing it to the ground to pick up some minute object. In truth, the tapering form, as well as the vibrating movements of the extremity of this cone of vapour, bore a striking resemblance to those of the trunk of that great animal.

"Whilst passing off over the water, a distant view of the cloud might have induced the spectator to compare its form to that of a huge umbrella suspended in the heavens, with the column of vapour representing the handle, descending and dipping into the foam of the billows. The waves heaved and swelled whenever the point of this cone passed over them, apparently as if some magical spell were acting upon them by the effect of enchantment. Twice I noticed a gleam of lightning, or of electric fluid, to dart through the column of vapour, which served as a conductor for it to ascend from the water to the cloud. After the flash the foam of the water seemed immediately to diminish for a moment, as if the discharge of the electric fluid had served to calm the excitement on its agitated surface." — See Transactions of the American Philosophical Society, vol. vi.

and an upward current of tremendous force produced. We may also infer that bodies are carried aloft by the joint action of electrical attraction, and the vertical blast which it produces.

40. The effects upon the leaves of trees noticed by me after the tornado of New Brunswick, in 1835, and still more those subsequently observed by Peltier after that of Obatenage in 1839, cannot be explained without supposing them to have been the medium of an electric discharge *.

41. When a convective discharge takes place between a stratum of air in proximity to the earth, and a stratum in the region of the clouds, the greater density and pressure of the lower stratum will determine the current to rush up in a vertical direction.

42. Experience has demonstrated that electricity cannot exist on one side of an electric, without its existence simultaneously upon the other side. If the interior of a hollow globular electric be neutral, so will the outside be; but if the interior be either positively or negatively excited, the outside will be found in the one case negative, in the other positive.

43. The atmosphere consists of an electric in the hollow globular form; and as electricity is known to pervade the space within it occupied by earth, the principle in question must also pervade the space beyond that portion of the atmosphere which is sufficiently dense to insulate or to perform the part of an electric.

44. Thus there are three enormous concentric spaces, of which the intermediate one is occupied by an electric, while the innermost one and the outer one are occupied by conductors. The two last-mentioned may be considered as equivalent to two oceans of electricity, of which one may be called the celestial, the other the terrestrial electric ocean.

---

* "In this hasty account I have, with the intention of returning to this portion of the subject, omitted to speak particularly of its effect upon trees. All those which came within the influence of the tornado, presented the same aspect; their sap was vaporized, and their ligneous fibres had become as dry as if kept for forty-eight hours in a furnace heated to ninety degrees above the boiling point. Evidently there was a great mass of vapour instantaneously formed, which could only make its escape by bursting the tree in every direction; and as wood has less cohesion in a horizontal longitudinal than in a transverse direction, these trees were all, throughout one portion of their trunk, cloven into laths. Many trees attest, by their condition, that they served as conductors to continual discharges of electricity, and that the high temperature produced by this passage of the electric fluid, instantly vaporized all the moisture which they contained, and that this instantaneous vaporization burst all the trees open in the direction of their length, until the wood, dried up and split, had become unable to resist the force of the wind which accompanied the tornado."

45. When either electric ocean is minus, the other must be plus, and at the same time any intermediate stratum of the atmosphere inclosing a stratum of clouds must be charged by induction.

46. Between the concentric strata of air, severally bounding the celestial and terrestrial ocean, there must be an electrical attraction tending to counteract gravitation, and thus to influence the density and pressure of the lower stratum of the atmosphere.

47. The proximities of a stratum of clouds electrified by the celestial ocean, must cause an accumulation of electricity in any portion of the terrestrial surface immediately subjacent; and by counteracting gravitation cause a local diminution of atmospheric pressure, which it is well known is a precursor, and probably a cause of wind and rain.

48. Those common discharges of electricity which take place during hurricanes, may be easily accounted for by supposing that they result from discharges between the celestial and terrestrial electric oceans.

49. Thunder-clouds may owe their charges to the celestial ocean, either by induction or conduction. Auroras may be the consequence of discharges from one part of the atmosphere to another, through the rare conducting medium which is occupied by the celestial ocean; or they may result from discharges from other planets or seas, or from any part of space, however remote. Since, agreeably to Wheatstone's experiments, electricity flies with a velocity not less than that of light, space can create no obstacle to its passage.

LXIV. *On the Rotation of a rigid Body round a fixed Point.* *By* James Booth, *Esq., M.A., Principal of, and Professor of Mathematics in Bristol College*[*].

I. THE problem of the rotation of a rigid body round a fixed point, acted on by no forces, or round its centre of gravity influenced by the force of gravity alone, has been analytically solved by Lagrange, at least so far as to indicate the leading properties of such motion, and to reduce its determination to the calculation of two elliptic functions, one of the first, the other of the third order; but these analytical formulæ, as has been justly remarked, do not give us any clear idea of the motion during the period of rotation; they enable us to determine the position of the body at the end of a given time, but do not at all assist our conceptions in following the motion of the body during its rotation.

---

[*] Communicated by the Author.

Some years ago, however, this defect of the theory was removed in a memoir presented to the French Institute, by an author remarkable as well for the originality as the perspicuity of his views, in which he reduces the motion of the body to that of an ellipsoid whose centre is fixed, rolling on a fixed plane.

In the very brief extract which has been published of this memoir, Poinsot assumes an ellipsoid whose centre coincides with the fixed point, and whose semiaxes are proportional to the inverse square roots of the moments of inertia, round the three principal axes passing through the fixed point, coinciding with the axes of the ellipsoid; and thence by the known geometrical properties of this surface, and the tangent plane, deduces the nature of the motion, with great elegance and simplicity.

In the following pages an ellipsoid is assumed different from that of Poinsot, in which the squares of the axes are proportional to the moments of inertia round the principal axes of the body passing through the fixed point, coincident with the axes of this ellipsoid, which may be termed the *Ellipsoid of moments.*

It becomes proper to mention, that as the two ellipsoids of moments, that assumed by Poinsot, and the one here adopted, are *reciprocal surfaces*, all the properties of such motion may be indifferently deduced on either system, and then at once transferred to the other, by the known properties of reciprocal surfaces; a few examples of such transformation are given towards the close of this paper.

It appears, however, the more eligible course to deduce the general properties of rotatory motion, directly and independently, from the fundamental and acknowledged principles of mechanics, than to have recourse to the aid of reciprocal surfaces, with the relations of which many readers may not be familiar.

Previous to entering on the subject to which this paper is more particularly devoted, it may be well to state and prove a few general propositions bearing on the subject.

II. Let $O x$, $O y$, $O z$, be three rectangular axes passing through the fixed point O, the axe $O z$ being the instantaneous axis of rotation of the body*, X, Y, Z the forces which act on any particle $d m$ of the body, of which the coordinates are $x y z$; these forces being translated to the origin, are there equilibrated by the resistance of the fixed point O, while they generate the moments $(\mathrm{Y}x-\mathrm{X}y)\,dm$, $(\mathrm{Z}y-\mathrm{Y}z)\,dm$, $(\mathrm{X}z-\mathrm{Z}x)\,dm$, in the planes of xy, zy, and xz respectively; hence the sum of the moments generated by the forces which act on the whole mass are $\int(\mathrm{Y}x-\mathrm{X}y)\,dm$, $\int(\mathrm{Z}y-\mathrm{Y}z)\,dm$, and $\int(\mathrm{X}z-\mathrm{Z}x)\,dm$, in the above-named planes.

---

* The existence of an axis of instantaneous rotation is assumed, as a proof of it from elementary principles may be easily given. (See Earnshaw's Statics, art. 109.)

III. Let the axis $\mathrm{O}z$ be the instantaneous axis of rotation, ω the angular velocity round this axe, then the effective forces are, $\mathrm{X}=-\omega y$, $\mathrm{Y}=\omega x$, $\mathrm{Z}=0$; substituting these values in the above formulæ, we find the moments $\omega\int(x^2+y^2)\,dm$ $-\omega\int xz\,dm$, and $-\omega\int yz\,dm$ in the planes of xy, yz, and xz respectively.

Now as the impressed and effective moments must by the principle of D'Alembert be equivalent, we shall have, denoting by K the impressed moment, and by $\alpha\,\beta\,\gamma$ the angles which its axis makes with the axes of coordinates, the following equations :—

$$\left.\begin{array}{c}\mathrm{K}\cos\alpha=-\omega\int xz\,dm,\ \mathrm{K}\cos\beta=-\omega\int yz\,dm,\\ \mathrm{K}\cos\gamma=\omega\int(x^2+y^2)\,dm\ \ \ldots\ldots\end{array}\right\}\quad(1.)$$

From these equations it follows that when $\int xz\,dm=0$, $\int yz\,dm=0$, or when z is a principal axis of the body, that $\gamma=0$, or that the plane of the impressed moment must be perpendicular to a principal axe, in order that this axe may be an axis of rotation; and the angular velocity round this axe is $=\dfrac{\mathrm{K}}{\int(x^2+y^2)\,dm}$.

IV. The centrifugal forces generated by the motion act in the direction of the radius vector, and are proportional to the square of the velocity divided by the radius; hence $\mathrm{X}=\omega^2 r\,.\,\dfrac{x}{r}=\omega^2 x$, $\mathrm{Y}=\omega^2 y$, $\mathrm{Z}=0$; translating these forces to the origin, the moments thence resulting are $\omega^2\int xz\,dm$, $-\omega^2\int yz\,dm$ and 0, in the planes of xz, yz, and xy.

Let G be the centrifugal moment, α', β', γ' the angles which its axis makes with the axes of coordinates, then

$$\left.\begin{array}{c}\mathrm{G}\cos\alpha'=-\omega^2\int yz\,dm,\ \ \mathrm{G}\cos\beta'=\omega^2\int xz\,dm,\\ \mathrm{G}\cos\gamma'=0.\ \ \ldots\ldots\ldots\ldots\end{array}\right\}\quad(2.)$$

V. *The plane of the centrifugal moment* G *passes through the instantaneous axis of rotation, and through the axis of the impressed moment* K.

For as γ' is a right angle, the plane of G passes through the axis of z, the instantaneous axis of rotation.

Again, let V be the angle between the planes of G and K, then $\cos\mathrm{V}=\cos\alpha\cos\alpha'+\cos\beta\cos\beta'+\cos\gamma\cos\gamma'$. Putting for these angles their values given by (1.) and (2.), we find

$$K\,G\cos V = (\omega^3-\omega^3)\,.\!\int y\,z\,d\,m\,.\!\int x\,z\,d\,m = 0,$$

hence $V = 90$, or the plane of G passes through the axis of K.

VI. To express G in terms of K, ω, and the moment of inertia round the instantaneous axis of rotation.

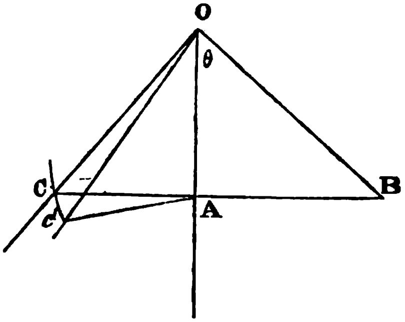
Squaring the six equations (1.) (2.), adding the first three together, and also the last three, we find

$$K^2 = \omega^2\left(\int x\,z\,d\,m\right)^2+\omega^2\left(\int y\,z\,d\,m\right)^2+\omega^2\left(\int (x^2+y^2)\,d\,m\right)^2$$
$$G^2 = \omega^4\left(\int x\,z\,d\,m\right)^2+\omega^4\left(\int y\,z\,d\,m\right)^2.$$

Multiplying the first of these equations by ω^2, subtracting the second from the first, and calling the moment of inertia round the instantaneous axis of rotation D, we get

$$G^2 = K^2\,\omega^2-\omega^4\,D^2 \quad \ldots\ldots\ldots \quad (3.)$$

VII. Let ω' denote the angular velocity round any line which makes the angle θ with the instantaneous axis of rotation, ω the angular velocity round this axis, then $\omega' = \omega\cos\theta$.



Let O A be the instantaneous axis, O B a line passing through the fixed point O, making the angle θ with the former O C, C A right lines perpendicular to O B, O A; let the point C move to c, a point indefinitely near the former, then $C\,c = O\,C\,.\,\omega'$, and $C\,c = C\,A\,.\,\omega$; hence $O\,C\,.\,\omega' = C\,A\,.\,\omega$, but $C\,A = O\,C\cos\theta$; hence $\omega' = \omega\cos\theta$.

VIII. Conceive an ellipsoid whose centre coincides with the origin, whose axes coincide with the principal axes of rotation of the body passing through the fixed point, and the squares of whose semiaxes are proportional to the moments of inertia round these axes, so that

$$A = n^3\,a^2,\quad B = n^3\,b^2,\quad C = n^3\,c^2 \quad \ldots \quad (4.)$$

n being a constant, a, b, c the semiaxes of the ellipsoid, and A B C the moments of inertia round those axes.

Let D be the moment of inertia round a right line passing through the centre, making the angles λ, μ, ν with the axes of coordinates, then

$$D = A\cos^2\lambda + B\cos^2\mu + C\cos^2\nu \quad \ldots\ldots \quad (5.)$$

Poisson, *Traité de Mécanique*, tom. ii. p. 56.

Or, putting for A, B, C their values given by (4.),

$$D = n^3(a^2\cos^2\lambda + b^2\cos^2\mu + c^2\cos^2\nu).$$

Now the part within the brackets is the square of a perpendicular from the centre of the ellipsoid on a tangent plane, making the angles λ, μ, ν with the axes, calling this perpendicular P, we have

$$D = n^3 P^2 \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (6.)$$

IX. Assume the impressed moment

$$K = n^3 f u, \quad \ldots\ldots\ldots\ldots\ldots \quad (7.)$$

u being the central semidiameter of the ellipsoid perpendicular to the plane of the impressed moment K. The product fu is of course constant, it will be shown presently that f and u are each constant.

X. *The axis of instantaneous rotation coincides with the perpendicular from the centre on the tangent plane to the ellipsoid, drawn through the vertex of the semidiameter u, and the angular velocity round this axis is inversely proportional to this perpendicular.*

Let p, q, r be the angular velocities round the three principal axes, α, β, γ the angles which the axis u of the impressed moment makes with the same axes, x, y, z the coordinates of the extremity of u, then by (III.) we have

$p = \dfrac{K\cos\alpha}{A}$, now $K = n^3 f u$, $\cos\alpha = \dfrac{x}{u}$, $A = n^3 a^2$, hence $p = \dfrac{fx}{a^2}$; in like manner

$$q = \frac{fy}{b^2}, \quad r = \frac{fz}{c^2} \quad \ldots\ldots\ldots\ldots\ldots \quad (8.)$$

$$\text{Hence } p^2 + q^2 + r^2 = \omega^2 = f^2\left\{\frac{x^2}{a^4} + \frac{y^2}{b^4} + \frac{z^2}{c^4}\right\} = \frac{f^2}{P^2} \quad (9.)$$

But the cosines which the axis of instantaneous rotation makes with the axes of coordinates, are $\dfrac{p}{\omega}, \dfrac{q}{\omega}, \dfrac{r}{\omega}$; and from the values of p, q, r just found, we get

$$\frac{p}{\omega} = \frac{Px}{a^2}, \quad \frac{q}{\omega} = \frac{Py}{b^2}, \quad \frac{r}{\omega} = \frac{Pz}{c^2}.$$

Now the cosines of the angles which the perpendicular to the tangent plane touching the surface at the point $(x\,y\,z)$ makes with the axes of coordinates, are also $\frac{Px}{a^2}, \frac{Py}{b^2}, \frac{Pz}{c^2}$; hence the axis of instantaneous rotation coincides with this perpendicular.

XI. *During the whole motion of rotation the semidiameter of the ellipsoid perpendicular to the plane of the impressed moment is invariable, or u is constant*; to show this we shall make use of the following property of the ellipsoid:—

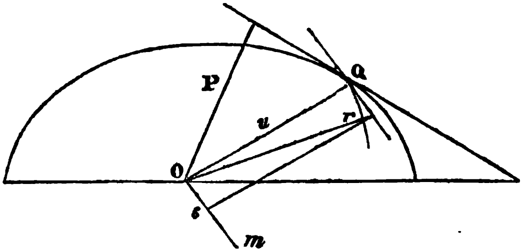
Let a tangent plane be drawn to an ellipsoid, u the semidiameter through the point of contact, P the perpendicular on this tangent plane from the centre, m the semidiameter of the ellipsoid perpendicular to the plane of u and P; m and u are the semiaxes of the section of the surface made by the plane containing u and m.

As P is perpendicular to the tangent plane, every plane which passes through this line is perpendicular to this plane, hence the plane containing u and P is perpendicular to the tangent plane.

In like manner, as the semidiameter m of the ellipsoid is perpendicular to the plane of u and P, the plane which passes through m and u, is perpendicular to the plane containing u and P; hence the tangent plane, and the plane of m and u, are each perpendicular to the plane of u and P, therefore their intersection is perpendicular to the same plane, and therefore parallel to m, and therefore perpendicular to u; but when a tangent to a conic section is perpendicular to the diameter passing through the point of contact, this diameter is an axe of the section; therefore u and m are the semiaxes of the section of the ellipsoid containing u and m.

Now u being the axis of the impressed moment, and P the perpendicular on the tangent plane, through the vertex of u, P is the axis of instantaneous rotation, therefore the plane of u and P is the plane of the centrifugal moment, and m perpendicular to this plane (V) is its axis.

Assume a point s, on this line m, so that $o\,s$ may be to u as G to K; and complete the rectangle O s Q v, the diagonal $o\,v$ of this rectangle will represent both in magnitude and direction, the axis of the resultant moment at the end of the first instant; during this instant the vertex of the axe of the resultant moment has travelled on the surface of the ellipsoid, and also on the surface of a concentric sphere, whose radius is u, since the line Q v perpendicular to O Q or u is a tangent both to this sphere and the ellipsoid; hence at the end of the first instant the vertex of the axe of the resultant moment will be found on the curve of double curvature the intersection of the sphere and ellipsoid; therefore u is constant.



XII. Now as u is constant, and $f u$ also constant (7.), f too is constant; but the angular velocity

$$\omega = \frac{f}{P}; \text{ (see (9.))}$$

hence the angular velocity round the instantaneous axis of rotation varies inversely as P.

XIII. *The angular velocity* ϖ *round the axis of the impressed moment is constant during the motion.*

Let θ be the angle between u and P, $\cos\theta = \frac{P}{u}$, and $\varpi = \omega\cos\theta$ hence

$$\varpi = \frac{f}{P}\cdot\frac{P}{u} = \frac{f}{u}, \text{ a constant quantity } \ldots \quad (10.)$$

XIV. *The magnitude of the centrifugal moment* G *varies as the tangent of the angle between u and* P.

Resuming the equation (3.), $G^2 = K\,\omega^2 - \omega^4 D^2$, and putting for K, ω, and D their values given by (7.), (9.) and (6.) and introducing the relation $P^2\tan^2\theta = u^2 - P^2$, we find

$$G = K\frac{f}{u}\tan\theta;$$

or putting for $\frac{f}{u}$ its value ϖ, the angular velocity round the axe of the impressed moment,

$$G = K\varpi\tan\theta \quad \ldots\ldots\ldots \quad (11.)$$

XV. The curve which is the intersection of the ellipsoid and sphere is on a cone of the second degree, whose circular sections are parallel to the central sections of the ellipsoid; let the equations of the ellipsoid and sphere be respectively

$$\frac{x'^2}{a^2} + \frac{y'^2}{b^2} + \frac{z'^2}{c^2} = 1, \text{ and } x'^2 + y'^2 + z'^2 = u^2.$$

Let the equations of the right line, passing through the centre and the point $x' y' z'$, be $x = \frac{x'}{z'} z$, $y = \frac{y'}{z'} z$: by the help of these four equations, eliminating $x' y' z'$, we get

$$\frac{x^2}{a^2}(a^2-u^2)+\frac{y^2}{b^2}(b^2-u^2)+\frac{z^2}{c^2}(c^2-u^2) = 0 \;.\;. \quad (12.)$$

Hence during the rotation of the body, the axis u of the impressed moment, is found on a cone of the second degree, whose circular sections are parallel to the circular sections of the ellipsoid, as we now proceed to show.

In an ellipsoid, let ϕ be the angle between the plane of a circular section, and the principal plane containing the greatest and mean axe of the ellipsoid, η and ϵ the eccentricities of the two principal sections perpendicular to this plane, whose semiaxes are b, c and a, c respectively,

$$\text{then } \cos\phi = \frac{\eta}{\epsilon}, \text{ or } \cos^2\phi = \frac{a^2}{b^2}\frac{(b^2-c^2)}{(a^2-c^2)} \;.\;.\;.\;.\;. \quad (13.)$$

If now in this formula, instead of a^2, b^2, c^2, the squares of the semiaxes of the ellipsoid, we substitute $\frac{a^2}{a^2-u^2}$, $\frac{b^2}{b^2-u^2}$, $\frac{c^2}{c^2-u^2}$, which are proportional to the squares of the corresponding semiaxes of the cone, we shall have, calling ϕ' the angle between a circular section of the cone and the plane of $a\ b$,

$$\cos\phi' = \frac{a^2}{b^2}\frac{(b^2-c^2)}{(a^2-c^2)},$$

a result independent of u; hence $\phi' = \phi$.

XVI. *The instantaneous axis of rotation moves on a cone of the second degree.*

Assume a point on the perpendicular P, (whose coordinates let be $x\,y\,z$) at the distance l from the centre.

Then $\cos\lambda = \frac{\text{P}\,x'}{a^2}$, or

$$a^4\cos^2\lambda = x'^2\left\{a^2\cos^2\lambda + b^2\cos^2\mu + c^2\cos^2\nu\right\}.$$

Now, $l\cos\lambda = x$, $l\cos\mu = y$, and $l\cos\nu = z$, substituting these values, we find $x'^2 = \frac{a^4 x^2}{a^2 x^2 + b^2 y^2 + c^2 z^2}$.

Finding analogous values for y' and z', introducing the relation

$x'^2 + y'^2 + z'^2 = u^2$, we obtain for the equation of this cone,

$$a^2(a^2-u^2)x^2 + b^2(b^2-u^2)y^2 + c^2(c^2-u^2)z^2 = 0. \quad (14.)^*$$

---

[*] It may be worth while, and it is not difficult to show, that the equations of these cones, the loci of the axis of the impressed moment and of

XVII. Now as the axe of the impressed moment is always on the surface of this cone whose circular sections coincide with those of the ellipsoid, the plane of the impressed moment will envelope during the rotation a cone *supplemental** to the former; whose *focals* will be therefore perpendicular to the circular sections of the first cone, that is, to the circular sections of the ellipsoid; hence the *optic axes*† of the ellipsoid of moments will be the focals of the cone enveloped by the plane of the impressed moment; thus the whole motion of the body consists in the uniform rotation of the plane of the impressed moment round its axis, while this plane rolls on the surface of the latter cone.

XVIII. From these considerations it follows, that we may dispense altogether with the Ellipsoid of Moments, and say, *that if two right lines are drawn from the fixed point of the body, in the plane of the axes of the greatest and least moments of inertia, making an angle with the axe of greatest moment whose cosine squared may be equal to* $\frac{\mathrm{A}\,(\mathrm{B}-\mathrm{C})}{\mathrm{B}\,(\mathrm{A}-\mathrm{C})}$, *and a cone be conceived having these lines as focals and touching the plane of the impressed moment, the whole motion of the body will consist in the uniform rotation of this plane section of the body in its own plane, while this plane envelopes the cone.*

Let A C B be the mean section of the ellipsoid, O N′, O N the optic axes, then if the plane of the impressed moment coincides with any of the principal planes, the cones round the optic axes as focals become also planes, and the axes of rotation coincide with the axes of the figure.

---

the axis of instantaneous rotation, are equivalent to the equations of the same cones given by Poisson, *Traité de Mécanique*, tom. ii. pp. 151, 152. For this purpose then, assuming the equation given by Poisson, tom. ii. page 140, $\mathrm{A}\,p^2 + \mathrm{B}\,q^2 + \mathrm{C}\,r^2 = h$; and putting for A, B, C, p, q, r, their values given in the preceding pages, we find $h = n^3 f^2$.

Eliminating then from (12.) and (14.) the quantities a, b, c, u, and introducing A, B, C, k, h instead, equation (12.) is changed to

$$\frac{(k^2-\mathrm{A}h)}{\mathrm{A}}x^2 + \frac{(k^2-\mathrm{B}h)}{\mathrm{B}}y^2 + \frac{(k^2-\mathrm{C}h)}{\mathrm{C}}z^2 = 0, \text{ and (14.) is changed to}$$

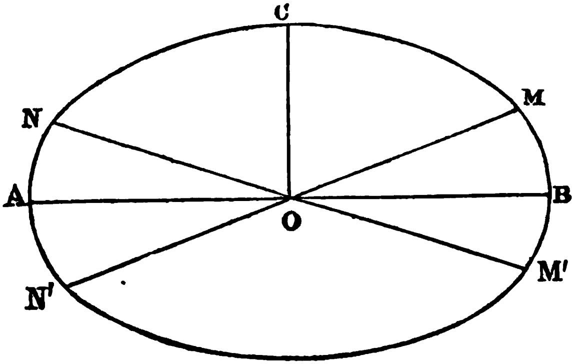
$$(k^2-\mathrm{A}h)\,x^2 + (k^2-\mathrm{B}h)\,y^2 + (k^2-\mathrm{C}h)\,z^2 = 0.$$

* Two cones are said to be supplemental when their corresponding vertical angles are supplemental.

The *focals* of a cone are two right lines drawn through the vertex of the cone, making with the axis through the vertex equal angles, such that the cosine of one of these angles is equal to the ratio of the cosines of the semiangles of the cone.

† I have ventured to use the appellation *optic axes*, a term borrowed from the wave theory of light, to denote the diameters perpendicular to the circular sections of an ellipsoid.

Again, if the plane of the impressed moment intersects the mean plane between N and C, it will envelope the cone whose



focals are O N, O N′; but if the plane of the impressed moment intersect between A and N, it will envelope the cone whose focals are O N, O M; hence the range in the former case, which may be taken as the measure of the stability of rotation round the principal axe whose moment of inertia is greatest, is to the range in the latter, which may be deemed the representative of the stability round the axis whose moment of inertia is the least, as the supplement of the angle between the optic axes, or between the circular sections of the ellipsoid is to the angle itself.

XIX. In the particular case, where the plane of the impressed moment passes through the optic axe of the ellipsoid, the cones become portions of the plane of the greatest and least axes of the surface, divided one from the other by the optic axes; hence if the plane of the impressed moment be disturbed from coincidence with the mean plane of the ellipsoid, so as to commence its rotation round one of the optic axes, it will either return to its former position of coincidence, or revolve through 180° round the optic axe, till it coincides at length with the mean plane on the opposite side; hence the axe of the impressed moment must revolve in a circular section of the ellipsoid, through an angle less than 180°, with a variable velocity, till it at length coincides with the mean axe of the ellipsoid, the body in the mean time uniformly revolving round this axe so moving in the circular section.

[To be continued.]

LXV. *On the Conversion of the Bisulphuret of Copper (Yellow Copper Ore) into the Sulphuret (Vitreous Copper) by Electricity.* By Mr. Robert Hunt, *Secretary to the Royal Cornwall Polytechnic Society**.

HAVING been recently engaged in an experimental inquiry into the electricity of mineral veins, and being desirous of examining all the phænomena which have any bearing on this most interesting subject, I have been induced to institute some experiments on the influence of electric currents upon copper pyrites, which is a bisulphuret of copper and iron.

At the Bristol meeting of the British Association, Robert Were Fox, Esq. exhibited the experiment of the conversion of the bisulphuret of copper into the sulphuret, and in the Fourth Annual Report of the Polytechnic Society (1836), several experiments are detailed in proof of the fact. In repeating those experiments, having adopted other methods than those pursued by Mr. Fox, I have arrived at irrefragable proofs of the process of decomposition, which alone induces me to add my testimony to the high authority of that gentleman.

The original experiment consisted in placing the yellow bisulphuret of copper in a solution of sulphate of copper, divided by a wall of clay from a cell containing plain or acidulated water, in which was placed a piece of metallic zinc connected by a wire with the ore. By this arrangement the ore was in a short time changed, over its surface, into the sulphuret or vitreous copper, having parted with a portion of its sulphur and its iron.

1. I was extremely desirous of ascertaining if this change would take place in a solution which did not contain copper. I therefore divided a vessel by a thin wooden partition. In one cell I placed a solution of the sulphate of soda, and in the other water acidulated with sulphuric acid. Into the sulphate of soda I placed a piece of the bisulphuret of copper weighing two hundred and fifty-six grains, connected with a piece of zinc which dipped into the other cell. After three days, the ore having become first iridescent and then gray over its surface, I removed and weighed it. Its weight was now two hundred and forty-seven grains, *having lost nine grains*. By adding ferrocyanate of potash to the solution a considerable portion of the cyanate of iron was formed; a convincing proof of the separation of iron from the ore.

2. A piece of the copper pyrites, weighing two hundred

---

* Communicated by the Author.

grains, was placed in a solution of the *muriate of barytes*, water only being in the cell containing the zinc with which the ore was connected. In a few days the ore was covered with a reddish-brown powder, which was proved to be peroxide of iron, beneath which was a coating of vitreous copper. A quantity of the *sulphate of barytes* was formed and collected, and traces of copper were evident in the solution. During the process there was a steady liberation of gas from the fluid in contact with the copper ore, which was collected and found to be pure hydrogen. This experiment proves that the sulphur liberated from the yellow ore is converted into sulphuric acid at the expense of the oxygen of the water, which is seized by the barytes and forms the sulphate of that earth, and that the iron is freed in the state of peroxide. It is possible the copper detected in this instance may have arisen from the wire used to make the connexion. The water in the cell which contained the zinc was now found, as was of course expected, to hold muriate of zinc in solution. From the adhesion of the peroxide of iron to the piece of ore, it was difficult to ascertain the actual loss of weight it had suffered.

3. Two pieces of copper pyrites, each weighing seventy-two grains, were connected by *zinc wires* with a single pair of zinc and copper plates excited by acidulated water, and both plunged into a solution of the muriate of soda. The zinc wires were so quickly destroyed from the action of the electro-negative ore on the positive metal, that it was found impossible to carry on the experiment for any time. The change was, however, well marked even in this case.

4. Seventy-four grains of copper pyrites was connected by *silver wire*, with the positive pole of a circular battery of a single pair excited with salt and water, and fifty-two grains of the same ore with the negative pole in like manner. These two pieces were placed in a precipitating glass filled with a solution of the muriate of barytes. It will be evident, that in this experiment the fragments of ore form the poles of a galvanic arrangement. In forty-eight hours they were removed; it was now found that one piece weighed seventy-five grains, and the other only fifty, the latter being partially changed into vitreous ore, while the former was covered with specks of the *peroxide of iron*. It was extremely interesting to find the peroxide acting as an acid would have done, and passing to the positive pole. Rather more than a grain of *sulphate of barytes* was collected from the glass; and the solution on being treated with the ferrocyanate of potash gave a precipitate of the *cyanate of copper*: copper in minute quantity was also detected by means of a piece of polished iron.

These experiments appear to me sufficient to prove the decomposition of the yellow ore. The loss of weight which is detected is accurately made up by the peroxide of iron, the sulphur of the sulphate of barytes and the copper. It may appear to some that the alterations in weight in the last experiment (4.) are trifling, but it must be remembered that the copper pyrites was exposed, for a short time only, to a weak current from a rather inconstant battery. If the action had been continued longer, with a sustaining battery the loss of weight would have been proportionally greater.

5. With a view of ascertaining the effect of a more powerful current on similar pieces of ore, I placed one piece in a solution of the sulphate of copper, and another in water divided from it by a membrane, connecting them respectively with the positive and negative poles of a battery of twelve pairs arranged as the *couronne de tasses*, excited with strong brine. I thus procured a tolerably powerful, but constantly weakening electric current. The arrangement was left for two days, when the piece of ore in the solution of sulphate of copper, which was connected with the negative pole, was found to have crumbled down considerably, eight grains of vitreous copper in a state of fine division having been collected from the bottom of the vessel, the loss of weight on the whole being ten grains and a half. Nothing is more interesting than to observe, on the grand scale of natural operations, examples of processes which we have imitated in the laboratory, and thus to find, at the same time, a proof of the correctness of our experiments, and a complete elucidation of a natural phænomenon.

At the last annual meeting of the Polytechnic Society, a communication was read from Joseph Carne, Esq., stating, that at the mines in the neighbourhood of Marazion, the bisulphuret of copper was always found in the blue slate, whereas the vitreous ore was constantly found in the red slate; and that the same fact was observed at the Carn Brea mines near Redruth, where the yellow ore was invariably changed into vitreous ore whenever the granite exhibited a red colour. This tincture is due to the peroxide of iron; and it is worthy of remark, that the stain does not extend many feet on either side of the load.

I must be excused from remarking, that it appears to me evident that the change of the bisulphuret of copper into the sulphuret is explained in the preceding experiments, and shown to depend upon the decomposing power of the electric currents which circulate through the metalliferous veins, as was discovered by Mr. Robert Fox, and the decomposing agency of which I have proved much to my satisfaction in some experiments conducted in East Pool copper mine.

November 4, 1841.

LXVI. *On the History of* Fernel's *Measure of a Degree.*

By Professor De Morgan.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I BEG to call the attention of mathematicians to a very strange circumstance in the account of Fernel's measure of a degree, which will show how the history of science is sometimes written.

Montucla (vol. ii. p. 231, first edition; vol. ii. p. 316, second edition) gives the usual account of Fernel's measuring by the revolutions of a wheel from Paris towards Amiens, and finding a degree to be 56746 Paris toises. In the second edition he adds, that the details are found in Fernel's *Cosmotheoria*.

Lalande (*Bibliographie Astron.*, p. 46) gives the *Cosmotheoria* as having 46 *feuillets*, and as published in 1528: he adds, that it is remarkable as containing the first determination of the true magnitude of the earth, and refers to his own account of it in the Memoirs of the Academy for 1787, page 216.

Delambre (Astronomy, vol. iii. p. 516) repeats the story, but makes the result 57070 toises: he does not refer to the *Cosmotheoria*.

On looking over the work of Fernel for another purpose, I was surprised to see that he himself states a very different result, and piques himself on its close agreement with that of Almæon (Al Mamun he means). The story about the wheel is correctly told; it should have been added, that he observed his solar altitudes with Ptolemy's rules. His result is, that the degree of latitude consists of 68 *Italian* miles and 96 paces, which he makes 68 miles 95¼ paces, to avoid fractions in the resulting diameter of the earth. I quote three different places where he has expressed this result, premising that my copy has 46 leaves, and was published in 1528; so that all doubt of its being the work described by Lalande may be impossible.

Leaf 2, page 1. "..., idipsum experimento comprobans, deprehendi accurata supputatione, cuique gradui circuli majoris tam in terræ quàm in maris convexo 68 Italica miliaria, passus 95 cum una quarta respondere."

Leaf 2, page 2. "Cuivis gradui ambitus terræ 68 milliaria, 95¼ passus."

Leaf 3, page 2. "Reperi passus 68096 qui milliaria sunt Italica 68, cum passibus 96. Malui tamen hos passus, in passus 95 cum uno quarto convertere, ne quæpiam fractio foret in terræ diametro præfigenda."

Now let us ask what are 68·096 Italian miles? The Italian mile was always used, under the idea that it was the old Roman mile; from which in fact it differs little for our present purpose, the latter being about 1614 yards, and the former 1628. But not to take any point against Fernel's measure, let us follow Dr. Bernard, who makes the statute mile to be 1056 paces, which would give 1667 yards to the mile of 1000 paces. This would give 64½ English statute miles to Fernel's degree, about *five miles* too little. And if, as is more probable, we take 1628 yards for Fernel's Italian mile, we find only 63 statute miles for his degree. The French toise is 2·1315 yards; so that Delambre makes Fernel's degree to be 69·12 miles. How he made the Italian mile longer* than the English statute mile, I have no idea. The confusion is older than Delambre and Lalande, as appears by Montucla's first edition. Riccioli gives a true account and so does Dr. Bernard. Weidler (p. 341) omits all mention of the degree of the *Cosmotheoria*, but says (without citing authority) that Fernel undertook to measure a degree, which he finished *in the year* 1550. The copy from which I quote belonged to Montucla, who has corrected errors in several places, but has taken no notice of this, though I see from one of his marks that he had read the page. Did Fernel measure another degree in 1550? If so, what is the authority for it? Why did Montucla, who is proved to have seen this very page of Weidler, refer to the *Cosmotheoria*? And why is this second degree mentioned only by Weidler? Perhaps some of your readers may be able to clear up this point.

Richard Norwood (Seaman's Practice, p. 41) being naturally desirous to get confirmation of his own degree (which I cannot help thinking has been much underrated) first contrives to twist the Arabian degree into nearly the same as his own; and then allows Fernel's to come as near to it, but only by representing (without authority) "the pace which he used as being more than five of our English feet." Yet Norwood himself, in the very first sentence of his own work, says, "It is a commonly received opinion, that allowing five of our English feet to a geometrical pace, a thousand of these paces make an Italian mile;" and Fernel certainly used the Italian mile of his day. On this the whole question seems to turn, but I think I may confidently assert that no evidence can be brought to show that the curious fiction which geometers of the sixteenth century called a geometrical pace was even as long as five English feet; and certainly it was not longer.

---

* There is a Tuscan and Lombard mile in modern road-books, which is a trifle longer than the English mile: but Delambre never could have mistaken this for the famous Italian mile, the universal standard of the middle ages.

I remain, Gentlemen, yours faithfully,

University College, Oct. 26, 1841. A. De Morgan.

LXVII. *Notices of the Results of the Labours of Continental Chemists. By Messrs.* W. Francis *and* H. Croft.

[Continued from p. 285.]

On Succino-hyposulphuric Acid.

By the action of anhydrous sulphuric acid on benzoic acid, Mitscherlich prepared his benzoesulphuric acid (benzoehyposulphuric acid, Fehling). Mitscherlich's formula is $S^2 O^6, C^{14} H^{10} O^3 + H^2 O$. Fehling gives the formula, according to Liebig's theory of the constitution of organic acids, as $S^2 O^5, C^{14} H^8 O^3 + 2 H^2 O$. Fehling has succeeded in preparing a similar compound with succinic acid. He treated succinic acid with the vapour of anhydrous sulphuric acid; it is absorbed in large quantities, and heat is evolved; a brown elastic mass is formed, which must be allowed to stand for twenty-four hours. The mass is then diluted with water, and carbonate of baryta or lead added until the filtered solution gives no precipitate with chloride of barium. The filtered solution is then precipitated by acetate of baryta or lead, and from the edulcorated salt of lead the free acid may be prepared by means of sulphuretted hydrogen. The solution evaporated *in vacuo* gives small verrucose crystals, which cannot be perfectly dried. The acid is soluble in water and alcohol. These crystals, when heated with carbonate and nitrate of potassa, gave 99·21% sulphate of baryta; according to the formula $C^8 H^4 O^5 + S^2 O^5 + 8 H^2 O$, the quantity would be 99·2%. The dry acid is probably $C^8 H^4 O^5 + S^2 O^5 + 4 H^2 O$. From the composition of the salt of lead, it is probable that the acid is quadribasic; $2 (C^4 H^4 O^3 + H^2 O) + 2 S O^3 = C^8 H^4 O^5 + S^2 O^5 + 4 H^2 O$. By completely saturating the acid with potash, a very deliquescent salt is obtained; by the addition of a little acid to its solution a salt is formed which may be obtained in good crystals, and is not deliquescent; it contains six atoms of water; *in vacuo* it loses two atoms. This salt with four atoms loses at 100° three atoms more, and its formula is then $C^8H^4O^5, S^2O^5 + 3\,KO + H^2O$, or $C^8H^4O^5.\underline{\overset{\cdot\cdot\cdot\cdot\cdot}{S}} + \left.\begin{matrix}3\,\dot{K}\\ \underline{\dot{H}}\end{matrix}\right\}$.

The salt dried *in vacuo*, and then at a gentle heat, contains three atoms of water, $C^8H^4O^5, S^2O^5 + 3\,KO + 3\,H^2O$. By adding more acid to this succino-hyposulphate another salt is obtained, $C^8H^4O^5, S^2O^5 + 2\,KO + 6\,H^2O$. It loses one atom of water *in vacuo*, at a gentle heat another atom, and at 100° two more. The ammonia salt dried at a low temperature is $C^8H^4O^5, S^2O^5 + 3\,N^2H^8O + 3\,H^2O$. The baryta salt is insoluble; its formula, after being dried at 100°, is $C^8H^4O^5, S^2O^5 + 3\,BaO + H^2O$. The lime salt is uncrystallizable; it contains two atoms of lime. If the acid solution, which has been filtered from the sulphate of lead in the preparation of the acid, be precipitated by acetate of lead, a salt falls containing $3\,PbO$; if the solution be first neutralized with ammonia, the precipitate contains $4\,PbO$. The first salt when dried in the air is $C^8H^4O^5, S^2O^5 + 3\,PbO + 4\,H^2O$. It loses three atoms of water at 100° C. The salt with $4\,PbO$ contains four atoms of water, which are driven off at 100° C. Formula $C^8H^4O^5, S^2O^5 + 4\,PbO$. The silver salt is very easily decomposed.

In the formation of benzoe-hyposulphuric acid, Liebig supposes that one atom of oxygen derived from the two atoms of sulphuric acid, combines with two atoms of hydrogen derived from the benzoic acid; the water thus formed adds itself to the basic water of the benzoic acid, and the resulting compound is therefore *bibasic*. Berzelius supposes that the water is derived from the benzoic acid alone, which is thus converted into a substance without acid properties, which can combine with sulphuric acid without influencing its saturating power. Mitscherlich also believes that the saturating power depends wholly on the inorganic acid. But the succino-hyposulphuric acid saturates four atoms of base; if the atom of succinic acid be $C^4H^4O^3 + H^2O$, then both acids have retained their saturating power in the new compound, and, from analogy, benzoesulphuric acid ought to be tribasic. Fehling considers it most probable that succinic acid is a tribasic acid; the hypothetical anhydrous acid is the $C^8H^6O^5$; this combines with two atoms of sulphuric acid, one atom of water is given off, and hyposulphuric acid, which saturates only one atom of base, is formed. The compound acid will then be quadribasic. Crystallized succinic acid is probably $C^8H^6O^5 + 3\,H^2O$. (*Annalen der Chemie und Pharmacie*, xxxviii. p. 285.)

On the Compounds of Cyanogen with Sulphuretted Hydrogen.

Völckel has made some experiments; under Wöhler, on the two compounds obtained by mixing together cyanogen and sulphuretted hydrogen. The yellow compound has been described by Gay-Lussac, the red one by Wöhler. By passing the two gases at the same time into alcohol, but so that the cyanogen is always in excess, the yellow body is obtained; when sulphuretted hydrogen is in excess the red one is formed. They may be purified by recrystallization out of alcohol. The red body has the formula $C^2\,N^2\,H^4\,S^2$. By adding acetate of lead to an excess of this body dissolved in alcohol, a yellow precipitate is obtained; its formula, when dried *in vacuo*, is $C^2\,N^2\,H^2\,S^2\,Pb$. In the red body, therefore, one equivalent of hydrogen has been replaced by one equivalent of lead. It may be considered as similar to mercaptan, viz. $C^2\,N^2\,H^2\,S + H^2\,S$. The lead compound is $C^2\,H^2\,N^2\,S + Pb\,S$. By boiling with water this salt is decomposed, sulphuret of lead is formed, cyanogen is evolved, and the solution contains both the red and the yellow body. The red body is soluble in a *cold* solution of potassa, it is precipitated again by acids. Boiled with a *concentrated* solution of potassa, it forms sulphocyanuret, sulphuret, and cyanuret of potassium. Boiled with a dilute solution it forms ammonia, oxalate of potassa, and sulphuret of potassium. It may be considered as oxamid, in which the oxygen is replaced by sulphur. The yellow compound decomposes very easily, does not give any lead compound; with a salt of silver it is decomposed like the red compound; cyanogen is evolved, and sulphuret of silver precipitated. According to Gay-Lussac, its composition is $C^4\,N^4\,H^6\,S^3$, or perhaps $2\,C^2\,N^2\,H^2\,S + H^2\,S$. If we double its formula we have the composition of allantoin, in which the oxygen is replaced by sulphur. (*Annalen der Chemie und Pharmacie*, xxxviii. p. 314.)

On some Benzoyl Compounds.

Laurent has examined the action of sulphuretted hydrogen and of hydrosulphuret of ammonium on the benzoyl compounds.

Hydruret of sulphobenzoyl is obtained by dissolving one volume of oil of bitter almonds in eight or ten volumes of alcohol, and adding slowly one volume of the hydrosulphuret; after a few minutes a white mealy powder is deposited. By adding a little hydrosulphuret to the boiling hot alcoholic solution of the oil, a white voluminous precipitate of the hydruret is produced. It is insoluble in water and alcohol, is dissolved by æther, but only in small quantity; becomes soft at 90° or 95° C. By distilling it is decomposed into several new bodies; it is decomposed by nitric acid. Formula $C^{14} H^{12} S^{2}$. The oxygen of the benzoyl has therefore been replaced by sulphur. [Might not the formula be $C^{14} H^{10} S + H^{2} S$, *i. e.* hydrosulphuret of benzoyl? Berzelius assumes a radical in benzoic acid, $C^{14} H^{10}$, which he calls benzoyl.—H. C.]

Hydruret of sulphazobenzoyl is formed with the above, but only in small quantity. It may be obtained by mixing one volume of hydrosulphuret of ammonium with a solution of one volume of oil of bitter almonds in four or five volumes of æther, and allowing the mixture to stand for a month; a white crystalline crust is formed, which may be purified by resolution in æther, &c. This hydruret is colourless and transparent, may be obtained in good crystals, oblique prisms with rectangular base. It melts at 125°; at a higher temperature ammonia is evolved and other products are formed. Boiling alcohol slowly decomposes it. It is rapidly decomposed by warm nitric acid; an oil (bitter almond oil?) is formed. Formula $C^{126} H^{108} N^{4} S^{12}$, or six atoms of hydruret of sulphobenzoyl, and two atoms of hydrobenzamid (hydruret of azobenzoyl), $6 (C^{14} H^{10} S^{2} + H^{2}) + 2 (C^{21} H^{15} N^{2} + H^{3})$.

Hydrosulphate of Azobenzoyl.—Equal volumes of almond oil, ammonia, and hydrosulphuret of ammonium, are allowed to stand six months in a closed bottle, the oil becomes almost solid; on boiling the mass with æther, a small quantity of the hydrosulphate remains in form of a white powder. It is almost insoluble in alcohol, somewhat soluble in æther, and from this solution it may be obtained in microscopic crystals. The probable formula is $C^{14} H^{10} N^{2} + H^{2} S$.

Azobenzoidin.—Oil of bitter almonds, which had not been purified by means of protochloride of iron and potassa, was distilled, and the first two-thirds preserved. A part of this, mixed in a bottle with an equal volume of ammonia, became a solid mass in fourteen days; this was treated with cold æther, which dissolved an oily matter, a substance crystallizing in needles and azobenzoyl: a white powder remained, which, when boiled with æther, left some benzoylazotid behind mixed with another substance, and the æthereal solution gave on evaporation crystals of azobenzoidin. It is colourless and inodorous, almost insoluble in alcohol, somewhat soluble in æther, is decomposed by boiling hydrochloric acid, forms with boiling nitric acid an acid probably new. Formula, $C^{14} H^{11} N^{\frac{1}{2}}$. If this formula be doubled, and fifty-nine taken as the equivalent of nitrogen, we obtain the formula $(C^{14} H^{10} N^{2} + N) + (C^{14} H^{10} N^{2} + H^{2}.)$ This body is isomeric with azobenzoid, which is insoluble in æther, uncrystalline, but crystallizes after fusion, which the above body does not.

Azobenzoïlid.—Purified bitter almond oil was distilled, and the first eighth of the product mixed with ammonia; in three weeks half of the oil had become solid; æther extracted the oily portion, the remainder was crystalline. It is insoluble in alcohol, very little soluble in æther, is decomposed by distillation, forms with nitric acid an oily substance. Formula $C^{14} H^{11} N^{\frac{4}{3}}$. It is therefore isomeric with azobenzoid and azobenzoidine.

Hydruret of benzoyline is formed by treating hydruret of azobenzoylin with hydrochloric acid; a thick oily matter is produced, which must be washed with boiling water and dried until it is solid. It is colourless, inodorous, easily soluble in alcohol and æther, is not decomposed by distillation; forms a compound with bromine. By the addition of ammonia to its boiling alcoholic solution, hydruret of azobenzoylin is formed. Formula $C^{14} H^{12} O^{2}$. It is therefore isomeric with hydruret of benzoyl.

Hydruret of Azobenzoylin.—It has been mentioned above, that in the process for procuring azobenzoidin by the action of ammonia on almond oil, an oily matter, azobenzoyl, and a substance in acicular crystals are produced. This mixture is heated with alcohol until it boils, and hydrochloric acid added; the whole is dissolved, except a small quantity of a crystalline substance. The alcoholic solution deposits an oily matter on evaporation; this, when treated with ammonia, becomes solid in a few minutes. The solid part must be quickly washed with a mixture of alcohol and æther, by which a white substance is obtained, which may be purified by repeated recrystallization out of alcohol. It is colourless, inodorous, tolerably soluble in boiling alcohol; the solution deposits it on cooling in the form of long needles. It may be distilled without decomposition, but is easily decomposed by hydrochloric acid. Formula $C^{14} H^{12} N^{\frac{4}{3}}$. It is therefore isomeric with hydrobenzamid, benzhydramid, and benzoinamid. The hydrobenzamid is easily soluble in æther, and crystallizes in octahedrons. Benzhydramid is not decomposed by hydrochloric acid, and benzoinamid is distinguished by its almost perfect insolubility.

Laurent has discovered benzoyl (Liebig's benzil) in the purified oil of bitter almonds. By treating almond oil with chloride of sulphur, benzoate of hydrobenzoyl is produced. (*Annales de Chemie et de Phys.*, 3rd ser., t. i. p. 291.)

[It may be remarked, that the mention of a great number of other bodies, whose composition and properties have not been examined, has been excluded from this extract.]

Formation of Murexid.

Liebig and Wöhler have found that when a *concentrated* solution of *pure* alloxan is boiled, an evolution of carbonic acid takes place, which continues for a long time. The solution then gives, with baryta, a blue precipitate, and with carbonate of ammonia a considerable quantity of murexid. On cooling, and during the boiling, a quantity of alloxantin is deposited, which explains the formation of murexid. If the mother liquor is saturated with ammonia, oxalurate of ammonia is obtained. Three atoms of alloxan give

$$\begin{array}{lll}
2 \text{ atoms Alloxantin} & = & C^{16}\,H^{20}\,N^{8}\,O^{20}. \\
1 \text{ atom Parabanic acid} & = & C^{6}\,H^{4}\,N^{4}\,O^{6}. \\
2 \text{ atoms Carbonic acid} & = & C^{2}\qquad\quad O^{4}. \\
\hline
3 \text{ atoms Alloxan} & = & C^{24}\,H^{24}\,N^{12}\,O^{30}.
\end{array}$$

(*Annalen der Chemie und Pharmacie*, xxxviii. p. 357.)

LXVIII. *A few Remarks on Electro-Metallurgy. By Mr.* T. B. JORDAN[*].

IN the course of a limited series of experiments, which I have tried in this interesting branch of the useful application of electricity, a few ideas have occurred to me, which, I believe, possess the charm of novelty. As no suggestion, however trivial in itself, can be considered altogether unimportant when it bears on the practical application of science to our manufacturing arts, I feel no hesitation in laying before the Society the following remarks with a view to their being made public. It is quite evident that Mr. Spencer's recent discovery, of the art of depositing metals from a solution of their salts by galvanic action, has placed in the hands of manufacturers a new means of copying, and the importance of this will be immediately apparent to those who know how large a portion of our manufactures in metal are the copies of original matrixes, or more indirectly the results of other copying processes. Some imagine that because it is only the means of copying, it is really of little value; but it is impossible to allow this to be a valid objection, when we consider how very few metal articles there are, which do not wholly or in part owe their origin to copying. If then it can be shown that this method possesses advantages for some description of work, which are not common to any other, and that there is good reason to suppose it may be œconomically conducted, then we may fairly infer, that it will in due time become one of the ordinary processes of our factories.

---

[*] From the Eighth Annual Report of the Royal Cornwall Polytechnic Society, 1840.

Its advantages are numerous, and some of them so self-evident, that they only require naming to be allowed; for instance, any surface, whatever be its material or form, may be covered with a coating of metal, without heat and without force; this coating may be allowed to accumulate to the required thickness, and may then be removed from its original, when the surfaces which were in contact will be found to be so perfectly alike, that it is impossible to discover the minutest scratch in one which has not its counterpart in the other. In proof of the accuracy of the copy, I may refer to the numerous admirable copies of engraved plates and medals already before the public; if any of these specimens be examined with a glass, it will be found that not only every line of the graver, but every scratch in the polished surface of the original plate is faithfully transferred, both to the deposited matrix, and the subsequent copy of it; and when we consider the fact that both these copies are of the same material as the original plate, and that that may just as well have been of a much softer and more workable material, we must immediately conclude that there is no other known mode of copying possessing like advantages. But as the application of it to the copying of engravings, medals, woodcuts, and similar subjects, is already well known, I merely refer to it in illustration of my views, and will now offer a few reasons for supposing that it may eventually be found an œconomical process for some of the more ordinary branches of our manufactures in metal.

I can now only explain the principles which lead me to imagine that it would not be a costly process on the large scale. In my preliminary experiments, I have used Mr. Smee's battery of platinated silver and zinc, excited by diluted sulphuric acid; it acts well, and is more convenient than the sulphate of copper batteries.

Mr. Spencer's very simple and cheap arrangements, in which the negative plate of a single pair is that receiving the deposit, is I think very suitable for flat work, but for complicated forms I should certainly prefer having the battery arrangement separate from the depositing cell, both on account of its being more manageable, and because in this way the solution of the sulphate of copper is kept constantly at the point of saturation by the destruction of a portion of the copper plate connected with the negative plate of the battery; of course the solution of this plate is effected by the portion of acid set free by depositing its copper on the mould, assisted by the galvanic action; so that in fact this method gives us the power of removing the material from any old and irregular piece of metal, and depositing it in the most highly wrought mould of which we may wish to obtain a copy.

In taking a comparative view of the probable cost of production, it will not be necessary to consider the first outlay for apparatus and moulds, because there must be a much larger outlay before commencing operations in the usual way; and leaving this out of the question, the cost of any electro-deposit would be made up of the value of the raw material, of the zinc and acid used in the battery, and of the time employed in making the arrangement.

The raw material may be obtained in considerable quantities, at its lowest price, as old sheathing copper would be admirably adapted to the purpose, on account of the facility with which it may be bent to any required form. The zinc and acid used in the battery would only have combined to form sulphate of zinc, which would be of some value. The time required for making the arrangements in the different troughs of a large factory, would be comparatively small, because one man may keep a great number at work, as they only require attention once in twenty-four hours—of course there must be many other persons employed in such a factory, in preparing new patterns, and new battery plates and connexions, but these come under other heads of expenditure which have their counterparts in the present mode.

I will now suggest a few of the plans which may be used for producing hollow vessels. The most simple process which has occurred to me, and the one which I have already used with success, is to form a block of some easily fused material into the shape of the interior of the required vessel, which serves as the matrix for depositing the copper on; when this is sufficiently thick, it may be placed on the fire till the block is melted, when the fluid material may be poured from any of its openings, and leave the hollow vessel complete and in a single piece, without the aid of the soldering iron or hammer. The temporary block on which this vessel is deposited may be formed of wax, cement, fusible metal, lead, or any other convenient material which melts at a much lower temperature than the metal to be deposited. I consider this method to be peculiarly applicable to the most complicated forms of metallic musical instruments, and to crooked tubular work generally; but it has certainly the great disadvantage of requiring a new mould for every repetition of the work, *or rather a new block*, for these may in many cases be cast in a mould which would be permanent for any number of repetitions. There is however one means of avoiding this in some cases, which is very well illustrated by the common *boot-tree*; if this were put together, and after having its surface metalized, placed in the depositing trough for a proper time, we should certainly have it covered with copper; we may then take out the centre or key-piece, which would unlock the others, and they may readily be removed, leaving a copper boot—not a very desirable article certainly, but the block may just as well have been formed to a more suitable shape. Still there are disadvantages in this mode of procedure which would limit the application to a few articles, although there are some which could not readily be accomplished in any other way. For the production of ornamental vases and other works of value, I would suggest the following method:—Let the whole, or a segment of the work, according to the nature of the design, be moulded in wax, carved in wood, cast in plaster, or produced in any more convenient way; then metalize the surface, and deposit on as large a portion of the original as will relieve in one piece; repeat this at different times over the whole work, and so manage the edges of each piece that the whole shall key together in the usual way, so that when finished you will possess a metallic mould for the required work. Should the design be complicated in form, a number of pieces will be requisite; but there are many very expensive articles which would readily relieve if the mould were in a single piece, and a very much more extensive class which may be made in two parts. Of course the inside of these metallic segments would correspond in polish and finish to the original model, and the metal mould, when complete, would serve for an unlimited number of copies, which would not require any cleaning up or hand finishing, but the parts would require to be put together, which I propose to effect by the same power that produced them; this I think may be done by covering the work with wax varnish while warm, and afterwards carefully cleaning all the edges, and then binding it together as if for soldering; the joints must now be touched over with nitric acid, and the article properly connected with the battery, and placed in the depositing trough; copper will immediately begin to deposit on the joints, and in a few hours I imagine that these joints would be quite as strong as any other part; if so, the method would have the decided advantage of joining the work with pure copper, and of doing this without exposing any part of it to the fire. I have not yet attempted to join any work in this way, but I consider that I have good evidence of its practicability, in the well-known fact of deposited copper uniting with any other portion of copper which has been cleaned with nitric acid, quite as intimately as the different parts of the original piece are united.

In the foregoing remarks I have endeavoured to show, that the system of electro-metallic deposits is capable of producing every article which leaves the workshop of the coppersmith, and that there is no very apparent reason for supposing that it would be too costly to compete with the usual processes of manufacture; still much of this opinion is speculative, and as yet I have not sufficient experimental evidence to offer in support of it; but such as I have, has fully satisfied me that it is correct. The small toy which accompanied this paper, was made a few days before the meeting, and I believe it is the first symmetrical copper vessel ever produced by galvanic action.

LXIX. *On the Chemical Statics of Organized Beings.*

By M. Dumas.

[Continued from p. 347, and concluded.]

III.—LET a seed be thrown into the earth, and be left to germinate and develop itself; let the new plant be watched until it has borne flowers and seeds in its turn, and we shall see by suitable analyses, that the primitive seed, in producing the new being, has fixed carbon, hydrogen, oxygen, azote and ashes.

Carbon.—The carbon originates essentially from carbonic acid, whether it be borrowed from the carbonic acid of the air, or proceed from that other portion of carbonic acid which the spontaneous decomposition of manures continually gives out in contact with the roots.

But it is from the air especially that plants most frequently derive their carbon. How could it be otherwise when we see the enormous quantity of carbon which aged trees, for example, have appropriated to themselves, and yet the very limited space within which their roots can extend? Certainly, when a hundred years ago the acorn germinated, which has produced the oak that we now admire, the soil on which it fell did not contain the millionth part of the carbon that the oak itself now contains. It is the carbonic acid of the air which has supplied the rest, that is to say, nearly the whole.

But what can be clearer and more conclusive than the experiment of M. Boussingault, in which peas, sowed in sand, watered with distilled water, and having no aliment but air, have found in that air all the carbon necessary for development, flowering and fructification?

All plants fix carbon, all borrow it from carbonic acid, whether this be taken directly from the air by the leaves, whether the roots imbibe within the ground the rain water impregnated with carbonic acid,—or whether the manures, whilst decomposing in the soil, supply carbonic acid, which the roots also take possession of to transmit it to the leaves.

All these results may be proved without difficulty. M. Boussingault observed that vine leaves which were inclosed in a globe, took all the carbonic acid from the air directed across the vessel, however rapid the current. M. Boucherie also observed enormous quantities of carbonic acid escape from the divided trunk of trees in full sap, evidently drawn by the roots from the soil.

But if the roots imbibe this carbonic acid within the earth, if this passes into the stalk and from thence into the leaves, it ends by being exhaled into the atmosphere, without alteration, when no new force intervenes.

Such is the case with plants vegetating in the shade or at night. The carbonic acid of the earth filters through their tissues and diffuses itself in the air. We say that plants produce carbonic acid during the night; we should say, in such a case, that plants transmit the carbonic acid borrowed from the soil.

But let this carbonic acid, proceeding from the soil or taken from the atmosphere, come into contact with the leaves or the green parts, and let the solar light moreover intervene, then the scene all at once changes.

The carbonic acid disappears; bubbles of free oxygen arise on all the parts of the leaf, and the carbon fixes itself in the tissues of the plant.

It is a circumstance well worthy of interest, that these green parts of plants, the only ones which up to this time manifest this admirable phænomenon of the decomposition of carbonic acid, are also endowed with another property not less peculiar, or less mysterious.

In fact, if their image were to be transferred into the apparatus of M. Daguerre, these green parts are not found to be reproduced there; as if all the chemical rays, essential to the Daguerrian phænomena, had disappeared in the leaf, absorbed and retained by it.

The chemical rays of light disappear, therefore, entirely in the green parts of plants; an extraordinary absorption doubtless, but which explains without difficulty the enormous expense of chemical force necessary for the decomposition of a body so stable as carbonic acid.

What, moreover, is the function of this fixed carbon in the plant? for what is it destined? For the greater part, without doubt, it combines with water or with its elements, thus giving birth to matters of the highest importance for the vegetable.

If twelve molecules of carbonic acid are decomposed and abandon their oxygen, the result will be twelve molecules of carbon, which, with ten molecules of water, may constitute either the cellular tissue of plants, or their ligneous tissue, or the starch and the dextrine which are produced from them.

Thus in any plant whatever, nearly the entire mass of the structure (charpente), formed as it is of cellular tissue, of ligneous tissue, of starch, or of gummy matters, will be represented by twelve molecules of carbon united to ten molecules of water.

The ligneous part, which is insoluble in water,—the starch, which gelatinizes (*l'amidon, qui fait empois*) in boiling water, —and the dextrine, which dissolves so easily in water cold or hot, constitute therefore, as M. Payen has so well proved, three bodies possessing exactly the same composition, but diversified by a different molecular arrangement.

Thus, with the same elements, in the same proportions, vegetable nature produces either the insoluble walls of the cells of cellular tissue, and of the vessels, or starch which she accumulates as nourishment around buds and embryos, or the soluble dextrine which the sap can convey from one place to another for the wants of the plant.

How admirable is this fecundity, which out of the same body can make three different ones, and which allows of their being changed one into the other, with the slightest expense of force, every time occasion requires it!

It is also by means of carbon united with water, that the saccharine matters so frequently deposited in the organs of plants for peculiar purposes, which we shall shortly mention, are produced. Twelve molecules of carbon and eleven molecules of water form the cane sugar. Twelve molecules of carbon and fifteen molecules of water make the sugar of the grape.

These ligneous, amylaceous, gummy and saccharine matters, which carbon, taken in its nascent state, can produce by uniting with water, play so large a part in the life of plants, that, when they are taken into consideration, it is no longer difficult to understand the important part that the decomposition of carbonic acid performs in plants.

Hydrogen.—In the same manner that plants decompose carbonic acid for the appropriation of its carbon, and in order to form together with it all the neutral bodies which compose nearly their entire mass, in the same way, and for certain products which they form in less abundance, plants decompose water and fix its hydrogen. This appears clearly from M. Boussingault's experiments on the vegetation of peas in closed vessels. It is still more evident from the production of fat or volatile oils so frequent in certain parts of plants, and always so rich in hydrogen. This can only come from water, for the plant receives no other hydrogenated product than the water itself.

These hydrogenated bodies, to which the fixation of the hydrogen borrowed from the water gives birth, are employed by plants for accessory uses. They form indeed the volatile oils which serve for defence against the ravages of insects; fat oils or fats, which surround the seed, and which serve to develop heat by oxidation (*en se brûlent*) at the moment of germination; waxes with which leaves and fruits are covered so as to become impermeable to water.

But all these uses constitute some accidents only in the life of plants; thus the hydrogenated products are much less necessary, much less common in the vegetable kingdom than the neutral products formed of carbon and water.

Azote.—During its life, every plant fixes azote, whether it borrows the azote from the atmosphere, or takes it from the manure. In either case it is probable that the azote enters the plant and acts its part there only under the form of ammonia or of nitric acid.

M. Boussingault's experiments have proved that certain plants, such as Jerusalem artichokes, borrow a great quantity of azote from the air; that others, such as wheat, are on the contrary obliged to derive all theirs from manure; a valuable distinction for agriculture; for it is evident that all cultivation should begin by producing vegetables which assimilate azote and air, to rear by their aid the cattle which will furnish manure, and employ this latter for the cultivation of certain plants, which can take azote from the manures only.

One of the most interesting problems of agriculture consists then in the art of procuring azote at a cheap rate. As for carbon, no trouble need be taken about it; nature has provided for it; the air and rain water suffice for it. But the azote of the air, that which the water dissolves and brings with it; the ammoniacal salts which rain water itself contains, are not always sufficient. With regard to most plants, the cultivation of which is important, their roots should also be surrounded with azotated manure, a permanent source of ammonia or of nitric acid, which the plant appropriates as they are produced. This, as we know, is one of the great expenses of agriculture, one of its great obstacles, for it possesses only the manure which is of its own production. But chemistry is so far advanced in this respect, that the problem of the production of a purely chemical azotated manure cannot be long in being resolved.

M. Schattenman, the skilful director of the manufactories of Bouxvilliers in Alsace, M. Boussingault and M. Liebig, have turned their attention to the functions of ammonia in azotated manures. Recent trials show that the nitric acid of the nitrates also merit particular attention.

But for what purpose is this azote, of which plants seem to have such an imperious want? M. Payen's researches partly teach us, for they have proved that all the organs of the plant, without exception, begin by being formed of an azotated matter analogous to fibrine, with which at a later period are associated the cellular tissue, the ligneous tissue, and the amylaceous tissue itself. This azotated matter, the real origin of all the parts of the plant, is never destroyed; it is always to be found, however abundant may be the non-azotated matter which has been interposed between its particles.

This azote, fixed by plants, serves therefore to produce a concrete fibrinous substance which constitutes the rudiment of all the organs of the vegetable.

It also serves to produce the liquid albumen which the coagulable juices of all plants contain, and the caseum, so often confounded with albumen, but so easy to recognize in many plants.

Fibrin, albumen and caseum, exist then in plants. These three products, identical in their composition, as M. Vogel has long since proved, offer a singular analogy with the ligneous matters, the amidon, and the dextrine.

Indeed, fibrin is like ligneous matter, insoluble; albumen, like starch, coagulates by heat; caseum, like dextrine, is soluble.

These azotated matters moreover are neutral, as well as the three parallel non-azotated matters; and we shall see that by their abundance in the animal kingdom they act the same part that these latter exhibited to us in the vegetable kingdom.

Besides, in like manner as it suffices for the formation of non-azotated neutral matters, to unite carbon with water or with its elements, so also for the formation of these azotated neutral matters, it suffices to unite carbon and ammonium with the elements of water; forty-eight molecules of carbon, six of ammonium, and seventeen of water, constitute, or may constitute, fibrine, albumen and caseum.

Thus in both cases, reduced bodies, carbon or ammonium, and water, suffice for the formation of the matters which we are considering, and their production enters quite naturally into the circle of reactions, which vegetable nature seems especially adapted to produce.

The function of azote in plants is therefore worthy of the most serious attention, since it is this which serves to form the fibrin which is found as the rudiment in all the organs, since it is this which serves for the production of the albumen and caseum, so largely diffused in so many plants, and which animals assimilate or modify according to the exigencies of their own nature.

It is in plants then that the true laboratory of organic chemistry resides;—thus carbon, hydrogen, ammonium and water, are the principles which plants elaborate; ligneous matter, starch, gums, and sugars on the one part, fibrin, albumen, caseum and gluten on the other, are then the fundamental products of the two kingdoms; products formed in plants and in plants alone, and transferred by digestion into animals.

Ashes.—An immense quantity of water passes through the vegetable during the period of its existence. This water evaporates at the surface of the leaves and necessarily leaves as residue, in the plant, the salts which it contained in solution. These salts compose the ashes, products evidently borrowed from the earth, to which, after their death, vegetables give it back again.

As to the form in which these mineral products deposit themselves in the vegetable tissue, nothing can be more variable. We may remark, however, that among the products of this nature, one of the most frequent and most abundant is that pectinate of lime, discovered by M. Jacquelain in the ligneous tissue of most plants.

IV.—If, in the dark, plants act as simple filters which water and gases pass through; if, under the influence of solar light they act as reducing apparatus which decompose water, carbonic acid and oxide of ammonium, there are certain epochs and certain organs in which the plant assumes another, and altogether opposite part.

Thus, if an embryo is to be made to germinate, a bud to be unfolded, a flower to be fecundated, the plant which absorbed the solar heat, which decomposed carbonic acid and water, all at once changes its course. It burns carbon and hydrogen; it produces heat, that is to say, it takes to itself the principal characters of animal life.

But here a remarkable circumstance reveals itself. If barley or wheat is made to germinate, much heat, carbonic acid and water are produced. The starch of these grains first changes into gum, then into sugar, then it disappears in producing carbonic acid, which the germ is to assimilate. Does a potato germinate, here also it is its starch which changes into dextrine, then into sugar, and which at last produces carbonic acid and heat. Sugar, therefore, seems the agent by means of which plants develop heat as they need it.

How is it possible not to be struck from this with the coincidence of the following facts?—Fecundation is always accompanied by heat; flowers as they breathe produce carbonic acid. They therefore consume carbon; and if we ask whence this carbon comes, we see in the sugar cane, for example, that the sugar accumulated in the stalk has entirely disappeared when the flowering and fructification are accomplished. In the beet root, the sugar continues increasing in the roots until it flowers; the seed-bearing beet contains no trace of sugar in its root. In the parsnep, the turnip and the carrot, the same phænomena take place.

Thus at certain epochs, in certain organs, the plant turns into an animal; it becomes like it an apparatus of combustion; it burns carbon and hydrogen; it gives out heat.

But at these same periods, it destroys in abundance the saccharine matters which it had slowly accumulated and stored up. Sugar, or starch turned into sugar, are then the primary substances by means of which plants develop heat as required for the accomplishment of some of their functions.

And if we remark with what instinct animals, and men too, choose for their food just that part of the vegetable in which it has accumulated the sugar and starch which serve it to develop heat, is it not probable, that, in the animal œconomy, sugar and starch are also destined to act the same part, that is to say, to be burned for the purpose of developing the heat which accompanies the phænomenon of respiration?

To sum up, as long as the vegetable preserves its most habitual character, it draws from the sun heat, light, and chemical rays. From the air it receives carbon, from water it takes hydrogen, azote from the oxide of ammonium, and different salts from the earth. With these mineral or elementary substances, it composes the organized substances which accumulate in its tissues.

They are ternary substances, ligneous matter, starch, gums and sugars.

They are quaternary substances, fibrin, albumen, caseum, and gluten.

So far then the vegetable is an unceasing producer; but if at times, if to satisfy certain wants, the vegetable becomes a consumer, it realizes exactly the same phænomena which the animal will now set before us.

V.—An animal in fact constitutes an apparatus of combustion from which carbonic acid is continually disengaged, in which consequently carbon undergoes combustion.

You know that we were not stopped by the expression *cold-blooded animals*, which would seem to designate some animals destitute of the property of producing heat. Iron, which burns vividly in oxygen, produces a heat which no one would deny; but reflection and some science is necessary in order to perceive, that iron which rusts slowly in the air disengages quite as much, although its temperature does not sensibly vary. No one doubts that lighted phosphorus in burning produces a great quantity of heat. Unkindled phosphorus also burns in the air, and yet the heat which it develops in this state was for a long time disputed.

So as to animals, those which are called warm-blooded burn much carbon in a given time, and preserve a sensible excess of heat above the surrounding bodies; those which are termed cold-blooded burn much less carbon, and consequently retain so slight an excess of heat, that it becomes difficult or impossible to observe it.

But nevertheless, reflection shows us that the most constant character of animal existence resides in this combustion of carbon, and in the development of carbonic acid which is the result of it, beginning also in the production of heat which every combustion of carbon occasions.

Whether the question be of superior or inferior animals; whether this carbonic acid be exhaled from the lungs or from the skin, does not signify; it is always the same phænomenon, the same function.

At the same time that animals burn carbon, they also burn hydrogen; this is a point proved by the constant disappearance of hydrogen which takes place in their respiration.

Besides, they continually exhale azote. I insist upon this point, and principally in order to banish an illusion which I cannot but believe to be one of the most prejudicial to your studies. Some observers have admitted that there is an absorption of azote in respiration, but which never appears unaccompanied by circumstances that render it more than doubtful. The constant phænomenon is the exhalation of gas.

We must therefore conclude with certainty, that we never borrow azote from the air; that the air is never an aliment to us; and that we merely take from it the oxygen necessary to form carbonic acid with our carbon, and water with our hydrogen.

The azote exhaled proceeds then from the aliments, and it originates from them entirely. This, in the general œconomy of nature, may in thousands of centuries be absorbed by plants, which, like Jerusalem artichokes, draw their azote directly from the air.

But this is not all the azote which animals exhale. Every one gives out by the urine, on an average, as M. Lecanu has proved, 230 grains of azote a day, of azote evidently drawn from our food, like the carbon and hydrogen which are oxidized within us (*que nous brûlons*).

In what form does this azote escape? In the form of ammonia. Here indeed, one of those observations presents itself which never fail to fill us with admiration for the simplicity of the means which nature puts in operation.

If in the general order of things we return to the air the azote which certain vegetables may sometimes directly make use of, it ought to happen that we should also be bound to return ammonia, a product so necessary to the existence and development of most vegetables.

Such is the principal result of the urinary secretion. It is an emission of ammonia, which returns to the soil or to the air.

But is there any need to remark here, that the urinary organs would be changed in their functions and in their vitality by the contact of ammonia? the contact of the carbonate of ammonia would even effect this; and so nature causes us to excrete urea.

Urea is carbonate of ammonia, that is to say, carbonic acid like that which we expire, and ammonia such as plants require. But this carbonate of ammonia has lost of hydrogen and oxygen, so much as is wanting to constitute two molecules of water.

Deprived of this water the carbonate of ammonia becomes urea; then it is neutral, not acting upon the animal membranes; then it may pass through the kidneys, the ureters, and the bladder, without inflaming them; but having reached the air, it undergoes a true fermentation, which restores to it these two molecules of water, and which makes of this same urea true carbonate of ammonia; volatile, capable of exhaling in the air; soluble, so that it may be taken up again by rain; and consequently destined thus to travel from the earth to the air and from the air to the earth, until, pumped up by the roots of a plant and elaborated by it, it is converted anew into an organic matter.

Let us add another feature to this picture. In the urine, along with urea, nature has placed some traces of albuminous or mucous animal matter, traces which are barely sensible to analysis. This, however, when it has reached the air, is there modified, and becomes one of those ferments of which we find so many in organic nature; it is this which determines the conversion of urea into carbonate of ammonia.

These ferments, which have so powerfully attracted our attention, and which preside over the most remarkable metamorphoses of organic chemistry, I reserve for the next year, when I shall give you a still more particular and full account of them.

Thus we discharge urea accompanied by this ferment, by this artifice, which acting at a given moment, turns this urea into carbonate of ammonia.

If we restore to the general phænomenon of animal combustion that carbonic acid of the carbonate of ammonia which of right belongs to it, there remains ammonia as the characteristic product of urine.

Thus, by the lungs and the skin, carbonic acid, water, azote;

By the urine, ammonia.

Such are the constant and necessary products which exhale from the animal.

These are precisely those which vegetation demands and makes use of, just as the vegetable in its turn gives back to the air the oxygen which the animal has consumed.

Whence come this carbon, this hydrogen burnt by the animal, this azote which it has exhaled in a free state or converted into ammonia? They evidently come from the aliments.

By studying digestion in this point of view, we have been led to consider it in a manner much more simple than is customary, and which may be summed up in a few words.

In fact, as soon as it was proved to us that the animal creates no organic matter; that it merely assimilates or expends it by burning it (*en la brûlant*), there was no occasion to seek in digestion all those mysteries which we were quite sure of not finding there.

Thus digestion is indeed but a simple function of absorption. The soluble matters pass into the blood, for the most part unchanged; the insoluble matters reach the chyle, sufficiently divided to be taken up by the orifices of the chyliferous vessels.

Besides, the evident object of digestion is to restore to the blood a matter proper for supplying our respiration with the ten or fifteen grains of coal, or the equivalent of hydrogen which each of us burns every hour, and to restore to it the grain of azote which is also hourly exhaled, as well by the lungs or the skin as by the urine.

Thus the amylaceous matters are changed into gum and sugar; the saccharine matters are absorbed.

The fatty matters are divided, and converted into an emulsion, and thus pass into the vessels, in order to form depôts which the blood takes back and burns as it needs.

The neutral azotated substances, fibrin, albumen and caseum, which are at first dissolved, and then precipitated, pass into the chyle greatly divided or dissolved anew.

The animal thus receives and assimilates almost unaltered the azotated neutral substances which it finds ready formed in the animals or plants upon which it feeds; it receives fatty matters which come from the same sources; it receives amylaceous or saccharine matters which are in the same predicament.

These three great orders of matters, whose origin always ascends to the plant, become divided into products capable of being assimilated, fibrin, albumen, caseum, fatty bodies, which serve to renew or recruit the organs with the combustible products, sugar and fatty bodies which respiration consumes.

The animal therefore assimilates or destroys organic matters ready formed; it does not create them.

Digestion introduces into the blood organic matters ready formed; assimilation employs those which are azotated; respiration burns the others.

If animals do not possess any peculiar power for producing organic matters, have they at least that special and singular power which has been attributed to them of producing heat without expenditure of matter?

You have seen, while discussing the experiments of MM. Dulong and Despretz, you have positively seen the contrary result from them. These skilful physicists supposed that an animal placed in a cold water calorimeter comes out of it with the same temperature that it had on entering it; a thing absolutely impossible, as is now well known. It is this cooling of the animal, of which they took no account, that expresses in their *tableaux* the excess of heat attributed by them and by all physiologists to a calorific power peculiar to the animal and independent of respiration.

It is evident to me that all animal heat arises from respiration; that it is measured by the carbon and hydrogen burnt. In a word, it is evident to me that the poetical comparison of a rail-road locomotive to an animal is founded on a more serious basis than has perhaps been supposed. In each there are combustion, heat, motion, three phænomena connected and proportional.

You see, that thus considering it, the animal machine becomes much easier to understand; it is the intermediary between the vegetable kingdom and the air; it borrows all its aliments from the one, in order to give all its excretions to the other.

Shall I remind you how we viewed respiration, a phænomenon more complex than Laplace and Lavoisier had thought, or even Lagrange had supposed, but which precisely, as it becomes complicated, tends more and more to enter into the general laws of inanimate nature?

You have seen that the venous blood dissolves oxygen and disengages carbonic acid; that it becomes arterial without producing a trace of heat. It is not then in becoming arterial that the blood produces heat.

But under the influence of the oxygen absorbed, the soluble matters of the blood change into lactic acid, as MM. Mitscherlich, Boutron-Charlard and Fremy observed; the lactic acid is itself converted into lactate of soda; this latter by a real combustion into carbonate of soda, which a fresh portion of lactic acid decomposes in its turn. This slow and continued succession of phænomena which constitutes a real combustion, but decomposed at several times, in which we see one of the slow combustions to which M. Chevreul drew attention long ago, this is the true phænomenon of respiration. The blood then becomes oxygenized in the lungs; it really breathes in the capillaries of all the other organs, there where the combustion of carbon and the production of heat principally take place.

A last reflection. To ascend to the summit of Mont-Blanc, a man takes two days of twelve hours. During this time, he burns at an average 300 grammes of carbon, or the equivalent of hydrogen. If a steam-engine had been employed to take him there, it would have burnt from 1000 to 1200 to accomplish the same work.

Thus, viewed as a machine, borrowing all its power from the coal that it burns, man is an engine three or four times more perfect than the most perfect steam-engine. Our engineers have therefore still much to do, and yet these numbers are quite such as to prove that there is a community of principles between the living engine and the other; for if we allow for all the inevitable losses in steam-engines which are so carefully avoided in the human machine, the identity of the principle of their respective powers appears manifest and clear.

But we have followed far enough considerations as to which your own reflections are already in advance of me, and where your recollections leave me nothing more to do.

To sum up, then, we see that of the primitive atmosphere of the earth three great parts have been formed:

One which constitutes the actual atmospheric air; the second, which is represented by vegetables, the third by animals.

Between these three masses, continual exchanges take place: matter descends from the air into plants, enters by this route into animals, and returns to the air according as these make use of it.

Green vegetables constitute the great laboratory of organic chemistry. It is they which, with carbon, hydrogen, azote, water and oxide of ammonium, slowly build up all the most complex organic matters.

They receive from the solar rays, under the form of heat or of chemical rays, the powers necessary for this work.

Animals assimilate or absorb the organic matters formed by plants. They change them by little and little, they destroy them. In their organs, new organic substances may come into existence, but they are always substances more simple, more akin to the elementary state than those which they have received. By degrees these decompose the organic matters slowly created by plants; they bring them back little by little towards the state of carbonic acid, water, azote and ammonia, a state which allows them to be returned to the air.

In burning or destroying these organic matters, animals always produce heat, which radiating from their bodies in space, goes to supply the place of that which vegetables had absorbed.

Thus all that air gives to plants, plants give up to animals, and animals restore it to the air, an eternal circle in which life keeps in motion and manifests itself, but in which matter merely changes place.

The brute matter of air, organized by slow degrees in plants, comes, then, to perform its part without change in animals, and serves as an instrument for thought; then vanquished by this effort and broken, as it were, it returns brute matter to the great reservoir whence it came.

Allow me to add, in finishing this picture, which sums up opinions which to my mind are but the necessary consequences and developments of the great path which Lavoisier marked out for modern chemistry; allow me, I say, to express myself as he did with regard to his fellow-labourers and his friends.

If in my lessons, if in this summing up, I have chanced to adopt without mentioning them the experiments or the opinions of M. Boussingault, it is that the habit of communicating to each other our ideas, our observations, our manner of viewing things, has established between us a community of opinions, in which we ourselves even afterwards find it difficult to distinguish what belongs to each of us.

In resting these opinions and their consequences on his name and on his authority, in telling you that we work actively, sometimes together, and sometimes apart, in order to verify and to develop all these facts, all these results by experiment, I do but evince my anxious desire to justify the interest which you have this year taken in my labours.

For this I beg to thank you. It has given me courage to undertake a long course of researches; if anything useful to the progress of humanity should result from them, let all the honour of it redound to the intelligent good will with which you have constantly surrounded me, and for which I shall ever be profoundly grateful.

LXX. *On the question, Whether there are any evidences of the former existence of Glaciers in North Wales?* By J. E. Bowman, *F.L.S. & F.G.S.*[*]

THE recent discovery of traces of ancient glaciers in Scotland and the north of England, rendered it probable that similar appearances would be found in North Wales, where, though situated in a somewhat lower latitude, the slight increase of temperature is fully counteracted by the greater altitude of the central mountain group. The geographical situation of all these parts of our island with respect to the sea, and the geological periods of their elevation may be considered to be so nearly the same, that if glaciers can be satisfactorily proved to have once existed in one, there seems no reason to doubt their occurrence in the rest, even admitting them to have had a much more local origin and limited range than Prof. Agassiz has supposed. If we compare the mountains of Britain with those of Switzerland, where the most un-

[*] Communicated by the Author.

doubted evidence of the former greater extent of glaciers may be gradually traced from the warm valleys of the Alps up to the actual feet of existing glaciers, the greater altitudes of the mountains and increased distance from the modifying influence of the sea, will be found more than adequate to compensate for their being situated ten degrees nearer to the equator. In this view it is therefore possible, assuming the theory of Prof. Agassiz of a general reduction of temperature in ancient times over the whole northern hemisphere, that Switzerland might be covered with ice and snow, while Britain might be comparatively free from them. But this reasoning, though plausible on a cursory glance, falls to the ground, if we accept the theory of this distinguished philosopher, that the glacial period occurred antecedent to the elevation of the Alps. There is abundance of direct geological evidence to prove that they have been raised, at least much higher than before, since the deposition and consolidation of the tertiary strata; and as it is scarcely less certain that the primary mountains of Britain were raised at a much earlier period, it seems to follow from their altitude and higher latitude, that the surrounding region must have been the seat of more intense cold than Switzerland, *then* without high mountains and situated in a much warmer climate.

Having spent a portion of the late summer in endeavouring to trace the geological sequence of the schistose rocks of North Wales, I have been naturally led into some high and unfrequented tracts where it seemed probable, if anywhere, that evidence of glaciers would be found. I have examined many of the main and lateral valleys on the east and south flanks of the Snowdon group, of the Arenigs, and of the north end of Berwyn chain, as well as of the moorland hills of Denbighshire, between Cerrig y Druidion and Llanrwst; also those which intersect the wild and barren hills that occupy the country between the great Holyhead road and Bala, and form the dividing ridge between Merionethshire and Denbighshire. These valleys descend from the highest mountains of North Wales, and are the troughs through which the numerous tributaries of the Dee, the Clwyd, and the Conway, are carried into the lower parts of the country. Many of them, from their smooth uniform slopes and easy inclinations, seem calculated to give rise to, and to retain those appearances which glaciers usually create and leave behind them. In none of them, however, have I found any features which appeared to me to have been produced by glaciers; none at least have retained that outward form or shape which seemed to justify me in referring them to such an origin.

There are, however, various appearances scattered here and there through the districts I have named, which, if they have not originated in glaciers, bear a sufficient resemblance to require a close examination. It is more important to make this examination than may at first sight appear, in order to guard those who have not witnessed the actual effects of glacial action from attributing every accumulation of gravel or streaked rock to such an origin. Though satisfied myself, from personal observation, that existing glaciers form moraines on their sides and at their feet, and abrade the rocks over which they move, so as to scratch and polish their surfaces, it has constantly been my practice to see, in the first place, if the analogous appearances I have met with in North Wales were not capable, on taking into account all the local circumstances, of satisfactory explanation by some obvious existing cause; conceiving it to be improper hastily to refer to the operation of ice, effects which would admit of an equally easy explanation from the transporting or erosive power of water.

Of true lateral moraines ranging along the slopes of the valleys more or less horizontally, and formed either of stones or gravel, no instance has fallen under my observation. I passed through many valleys, where, from the gradual rise of the sides and the laminar or schistose structure of the rock, the detritus from above had lodged and accumulated into a talus, or formed continuous inclined planes of great uniformity, which were often covered with herbage. This was strikingly seen in the valley above Penmachno, but I could not discover any trace of lateral moraines, nor of terminal ones, about the outlet of the valley into the great trough of the Conway. In the neighbouring valley of the infant Conway, between Yspytty and Pentre Voelas, where the river first runs parallel with the great Holyhead road, the slopes are covered on both sides by a succession of rounded wavy knolls or swellings, which are very striking in such a barren and rugged district, from their smoothness and rich verdure, a character by which they may be recognised at a considerable distance. Near the lower end of the valley, on the east side, several of them are detached, and are about fifty yards diameter and twenty or thirty yards high; but they usually run into one another and are of no regular shape, except that they all slope towards the stream, so that they look like great undulating and projecting breastworks and bastions thrown up for military defence. Some of these mounds cover an area of 100 acres, and are two or three hundred feet high, looking like little hills, but without any regularity or uniformity in the grouping, their bases running into one another and sloping towards the middle of the valley on both sides. One only near the little stream of the Clettwr and above the road, seemed, when seen from a distance, to have a pretty level top; but I observed none with steep sides or escarpments, as though they had been eroded or cut down by a current. The road from Pentre Voelas to Yspytty winds among them, the higher ones being above it to the S.E., the rest inclining towards the valley below. Similar mounds mask the rocky slope on the opposite bank of the Conway as high as half a mile above Yspytty, becoming gradually lower and thinner till they disappear altogether. In a north-west direction they extend from Pentre Voelas to the opening of the valley of the Machno, and may often be seen on both sides the great road in the less precipitous parts of the gorge, though more frequently and more distinctly on the left bank of the river. From partial openings into several of them, I saw they were formed of local drift or gravel from the neighbouring dark schistose rocks, without any large boulders. In one of them, near the great road, there was a streaked or bedded character, the layers being formed of pebbles of different sizes, as though they had been left by a current which had varied in intensity. It appeared to me that these hills were not composed throughout of this gravelly drift, but that this material only formed a coat or covering upon the previously existing prominences of the lower Silurian schist of the district, sufficiently thick to conceal the rocky nucleus and to give it a smooth rounded outline. Neither could I attribute their shape to any form or modification of lateral moraine; and I was of opinion, that even should any one contend that they had originated in a glacier, it would be necessary to admit they must have been subsequently modified by water. It is not easy to understand how so large a mass of diluvium could have been accumulated in this particular locality. Seeing that the river Conway, soon after passing through it, has to make its way along a narrow gorge, and that from the precipitous form of the rocks on each side, it must always have flowed in the same channel, I was at first inclined to suppose that some obstruction might have filled this gorge and pounded up the river and the detritus it had brought down from the higher lands. But on examination of their relative levels, it appeared that the gravelly hills above the road east of Yspytty were higher than the surface of a lake so dammed up, and consequently they could not have originated from such a cause. It is singular that in no other neighbouring valley of similar character, were there any accumulations of the same material, and further observation will be necessary to explain their origin.

The next class of appearances I have met with in North Wales resembling those produced by glacier action, are the striæ or scratches on the surfaces of rocks. Of these I have found several decided and strongly marked instances; and as they are due to various causes, and can in some cases be demonstrated *not* to have been connected with glaciers, it may be useful to describe them somewhat in detail.

The first I met with occurs in the Upper Silurian rocks on the right bank of the Dee, in the immediate neighbourhood of Llangollen. Here the true stratification is easily ascertained, since it is not affected by any cleavage, though the rock has a system of joints which divide the beds diagonally to the planes of the surface into irregular rhombs. The beds are nearly horizontal, having an average dip to the S.S.E. of about five or six degrees; and they rise to the height of from 300 to 400 feet, without any apparent dislocation or change in the direction of the dip. At the base of this precipitous hill, and in a little quarry, the outcrop of the beds is seen in various places; their surfaces are generally smooth and uniformly plane, occasionally slightly undulated. I observed the level surface of one of them to be streaked or traced with strong and perfectly parallel grooves or furrows, some as fine as though they were made by the point of a pin, and others the eighth of an inch wide, promiscuously and closely ranged alongside others of intermediate dimensions. The whole surface was occupied by these grooves and their intermediate ridges, and had the appearance of having been planed with a tool with a snipped or indented edge. Some of the broader furrows were truncated and terminated abruptly, and one or more narrower ones seemed to be connected with and to issue from them. The range or bearing of the striæ was N.N.W. and S.S.E., and the breadth of surface exposed was from twenty-five to thirty yards, every part of which was channeled in the manner described. It was at once evident, from the general appearance of the quarry, that this bed had never been exposed or disturbed from those above or below it, since it was closely and conformably overlaid by the mass above, the whole having a slight inclination to the S.S.E. The furrows were also evidently continued inwards upon the yet undisturbed portions; but to remove all doubt upon this head, I employed a quarryman to cut away the superincumbent bed more than a foot inwards from the face, where they were found to be equally clear and strong. The specimens I obtained were from this part, and had never before been exposed since the original deposition of the beds. The under surface of the superincumbent bed was moreover similarly though more faintly streaked;

in some places so obscurely, that it would not have been perceived to be so on casual inspection; but in others, very clearly. There was another bed lying about a foot lower in the series, similarly though less distinctly striated; and the striæ had exactly the same direction.

It was therefore abundantly clear that these grooved striæ could not have been produced by glacial action, or indeed by any cause operating after the original deposition or consolidation of the beds; inasmuch as they are nearly at *right angles* to the course of the river, and therefore to the *direction* of a glacier, and especially as they are found under beds in close contact, that have never suffered any partial or relative disturbance *inter se**. Since, however, they exist on two surfaces in contact, and somewhat resemble those polished portions of rocks well known by the name of Slickensides, which are believed to be the effect of friction on two surfaces rubbed against each other, it is worth while to examine whether they might not have been produced by a similar action. As already stated, the appearance of the quarry and precipice above the road shows that the rock has been tilted aside *en masse*, and has not been affected by any partial or subsequent dislocation; and the nearly horizontal position of the beds renders it extremely improbable, if not impossible, that they could have been so rubbed against each other as to produce the furrows in question. The appearance called Slickensides is found on the perpendicular or slanting sides of fissures and veins, and is caused by the unequal and repeated motion of the two surfaces against each other. But in proportion as the direction of these sides varies from the slanting to the horizontal position, this kind of action diminishes, from the increasing difficulty of maintaining motion on the horizontal plane. Great lateral pressure, from the gravity of an upheaved mass, may suddenly and for once act horizontally; but a succession of alternating throes or paroxysms can only be propagated from below; from any other point, motion would soon be overcome by the *vis inertiæ* of the mass. It is easy to understand how the upper portion of a stratified rock may slide off its base when but gently inclined; but it is scarcely credible that any possible circumstances could cause it, in such a position, to be moved backwards and forwards long enough to produce the grooved appearance impressed upon the bed in question.

---

* It is scarcely necessary to say that they differ essentially from the furrows which traverse the mountains of Scandinavia, as described by Seffström (see the last Part of Taylor's Scientific Memoirs); and from the furrowed and polished rocks of Fontainbleau, as noticed by Durocher in the Phil. Mag. for Nov. 1841.

Again, an examination of the striæ in hand specimens will show that some of the more delicate lines diverge from the general parallelism, and unite with the contiguous ones without suffering interruption from the intervening furrows. In some parts of the surface there are groups of faint transverse elevations and depressions which run across many contiguous striæ with a gentle curve; and in others the grooving is not confined to a single plane, but seems to penetrate beneath the surface, and to pervade several laminæ inwards. None of these appearances could have been produced by a hard body sliding along its surface in right lines, as in slickensides; and I regret my inability to offer any satisfactory explanation. The only conjecture I can make is, whether, as these striæ lie in the direction of the magnetic meridian, they may not have been produced by some electric or magnetic power acting upon the chemical ingredients of the bed or upon its surface, and causing them to assume a polaric direction while yet in a soft or semi-indurated state. They belong to a class of phænomena hitherto little noticed and still less understood, but which are nevertheless of great interest and worthy of especial attention, from the important bearing they have on questions of physical geology, about which we are at present almost totally in the dark, but which may one day throw light on the original structure of our globe.

I come now to describe a still more curious case of striated surface, found on a system of rocks lying geologically far below that just mentioned, and which, though unfossiliferous, will range with the Lower Silurian formation. It occurs in the bed of a little mountain stream named Clettwr, that falls into the Conway a mile or two below Yspytty, having cut its way through the great mounds of gravel already described in that vicinity and exposed the solid rock beneath. The rock is a very hard blue schist, in thick compact beds, with a dip of 15° N.N.E. and an obscure cleavage dipping 48° N.E., which has but partially obliterated the bedding. The true stratification is indicated by many thin streaked laminæ which appear as lines in the section, and are quite parallel with the undulated surface of the bed about to be described. This surface is somewhat glossy and very strongly marked with a system of wavy lines that preserve a general parallelism, but run into one another, and do not long preserve their individual and separate character. The surface may be more correctly said to be corrugated and undulated than grooved, for the striated lines do not possess the perfect parallelism and continuity of those near Llangollen, though they maintain an uniformity of character, are persistent over every part of the surface that was exposed, and were also found at intervals of a few yards, wherever I removed the overlying strata. Their direction is E.S.E. and W.N.W., or 45° W. of the magnetic meridian, and at right angles with the direction of the valley. But the most singular feature of this bed is a second system of closer, finer, and more regular lines which cross the larger undulations at an angle of about 70°, traversing their intermediate hollows as well as their crests and sides, thus covering the whole surface, and giving it a reticulated appearance very difficult to describe. Now and then, at irregular intervals, there is an occasional thicker line; or perhaps it would be more correct to say that the interval between the fine lines is greater in these places than usual. This double system of lines seems to originate in a thin plate of somewhat different material, about the eighth of an inch thick, and interstratified with the schist; for the lines cover both its upper and under surface, and the still thinner laminæ of which this plate is composed, are seen by their darker and lighter shades, to be undulated also throughout its whole thickness. This plate easily separates from the contiguous thicker beds, and when removed, is found to have left a sharp and perfect impression of both sets of lines on each of the surfaces with which it was in contact. Not only so, but the wavy lines have affected and penetrated the thicker beds, rendering them more glossy, and giving to the split of the cleavage a corresponding undulation which gradually dies away at the depth of one-third or half an inch beneath the surface. This is the more singular, as the cleavage planes cut the surface at an angle of 15° with the corrugated lines, and throw the undulations out of their regular course. There is another somewhat similar marking on the surface of a second bed in the same brook, about a foot higher in the series, and in the same direction or line of strike. Here the lines are broader and have been deeper; but being in the direct course of the stream, they are much obliterated by the water passing over them, and I did not bring away a specimen.

The reasoning already employed to show that the striæ on the Upper Silurian bed near Llangollen could not have been formed by a glacier, nor by slickensides action, is in a great measure applicable to those now under consideration; it applies to them, if possible, with greater force, since the double series of intersecting lines offers the most decisive evidence that they could not have been produced by either. In speculating as to their real origin, I will only add to the suggestion before thrown out, that when this class of phænomena shall be better understood, the specimen in the bed of the Clettwr may possibly throw some light on the obscure subject of cleavage.

A slight notice of a different kind of striæ on the Welsh rocks, arising from another cause and capable of easy explanation, will be sufficient. At the north end of Trefriw, in the vale of Llanrwst, by the side of the road to Conway, large surfaces of the true beds of Lower Silurian rocks are laid bare in several different places; they have an average dip of about 18° to the E.S.E., and form smooth and regular inclined planes. This arises from wayboards of softer shale, or from slight adhesion of the beds. On one of these inclined planes to the north of the little quay, are many nearly parallel lines or striæ, running in the direction of the valley, which are very liable to be mistaken for those produced by glaciers grating upon their bed. But on close examination I found them to be only the ends or sections of one of the series of joints which pervaded the bed and divided it nearly at right angles to its surface. The very general occurrence of joints symmetrically arranged in the Silurian rocks, makes it necessary to guard against deception arising from this cause. I will only add, under this head, a short notice of one or two patches of rocks by the side of the Holyhead road, where it overlooks the great chasm opening into Glyn Lledr, a mile east of the Waterloo Bridge, which have the appearance of being somewhat striated and polished. But the surfaces are also irregularly undulated, and the dip is to the south, or diagonal to the direction of the trough of the Conway; and there is a very steep hill immediately above them, down which, if these striæ were produced by glaciers, the icy mass must have descended, a supposition which inspection shows to be impossible. Besides, the slopes of the great opening from Glyn Lledr are very favourable for the formation and preservation of lateral moraines, of which I could observe no traces. And it may be remarked generally, that as both lateral and terminal moraines are more frequently formed on the outskirts of glaciers than striæ on the rocks beneath, and are from their situation and the nature of the material, more likely to be preserved as monuments to after ages, their occurrence in North Wales, either accompanied by striated rocks, or alone in other parts of the country, would have added to the probability of their common origin; while their entire absence from localities so favourable for their production, makes it necessary to exercise the greater caution, in referring to glacial action the other phænomena described in this paper.

I met with but one example of rock with a rounded or domed surface, to which Saussure has given the name of "*roches moutonnées*," and this was in a locality where it seemed so much more natural to refer it to a cause still in operation, that it was not necessary to call in the aid of glaciers. The spot is well known to tourists as one of the wildest and most alpine in North Wales, where the torrents from Llyn Ogwen and Llyn Idwal, on issuing from their respective lakes, are precipitated over the rocky barrier that forms the head of Nant Francon. Standing on the bridge between these two torrents, the rocks on both sides, that form, as it were, the crest of this barrier, and obstruct the free passage of the water, seem to have a smooth or rounded form that is evidently not natural. That in the stream of the Ogwen is the largest and the most regularly domed; and the side next the bridge having been recently cut away in the direction of the cleavage, their planes form a perpendicular face, on which the true lines of stratification may be traced with sufficient distinctness. The dip is towards the S.E. at an angle of about 40°, thus proving the rounded top to have no connexion with either of the divisional planes just named, or with the intersecting joints, which, as usual, have flat surfaces. Though rounded, the surface is not polished, but has a dull and weathered appearance, the effect of abrasion rather than of friction, on the hard hornstone porphyry, of which the rock is composed.* These rounded bosses of rock are at present some feet higher than the level of the respective torrents that wash their bases; but it is not difficult to believe that a period has existed when they were covered by the torrent, and that in the lapse of ages, the stones and ice it has carried along with it, having given them this rounded form, have continually deepened the channel, till the domed surfaces have been left projecting in their present situation.

Such are the principal appearances I have observed in North Wales, which approximate to the effects produced by existing glaciers upon the rocks and debris by which they are surrounded. The phænomena of moraines interested me much in Switzerland; and the recollections of their singular features would, I think, have enabled me to detect any traces of similar remains in the mountainous district under consideration. I would not, however, be understood as meaning to deny their existence in it altogether; for there are still so many obscure valleys that I have not visited, at least since the discovery of the remains of glaciers in Britain, that it would be presumptuous to speak dogmatically upon so obscure a subject. My object has been to confine myself to what I have seen during the late summer, to describe appearances with accuracy, and refer them to their real causes, independently of any theory. Believing that glaciers once existed in Scotland and in the north of England, I thought it probable traces of them might be found in Wales also; and I have shown that not a few appearances there may, on a hasty survey, be referred to such an origin. But believing, after deliberate examination, that these have either been produced by other adequate causes, or *could not* have been due to glacier action, I have felt myself bound honestly to state the conclusions I have arrived at, being satisfied from some experience, that to allow the observation or the judgement to be warped by preconceived theory, however plausible, or to decide on partial insufficient evidence, must be ultimately injurious to the cause of truth.

---

* Several varieties of hornstone porphyry are seen near this spot; and on the ascent to Cwm Idwal a thick perpendicular vein of hornstone schist is exposed, and is thrown by a dislocation on the opposite side of the road west of the bridge. It is extracted for whetstones.

Manchester, Oct. 18, 1841.

LXXI. *Calculation of Logarithms by means of Algebraic Fractions.* *By the Rev.* R. MURPHY, *M.A.*[*]

THE logarithms here used are Napierian, which may be readily converted into logarithms of any other system, simply multiplying them by the modulus.

Take the coefficients of a binomial raised to any positive and integer power n, and the same number of the coefficients of a binomial raised to the next superior negative power, $-(n+1)$; multiply both sets, term by term, multiply the products by the successive powers of a number as $1, t, t^2, \ldots t^n$, and connecting all the terms thus formed by the sign $+$, this sum is the denominator of the fraction.

Multiply the last term but one of this denominator by $\frac{2}{n}$, the last but two by $\frac{2}{n}+\frac{2}{n-1}$, the last but three by $\frac{2}{n}+\frac{2}{n-1}+\frac{2}{n-2}$, and so on; connect the products with the sign $+$, the sum thus obtained is the numerator of the fraction.

The fraction thus formed is to be added to $\log t$ in order to obtain $\log(t+1)$.

The sum is always deficient.

Call the above denominator P_n and put $T = 4t(t+1)$, the deficiency is greater than $\frac{1 . 2 . 3 \dots n}{1 . 3 . 5 \dots (2n+1)} \cdot \frac{2}{P_n} \cdot \frac{1}{(T+1)^{\frac{n+1}{2}}}$, and less than $\frac{1 . 2 . 3 \dots n}{1 . 3 . 5 \dots (2n+1)} \cdot \frac{2}{P_n} \cdot \frac{1}{T^{\frac{n+1}{2}}}$.

---

[*] Communicated by the Author.

The number n is arbitrary, but had better be taken large when the number t is small.

If we take $n = 10$ when $t = 1$, we find by this rule log 2 $= \frac{5612726\frac{43}{60}}{8097453}$, which converted to a decimal is necessarily accurate to the 15th place inclusive, which is known by the above limits.

By the application of this and similar methods, a mere arithmetician will be able to calculate logarithms, accurate to any extent, and to their last decimal figure.

Robert Murphy.

P.S. In my paper contained in the last Number, p. 369, for "a is a primitive radix," read a is the next superior primitive radix. The following numbers are verified instances of the general formula in that paper, viz. $(3n^2+2)$5, 29, 149; $(5n^2+3)$ 23, 83; $(15n^2+13)$, 73; $(20n^2+17)$ 37; $(21n^2+19)$, 103, and several others, and I have not met with an exceptional case.

LXXII. *On the Action of Metallic Copper on Solutions of certain Metals, particularly with reference to the detection of Arsenic.* *By* H. Reinsch[*].

M. REINSCH was induced to enter into the examination of the action of copper on certain metallic solutions, and its application as a test for arsenic, from the circumstance of having obtained a precipitate of metallic arsenic upon a piece of copper immersed in commercial hydrochloric acid.

This action of metallic copper he considered the more important, as by that means he hoped to be able to devise a simpler method of detecting arsenic, and also of separating it from solutions, which might be less liable to objection than that by Marsh's apparatus, the uncertainty of which has been lately pointed out by the experiments of MM. Danger and Flandin.

The process is founded upon the same principle as that adopted by M. Fuchs for the analysis of crude iron and iron ores*.

---

[*] The above is an abstract from a paper in Erdmann's *Journal für Praktische Chemie*, No. 19, 1841.

A slip of copper immersed into hydrochloric acid, sp. gr. 1·172, containing arsenic, was not acted upon, in a closed vessel, after remaining twelve hours at the ordinary temperature; if the acid was diluted with an equal quantity of water, action took place after a few hours, the arsenic being precipitated upon the copper; if the solution was exposed to the air the action occurred in a still shorter time, but if heated it took place nearly immediately, whether the acid was concentrated or diluted. The copper was covered at first with a grayish brilliant metallic coating, which upon increase of temperature, and according to the quantity of arsenic present in the solution, turned black, and finally separated in black scales.

In order to ascertain the delicacy of this reaction, a solution of one part of arsenious acid was made in 1000 parts of pure hydrochloric acid diluted with water. A portion of this solution (to which one-third of pure hydrochloric acid was added), containing 1-100,000th part of arsenic, was acted upon as soon as it was heated; at first the precipitate had the appearance of iron, but after long boiling it became black with a metallic lustre. With a solution containing 1-200,000th part, the copper was distinctly covered with arsenic after a quarter of an hour's boiling. The limit appears to be between 1-250th and 1-300,000th parts; and is therefore considerably greater than that of any other reagents, and is not likely to be mistaken for other substances.

If arsenious acid be dissolved in water, and a slip of copper immersed, no action takes place; but if a few drops of hydrochloric acid be poured upon the copper, the arsenic is immediately precipitated. A quantitive estimate of the arsenic may be made by this method, as on boiling the solution nearly the whole of the arsenic is separated from the copper, and the quantity of arsenic may be calculated from the loss of weight sustained by the copper. The arsenic obtained may either be further tested by Marsh's apparatus, or it may be heated in an open glass tube, when brilliant crystals of arsenious acid will be obtained, which may be dissolved in a weak alkaline solution and tested by the usual reagents; or the slip of copper may be heated in a glass tube with hydrogen gas, when metallic arsenic will be sublimed.

Antimony.—Copper does not precipitate this metal with the same iron-like appearance as arsenic; the precipitate has a less metallic lustre, and is of a decided violet colour. With a solution containing 1-200,000th part of antimony, the precipitation is so thin that the copper shines through, but has still a violet hue: upon making both precipitates they are easily discriminated by comparison. The delicacy of the reaction is equal to that of arsenic. Antimony also gives no precipitate without the presence of hydrochloric acid, but the addition of a few drops immediately occasions one.

---

* See Phil. Mag. for February 1841.

Tin.—Copper does not occasion a precipitate in a solution of this metal containing 1-100th part to which an equal quantity of hydrochloric acid has been added, if kept from the air; on boiling the solution there are slight indications of a metallic precipitate. If exposed to the air in an open vessel, the metal is precipitated after a few days as a grayish black powder: in weaker solutions no precipitate whatever occurs.

Lead.—With a solution containing 1-500th part of lead, to which an equal quantity of hydrochloric acid was added, copper produced no precipitate, when kept free from the air; on boiling only a few spots appeared. In dilute solutions there was no action; on exposure to the air a black powder was precipitated.

Bismuth.—With a similar solution of bismuth the slip of copper was immediately covered with a grayish metallic coating, when kept free from the air; this precipitate was gradually converted into small flat crystals; on heating the solution, the copper was immediately covered with a crystalline deposit. The metal was also precipitated from very weak solutions.

Mercury.—A solution containing 1-1000th part of mercury mixed with hydrochloric acid, was immediately precipitated upon the copper as a white coating. A solution containing 1-50,000th part of corrosive sublimate without the addition of hydrochloric acid, had no action on the copper when cold; on heating it produced a golden colour; on addition of hydrochloric acid and after boiling, the copper had a gray appearance, and metallic mercury might be observed by means of a single microscope.

Silver.—A solution containing 1-1000th part of silver, to which hydrochloric acid was added, produced a white coating upon a slip of copper; after twelve hours there were small dendritical crystals formed, which were increased upon heating the solution. A solution containing 1-50,000th part of silver without hydrochloric acid, gave the copper a yellowish tinge, which disappeared upon adding hydrochloric acid, but no silver was separated with a solution containing 1-20,000th part silver; the metal was precipitated in metallic spots.

It follows from this examination:—

1st. That metallic copper is the most delicate and surest reagent for the detection of arsenic, the reaction taking place with a solution containing 1-200,000th part.

2nd. That with antimony the action is similar to arsenic, but it is not precipitated with such a metallic lustre, and differs from it by the decided violet colour produced.

3rd. That lead and tin are not precipitated either from a concentrated or dilute solution in the metallic state; this only occurs upon exposure to the air.

4th. The solution of bismuth is immediately precipitated in a crystalline state.

5th. That silver and mercury are precipitated with a silver lustre, and the reaction does not exceed a 1-20,000th part in solution.

LXXIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xviii. p. 241.]

June 17, 1841. THE following papers were read, viz.—

1. "Experiments on the electric conditions of the Rocks and Metalliferous Veins (Lodes) of Longclose and Rosewall Hill Mines in Cornwall." By William Jory Henwood, Esq., F.R.S., &c., Secretary of the Royal Geological Society of Cornwall.

The experiments, of which the results are given in this paper, were undertaken with the view of determining whether it was in consequence of the imperfections of the galvanometers, or other apparatus, employed, that Mr. R. W. Fox, and other experimenters, had been unable to detect the presence of electricity in the tin veins of Cornwall. The mode of experimenting was in principle the same as that pursued by Mr. Fox, namely, that of placing plates of metal in contact with the points to be examined, carrying wires from the one to the other, and interposing a galvanometer in the circuit. The plates employed were of sheet-copper and sheet-zinc, and they were about six inches long, and three inches and a half wide. The wires were of copper, one twentieth of an inch in diameter, and the same that had been used by Mr. Fox.

The tabular results of these experiments show that both the granite and the tin vein at *Rosewall Hill* mine, and also the greenstone and the copper vein in that of Longclose, present unequivocal traces of electric currents, whether different parts of the same veins or various portions of the same rocks were examined.

It also appears, from these experiments, that the nature and positions of the small metallic plates employed materially affect, not only the intensity, but in some cases also the directions of the currents; and also that there is a considerable difference in the results when the same plates of metal are placed on different ingredients in the veins, even though these may be in immediate contact with each other,

2. "Researches in the Theory of Machines." By the Rev. H. Moseley, M.A., F.R.S., Professor of Natural Philosophy and Astronomy in King's College, London.

Of the various names, such as "useful effect," "dynamical effect," "efficiency," "work done," "labouring force," "work," which have been given to that operation of force in machinery which consists in the union of a continued pressure with a continued motion, the author gives the preference to the term *work*, as being that which conveys, under its most intelligible form, this idea of the operation of force, and as being the literal translation of the word "travail," which among French writers on mechanics has taken the place of every other.

The single unit, in terms of which this operation of force is with us measured, viz. the work of overcoming a pressure of one pound through one foot, he considers to be distinguished sufficiently, and expressed concisely enough, by the term *unit of work*, rejecting as unnecessary, and as less likely to pass into general use, the terms "dynamical unit," and "dynam," which it has been proposed to apply to it.

Having thus defined the terms *work* and *unit of work*, and paid a tribute of respect to the valuable labours of M. Poncelet in the theory of machines, and expressed admiration of the skill with which he has applied to it the well-known principle of *vis viva* under a new and more general form, the author proceeds to remark, that the interpretation which M. Poncelet has given to that function of the velocity of a moving body which is taken as the measure of its vis viva, associates with it the definitive idea of a force opposed to all change in the state of the body's rest or motion, and known as its "*vis inertiæ*," "*vis insita*," &c. The author conceives that the introduction of the definitive idea of such a force into questions of elementary and practical mechanics is liable to many and grave objections; and he proposes a new interpretation of it, viz. "that one half of this function represents the number of units of work accumulated in the moving body, and which it is capable of reproducing upon any resistance opposed to its progress." This interpretation he establishes by mechanical considerations of an elementary kind. Taking, then, this new interpretation of the function representing one half the vis viva, and dividing the parts of a machine into those which receive the operation of the moving power (the moving points) and those which apply it (the working points), he presents the principle of vis viva in its application to machines under the following form:—"The number of units of work done by the moving power upon the moving points of the machine is equal to the number yielded at the working points, *plus* the number expended upon the prejudicial resistances, *plus* the number accumulated in the various parts of the machine which are in motion." So that the whole number of units of work done by the moving power, or upon the moving points, is expended, partly in that work done at the working points, whence results immediately the useful product of the machine, and partly upon the prejudicial resistances of friction, &c. opposed to the motion of the machine in its transmission from the moving to the working points; and all the rest is accumulated or treasured up in the moving parts of the machine, and is reproducible whenever the work of the moving power from exceeding shall fall short of that which must be expended upon the useful and the prejudicial resistances to carry on the machine.

He then proceeds to observe, that in every machine there thus exists a direct relation between these four elements,—the work done upon the moving points, that expended at the working points, that expended on the prejudicial resistances, and that accumulated in the moving elements. This relation, which is always the same for the same machine, and different for different machines, he proposes to call, in respect to each particular machine, its *modulus*; and he states the principal object of this paper (and of another which he proposes subsequently to submit to the Society) to be, first, the general determination of the modulus of a simple machine; secondly, that of a compound machine, from a knowledge of the moduli of its component elements; and, thirdly, the application of these general methods of determination to some of the principal elements of machinery, and to the machines which are in common use.

The author then states, that the velocities of the different parts, or elements of every machine are connected with one another by certain invariable relations, capable of being expressed by mathematical formulæ; so that, though these relations are different for different machines, they are the same for the same machine. Thus it becomes possible to express the velocity of any element of a machine, at any period of its motion, in terms of the corresponding velocity of any other element. Whence it results that the whole vis viva of the machine may at any time be expressed in terms of the corresponding velocity of its moving point (that is, the point where the moving power is applied to it), and made to present itself under the form $V^2 \Sigma \omega \lambda^2$, where V represents the velocity of the moving point of the machine, ω the weight of any element, and λ a factor determining the velocity of that element in terms of the velocity V of the moving point. Substituting this expression for the vis viva or accumulated work in the modulus and solving in respect to V, an expression is obtained, whence it becomes apparent that the variation of the velocity V of the moving point, produced by any given irregularity in the work done upon the moving or working points, will be less, as the factor $\Sigma \omega \lambda^2$ is greater. This factor, determinable in every machine, and upon which the uniformity of its action under given variations of the power which impels it depends, he proposes to introduce into the general discussion of the theory of machines as the *coefficient of equable motion*.

He then proceeds to investigate general methods for the determination of the modulus of a machine, deducing them from those general relations which are established by the principles of statics, between the pressures applied to the machine, in its state bordering upon motion.

That he may escape that complication of formulæ which results from the introduction of friction, by the ordinary methods, into the consideration of questions of equilibrium, the author calls to his aid a principle, first published by himself in a paper on the 'Theory of the Equilibrium of Bodies in contact,' printed in the fifth volume of the Cambridge Philosophical Transactions, viz. "that when the surfaces of two bodies are in contact under any given pressures, and are in the state bordering upon motion, on those surfaces, then the common direction of the mutual resistances of the surfaces is inclined to their normal at the point of contact at a certain angle, given in terms of the friction of the surfaces by the condition that its tangent is equal to the coefficient of friction." This angle the author has called "*the limiting angle of resistance*:" it has since been used by other writers under the designation of the "*slipping angle*."

He next proceeds to determine the modulus of a simple machine, moveable about a cylindrical axis of given dimensions, and acted upon by any number of pressures in the same plane. He applies the principle last stated to determine the general conditions of the equilibrium of these pressures, in the state bordering upon motion by the preponderance of one of them; and, solving the resulting equation in respect to that one pressure by the aid of Lagrange's theorem, he deduces immediately the modulus from this solution by principles before laid down. The modulus, thus determined, he then verifies by an independent discussion of that particular case in which three pressures only are applied to the machine, one of which has its direction through the centre of the axis.

This solution he next considers more particularly with reference to a machine moveable about a fixed axis under one moving and one working pressure (their directions being any whatever) and its own weight; which last is supposed to act through the centre of the axis. He shows that it is a general condition of the greatest economy in the working of such a machine, that the moving and working pressures should have their directions, one of them upwards, and the other downwards, and that both should therefore be applied on the same side of the axis of the machine. He moreover shows that if the direction of one of these pressures be given, there is then a certain perpendicular distance of the other from the centre of the axis, and a certain inclination of its direction to the vertical; at which perpendicular distance, and which inclination, this pressure being applied, the machine will yield a greater amount of work, by the expenditure of a given amount of power, than it will yield under any other circumstances of its application: so that this particular distance and inclination are those whence results the most economical working of the machine.

Professor Moseley then commences his application of these general principles to elementary machines with the pulley. He establishes the modulus of the pulley under any given inclination of the parts of the cord passing over it, taking into account the friction of the axis, the weight of the pulley and the rigidity of the cord, and adopting, with respect to the last element, the experiments of Coulomb. This general form of the modulus of the pulley he applies, first, to the case in which both strings are parallel, and inclined to the vertical at any angle; secondly, to the case in which they are equally inclined on either side of the vertical; thirdly, to the case in which one is horizontal and the other vertical; and, fourthly, to that in which both are horizontal. He concludes his paper by a deduction from this last case of the modulus of a system of any number of pulleys or sheaves, sustaining among them the weight of any given length of rope horizontally.

3. "On the Nervous Ganglia of the Uterus." By Robert Lee, M.D., F.R.S.

The author, in a paper which was read to the Royal Society on the 12th of December, 1839*, had described four great plexuses under the peritoneum of the gravid uterus, having an extensive connexion with the hypogastric and spermatic nerves. From their form, colour, general distribution, and resemblance to ganglionic plexuses of nerves, and from their branches actually coalescing with those of the hypogastric and spermatic nerves, he was induced to believe, on first discovering them, that they were ganglionic nervous plexuses, and that they constituted the special nervous system of the uterus. He states in the present paper, that subsequent dissections of the unimpregnated uterus, and of the gravid uterus in the third, fourth, sixth, seventh, and ninth months of pregnancy, have enabled him not only to confirm the accuracy of his former observations, but also to discover the important fact, that there are many large ganglia on the uterine nerves, and on those of the vagina and bladder, which enlarge with the coats, blood-vessels, nerves, and absorbents of the uterus during pregnancy, and which return, after parturition, to their original condition before conception took place. The author next proceeds to describe the two great ganglia situated on the sides of the neck of the uterus, in which the hypogastric and several of the sacral nerves terminate, and which he calls the *hypogastric*, or *utero-cervical ganglia*. In the unimpregnated state, they are of an irregular, triangular, or oblong shape, about half an inch in the long diameter, and always consist of grey and white matter, like other ganglia. They are covered by the trunks of the vaginal and vesical arteries and veins; and each ganglion has an artery of considerable size, which enters it near the centre and divides into branches, accompanying the nerves given off from its anterior and inferior borders. From the inner and posterior surface of each of these ganglia, nerves pass off, which anastomose with the hæmorrhoidal nerves, and ramify on the sides of the vagina, and between the vagina and rectum. From the inferior border of each hypogastric ganglion several fasciculi of nerves are given off, which pass down on the sides of the vagina, and enter some large flat ganglia, midway between the os uteri and ostium vaginæ. From these vaginal ganglia innumerable filaments of nerves, on which small flat ganglia are formed, extend to the sphincter, where they are lost in a white dense membranous expansion. From this great web of ganglia and nerves numerous branches are sent to the sides of the bladder, and enter it around the ureter. All these nerves of the vagina are accompanied with arte-

---

[* An abstract of this paper appeared in Phil. Mag. S. 3. vol. xvi. p. 590. —Edit.]

ries; and they often form complete rings of nerve around the trunks of the great veins.

The author then describes the nerves which are given off from the anterior margin of each hypogastric ganglion, some of which pass on the outside of the ureter and others on the inside, and meet in front of the ureter in a ganglion, which he calls the *middle vesical ganglion*. There are other two ganglia, he states, formed on these nerves; one between the uterus and ureter, and the other between the ureter and vagina. These he calls *the internal and external vesical ganglia*. Not only is the ureter inclosed within a great ring of nervous matter, which, he says, resembles the œsophageal ganglia in some of the Invertebrata; but the trunks of the uterine artery and vein are likewise encircled by a great collar of nervous matter, between which and the hypogastric ganglion several large and some small branches pass.

The author gives the following description of the vesical ganglia. The internal vesical ganglion, which usually has a flattened or long bulbous shape, is formed entirely upon the nerves which pass from the hypogastric ganglion, and run between the uterus and the ureter. It has an artery which passes through its centre. It first gives off a large branch to the ring of nerve or ganglion which surrounds the uterine blood-vessels; it then sends branches to the anterior part of the cervix uteri, and afterwards a great number of small filaments to the muscular coat of the bladder behind, where it is in contact with the uterus; and it then sends forwards a large branch, which terminates in the middle vesical ganglion. This ganglion sends off a great number of large nerves to the bladder. Some of these accompany the arteries, and can be seen ramifying with them upon the whole of the superior part of the organ, even to the fundus. Filaments of these nerves, scarcely visible to the naked eye, are seen in one of the preparations ramifying upon the bundles of muscular fibres, occasionally forming loops and inclosing them, or passing down between them to the strata of fibres below. Some of the smaller branches of the middle vesical ganglion do not accompany the arteries, but are distributed at once to the parts of the bladder around the ureter.

The external vesical ganglion is formed entirely upon the nerves which proceed from the hypogastric ganglion, and pass on the outside of the ureter. This is a small thin ganglion, the branches of which are sent immediately into the muscular coat of the bladder. It usually sends down a long branch to anastomose with the nerves issuing from one of the vaginal ganglia.

From the inner surface of each hypogastric ganglion numerous small white, soft, nerves pass to the uterus, some of which ramify upon the muscular coat about the cervix, and others spread out under the peritoneum to coalesce with the great ganglia and plexuses situated on the posterior and anterior surfaces of that organ. Large branches also go off from the inner surface of the ganglion to the nerves surrounding the blood-vessels of the uterus, which they accompany in all their ramifications throughout its muscular coat.

This paper is illustrated by two drawings, in which the hypogastric, vaginal, vesical and uterine ganglia are delineated in the fourth month of pregnancy, and also the plexuses of nerves on the anterior surface of the uterus.

From an examination with the microscope of portions of the plexuses under the peritoneum of a gravid uterus in the ninth month, which had long been immersed in rectified spirit, Professor Owen and Mr. Kiernan inferred that they were not nervous plexuses, but bands of elastic tissue, gelatinous tissue, or cellular membrane.

The author concludes his paper with a letter from John Dalrymple, Esq., containing the results of the observations he had made with the microscope on the uterine nerves in the recent state. Filaments of the nerves which surrounded the ureter, and which were situated upon the body of the uterus, were submitted to the microscope. The instrument employed was a very powerful object-glass, whose focus was the eighth of an inch, made by Ross. Mr. Dalrymple found that it was impossible, even with the most careful dissection, to detach any filament of nerve without including a quantity of cellular and elastic tissue; so that although the tubular portion indicating the nerve was distinct, yet it was surrounded by innumerable extremely minute threads coiled and contorted, such as those which constitute the component of elastic tissue, and the ultimate element of cellular membrane. Under slight pressure, however, the tube was plainly discernible, and was found to contain granular matter, not uniformly distributed, but collected in minute masses at intervals. Small blood-vessels were also here and there seen, with blood-discs within them, which served to indicate the difference between the nervous and vascular tubes, and thus to avoid the possibility of error. Being, however, aware that some of the most distinguished foreign microscopical anatomists had differed as to what was the real characteristic of the nerves of the sympathetic system, and feeling, from this discordance of opinion, that there was no absolute test, or at least none which was not open to cavil, Mr. Dalrymple thought of making a comparison of the uterine nerves with those that undeniably belonged to the ganglionic system. He therefore traced some nerves on the surface of the stomach up to the great ganglion that gave them origin; and he selected some also from the small intestine. These he submitted to the same microscopical power, and under the same circumstances of light, and pressure, and medium. In all of these he observed the tubular part filled with granular matter, and similarly collected in minute masses. He also observed that each tube was surrounded by the minute serpentine threads before described. In fact, so closely did they agree in every particular with the appearances presented by the uterine nerves, that it would have been impossible to distinguish the one from the other.

GEOLOGICAL SOCIETY.

[Continued from p. 402.]

March 24, 1841.—The reading of the paper on Russia, by Mr. Murchison and M. E. de Verneuil, was resumed and concluded.

The Memoir, of which the following is an abstract, is the result of a journey through the Northern and Central Governments of Russia in Europe, made during the summer of 1840, a verbal account of some of the chief points of which, accompanied by a new geological map of those regions, was offered to the public at the meeting of the British Association for the Advancement of Science, in September 1840.

Introduction.—The authors preface their memoir with a sketch of the condition of geological knowledge concerning the flat and central countries of Russia in Europe anterior to their visit, and show that the early efforts of Strangways* had not been followed up by any connected attempt to establish the classification and succession of the older sedimentary deposits on the true principles of the order of their superposition, and their distinctions by organic remains. They point out, however, that certain elements of the subject had been prepared; first, by the map and descriptions of Strangways; secondly, by the publication of the palæontological works of Fischer de Waldheim, Pander, and Eichwald; thirdly, by the recent researches of Colonel Helmersen in the Waldai Hills; and fourthly, by the important zoological distinctions indicated by M. Leopold de Buch, who, on hearing of the plan of the voyage of the authors, expressed his belief (from the examination of certain fossils alone) that the triple subdivision of the *palæozoic* rocks into the Carboniferous, Old Red, and Silurian systems, as indicated by Mr. Murchison†, would be found to prevail in Esthonia, Livonia and Courland.

After alluding to the vast importance to the Russian empire of a correct knowledge of the subsoil of these flat regions, the authors explained the scheme which they had devised, before they left their own countries, for ascertaining the data required. Aware of the two great difficulties which are opposed to the examination of this region,—the slight altitude of the masses above the sea, and the vast quantity of drift or the slight superficial detritus which obscures the fundamental rocks,—they overcame these obstacles by examining, in succession, the banks of the rivers between the longitude of St. Petersburgh and of Archangel, which, flowing from N.N.W. to S.S.E., or transverse to the only apparent lines of elevation, might be expected to offer the evidences required. They also ascended the great Dwina, from the White Sea to Oustiug Veliki; and afterwards extended their researches to the south of Nijnii Novogorod, in order to determine the relations of the secondary rocks to those older deposits with which they had become familiar.

In terminating these introductory explanations, the authors dwelt with pleasure on the valuable assistance they had received, particularly in the early part of their tour, from the Baron A. de Meyendorf‡, now executing, by order of his Imperial Majesty, a statistical survey of Russia, who endeavoured to combine geology and natural history with the chief object of his expedition by attaching to it two excellent naturalists, Count Keyserling and Professor Blasius. They further testified their warm thanks to the Russian minister, the Count de Cancrine, who specially aided this geological inquiry, and they also acknowledged their obligations to Count Nesselrode, Count Alexander Strogonoff, Baron Humboldt, Baron Brunnow, and General Tcheffkine. They further expressed their sense of the value of the services of a zealous young geologist, Lieutenant Koksherof, without whose aid the authors could not have accomplished their task. A geological map and sections illustrated the description, and the characteristic fossils of each group were laid upon the table.

---

* Geol. Trans., First Series, vol. v.; Second Series, vol. i.

† Silurian System and map.

‡ Assisted by M. Zenofief.

Crystalline Rocks, Metamorphic Rocks, Trap Rocks, Physical Geography, &c.—Before they proceed to describe the sedimentary deposits in their order from S. to N., or from the older to the younger strata, the authors mention some peculiar varieties of gneiss which occupy the little islands of the White Sea near Onega, one of which is charged with garnets. They then give a brief sketch of the altered condition of the sedimentary strata on the western shore of the lake Onega, where they are pierced by masses of greenstone and trappean conglomerate.

A few words explain how the Waldai Hills, the great watershed of Central Russia, afford the best means of reading off the succession of the older strata. The rivers Msta, Wolkoff, Siass, &c., which flow from the south to the north, having short courses, necessarily occupy deeper rents, and therefore expose on their banks better sections than those streams, which, descending on the other side of the crest, glide along on a very slightly inclined plane to the south. By examining the banks of the north-flowing rivers, the older formations were found to succeed each other in the following *ascending order* :—

1. *Silurian Rocks.*—The oldest sedimentary deposits of Russia (those on which St. Petersburgh is situated) are clays, sandstone, limestone and flagstone, which from their position and organic remains are considered the equivalents of the Silurian system of the British Isles. The detailed order of these beds was long ago given by Strangways; but at the early day when he wrote, the study of organic remains was not sufficiently advanced to enable him to determine the exact place of these beds in the geological series, nor to point out their true relations to the adjacent masses. Many of the fossils have since been described by the native authors, Pander and Eichwald, and recently some very characteristic forms by M. de Buch.

The Silurian deposits consist in ascending order of *blue clay, intermediate grit,* and *overlying limestone*, &c. In the first of these no organic remains have yet been found; and the intermediate sandstone or grit is alone distinguished by a remarkable form unknown in western Europe (*the Ungulite*), which the authors consider to be nearly allied to *Orthis*. They likewise discovered in this grit one small shell resembling a *Pecten*. In the limestones, and certain overlying flagstones first described on this occasion, organic remains abound; and they agree well in the leading characters on which the Silurian system was established, viz. that the forms of *Trilobite*, *Orthoceratite*, and *Orthis* are distinct from the types of the overlying members of the palæozoic series.

The most prevalent fossils are the *Orthoceratites vaginatus*, *Asaphus expansus*, *Illænus crassicauda*, the peculiar Crinoidean *Spheronites* (allied to the *Ischadites* of the Upper Silurian rocks), and a vast profusion of many species of *Orthis*. Although, upon the whole, the Silurian fossils of Russia differ more than those of Sweden from British species of the same age (as might indeed be expected from their more remote distance), certain shells are identical with those published from England; among which are enumerated, *Leptæna depressa* (*L. rugosa*, Dalm.), *Leptæna sericea*, *Lingula Lewisii*, *Orthis canalis* (*O. elegantula*, Dalm.), &c.; and according to M. Eichwald, two or three species of Trilobites*.

With the exception of some very trivial dislocations in the low hills south of St. Petersburgh, the Silurian rocks are so uniformly horizontal, that in the fine quarries on the banks of the Wolkoff, the authors were able to prove a difference of 2° or 3° to the S.S.E. only by pouring water on the surface of the rocks.

These Silurian deposits occupy the islands of Öland, Gothland, &c. in the Baltic, and trend along the shores of Esthland in a broad band from W.S.W. to E.N.E., till they are lost under vast heaps of granitic detritus between the lakes Ladoga and Onega. Near the latter, these deposits are deflected to the north, and there meet with great ridges of trappean rocks, which run from N.N.W. to S.S.E. In that region all the deposits are in a metamorphic condition; the limestones present no distinct traces of fossils; and the authors having satisfied themselves that there was no chance of observing any further evidence of a descending order between such rocks and the great primarized granitic chain of Scandinavia and Russian Lapland, the boundary of which they coasted, confined their attention to the *ascending* order of the strata, which is clearly exhibited on the banks of the Wolkoff and at other places.

2. *Old Red, or Devonian System.*—That the inferior strata are the true equivalents of the Silurian system, was determined not only by their aspect and fossil contents, but by their being overlaid by other rocks which are completely identical with the "Old Red System" of the British Isles, as defined by Mr. Murchison†. This system is of great extent in Russia. It passes from Livonia by the lakes of Ilmen and the Waldai Hills, and is extended over a vast region to the N.E., where it constitutes a large portion of the shores of the White Sea. This system consists of flagstone, clays, marls, cornstones and sandstones, the whole bearing a considerable resemblance to some red deposits of the same age in our isles, but differing by containing copious *salt springs* and much *gypsum*. It was the occurrence of so much salt and gypsum that led previous writers to consider these deposits an equivalent of our new red system, which,

---

* See Professor Eichwald's work, published since the authors' visit to Russia, entitled 'Silurische-Schichten-system in Esthland.'

† See Silurian Researches, p. 165, and Table with the Map.

being found to contain the same minerals in the western parts of Europe, had been even termed by some, the saliferous system. That the red deposits (red and green) are, however, the true equivalents of our old red sandstone, is demonstrated, not only by order of superposition, but also by the many organic remains which they offer. Fishes are the most distinguishing fossils of this great Russian system, and among these are species (notably the *Holoptychius Nobilissimus*, Murchison, with the *Coccosteus*, *Diplopterus* and *Ctenoptychius* of Agassiz), forms which occur in deposits of the same age in Scotland. The fishes are in abundance, and a work, illustrative of them, is now preparing by Professor Asmus, of Dörpat, near which University they abound. The authors have traced these fish-beds for a great distance, occupying several stages in the system, and each stage characterized by peculiar species of ichthyolites.

The zoological contents of this system are also of great value in illustrating and confirming the palæozoic classification proposed by Messrs. Sedgwick, Murchison and Lonsdale; or in other words, the evidences found in Russia leave no doubt that the old red and Devonian systems of rocks are identical. The *Orthis subfusiformis*, *O. striata*, *Spirifer calcarata*, *S. trapezoidalis*, *Productus caperatus*, *Terebratula prisca* (large var.), and *Serpula omphaloides*, shells distinct from those of the carboniferous system, but similar to those which occur in Devonshire, Westphalia, Belgium, and other places (in deposits which have been shown by these authors to be of the age of the old red sandstone), are found in Russia *in the same beds with the fossil fishes of the old red sandstone* of the British Isles.

Still more striking, observe the authors, are these cumulative proofs, when it is stated, that although in France and Germany there are scarcely any lithological equivalents for the British old red system, yet, that in extending researches far to the east, this member of the series is found to resume very many of the same mineral characters which distinguish it in the central and northern parts of the British Isles; and then under *similar conditions* it contains the ichthyolites of the British deposits.

3. *Carboniferous System*.—In the northern regions of Russia, the lower or calcareous part only of the carboniferous system exists, which in the Waldai Hills, near Wytegra, on the Onega, and in many other places, is seen to overlie the old red sandstone. The inferior beds consist of incoherent sandstones and bituminous shale, which sometimes contain thin beds of *impure pyritous coal*, and impressions of several plants well known in the carboniferous system of our own islands. These are surmounted by various bands of limestone, the lowest of which only have occasionally some mineralogical resemblance to the mountain limestone of Western Europe; other beds being lithologically undistinguishable from the magnesian limestone of England; some from a pisolite; a third and very prevalent band of considerable thickness is milk-white, and not more compact than the calcaire grossier of Paris. This white Productus limestone was traced by the authors from the neighbourhood of Moscow to beyond Archangel (and they ascertained that it ranged

far into the country of the Samoiedes), a distance of not less than 1000 miles. This formation has also a mineral resemblance to chalk, in being loaded with thin bands of flints, sometimes concretionary, in which shells and corals occur. Associated with this formation, on the banks of the Dwina, about 200 wersts above Archangel, and south of Süsskaia, are splendid bedded masses of white gypsum, which, for many miles, present at a little distance all the appearance of white limestone*. With these grand gypseous deposits, in which are occasionally large concretions, two or three thin bands of limestone alternate, in one of which the authors detected fossil shells (*Avicula*) which are new to them. Other peculiar bands near Ust-Vaga, which are rather higher in the series, contain a *Productus* approaching to *P. scabriculus*, with *Pectens* and *Corals*.

The carboniferous limestone of Russia is highly fossiliferous, and from the normal and unaltered condition of most of the beds, the fossils are generally in an excellent state of preservation. Among them are many well-known British species, the lower beds being distinguished by the large *Productus hemisphericus* so well known in the same lower beds of England and Scotland; and the white beds being loaded with many of the species published by Fischer, Phillips and Sowerby, such as *Productus Martini*, *P. punctatus*, *Sanguinolaria sulcata*, *Spirifer Mosquensis*, *Cardium alæforme*, *Cidaris vetustus*, together with the abundant and characteristic Russian coral, *Chætites radians* (found, according to Mr. Lonsdale, in the carboniferous limestone of Bristol, &c.), and the *Lithostrition floriformis*, one of the most characteristic fossils of the English carboniferous limestone, &c.

Owing to its mineral aspect, the age of this rock had, till within the last year, been misunderstood; but Colonel Helmersen having observed its position in the Waldai Hills and its association with certain beds of coal, and having ascertained the nature of the fossils through the examination of M. von Buch, he first gave out in Russia, that it must be considered the true mountain limestone. The authors have completely confirmed this view, by ascending and descending sections, and have largely extended it.

Newer Red Formations.—The manner by which the authors were led to believe in the existence of newer red deposits, forming a vast basin in the governments of Vologda, Nijnii, Kostroma, is explained at some length, by describing the ascending section of the Dwina, and by details relating to the structure of the banks of the rivers Volga, Okka, &c. They show that, although this great red series of the central government agrees with that of the north, in containing salt and gypsum, yet that it differs from the "old red" group in the lithological and zoological character of its marls, limestones, and fine conglomerates, none of the fishes or organic remains before alluded to being anywhere discoverable. In expressing their suspicion that this newer red system may be found eventually to contain the *equivalents* of the upper coal measures, lower new red sandstone

---

* See M. Roberts's account of these white cliffs, which he supposed to be limestone.—*Bulletin de la Soc. Géol. de France*, 1840.

(*rohte-todte liegende*), magnesian conglomerate, zechstein, and the Trias of German geologists, the authors reserve their opinions on such details until they have accomplished a tour to the Ural Mountains, on the western flanks of which they hope to detect the evidences required; it being very difficult to trace the exact sequence in the flat and obscure regions over which they followed these deposits to so wide an extent.

Oolitic or Jurassic Series.—Certain rocks of the oolitic series have been long known to exist in the centre of Russia, and some of the fossils of this series were sent to England by Mr. Strangways.

The beds of black shale which rest at once on the great red formation along the banks of the Volga, between Kostroma and Nijnii Novogorod, belong unquestionably to the middle oolite, as they contain *Ammonites* and *Belemnites*, closely approaching, if not identical in species with those of the Oxford clay and "Kelloway Rock" of Smith. Other fossils found near Jelatma, Kacimof and Moscow exhibit close relations to the fauna of the Lias as well as to that of the middle and lower oolite. Having examined a suite of specimens from Moscow, Professor Phillips confirms the views of the authors, who are disposed to think that the middle and lower oolite, as well as the Lias, are all represented in Central Russia simply by beds of black shale with subsidiary courses of oolitic marlstone, concretions, &c. Near Moscow these shales repose directly and conformably upon the carboniferous limestone. Among the fossils of the group on the Volga and the Okka are *Ammonites flexistria*, *A. Gulielmi*, *A. Königii*, *A. sublævis*, with *Gryphæa Maccullochii?*, &c. Among the fossils from Moscow are *Ammonites* of many species, some of which are figured by Fischer, others are described by Professor Phillips, for this memoir. *Belemnites absolutus* (*B. sulcatus*, Miller); *Serpula tetragona*, Sow.; *Amphidesma? donaciforme*, Phill.; *Lima proboscidea?*, Sow.; *Pecten Fisherii*, *N.S.*, *Inoceramus dubius*, Sow.; (*P. rugosus*, Fischer) *Terebratula serrata*, Sow.; *T. acuta*, Phill. These forms characterize the lower oolite and lias of the British Isles.

Ferruginous Sand.—The shales of the oolitic series are covered by ferruginous sands, occasionally green, which contain large flattened concretions of grit (the Moscow millstones); but never having observed fossils in this rock, the authors are unwilling as yet to hazard an opinion regarding its age. With the exception of certain very recent deposits, these grits are the youngest solid strata in the northern half of Russia in Europe.

Chalk.—The cretaceous system is largely developed in the south, near Simbirsk, and in the Crimea; but on this occasion the authors did not extend their tour to the chalk districts.

Tertiary Deposits.—The white shelly limestone of Crimea, and its relations to the underlying chalk, have already been described by one of the authors*. Such deposits have not yet been discovered in any of the northern or central regions of Russia.

Post Pleiocene (*Pleistocene*).—It was formerly the general belief, that the great masses of superficial detritus, whether clays, sands or blocks, which cover so very large an area of the northern region, were all referable to one epoch (diluvian) in which the bones of great extinct quadrupeds were also imbedded. The duration of their journey was not sufficient to enable the authors to make many distinctions of age between these different masses; but they have commenced this division by the discovery of beds of clay and sand on the banks of the Dwina and Vaga, upwards of 200 miles south of the White Sea, which contain *twenty-two species* of shells, many of which still preserve their colours, and which, having been referred to Dr. Beck, of Copenhagen, have been pronounced by him to be all of modern *northern* species. Mr. Lyell states that they are identical with the Uddevalla group described by him in Sweden. Mr. Smith adds, that these shells are nearly all the same as those which he has found in various ancient elevated sea bottoms around the coasts of Scotland. In referring twenty of these to modern arctic species, Mr. G. Sowerby doubts if a certain *Mya* has ever been found recent, and states that a *Cardium*, approaching to *C. ciliatum*, is different from any northern form he is acquainted with, and near to certain Australian types. This discovery, in which they were assisted by Count Keyserling, who accompanied the authors in their tour to Archangel, is conceived to be of high geological interest, as it demonstrates that, during the *quasi* modern period, the whole of the vast flat country of north-eastern Russia was beneath the sea for a considerable time, the eastern boundary of that sea being probably the slopes of the Ural Mountains.

---

* M. E. de Verneuil.

Drift and Erratic Blocks.—Overspreading all the formations, and greatly obscuring them, is a vast mass of detritus, the large granitic and other crystalline blocks of which have excited much attention, from the days of Pallas to the present time. This detritus, the blocks of which have all been derived from the north, is shown to have been deposited *under the sea*, or in other words, upon a sea bottom, since it covers the above-mentioned shells.

Notwithstanding the obscuration occasioned by this wide-spreading drift, it is stated that the nature of the subsoil, or fundamental deposits, can often be surmised from the colour of the superficial clay and sand, and the materials of small detritus, the surface of the Silurian zone being grey, that of the old red, red; whilst the cover of the carboniferous limestone is often charged with many broken flints derived from the underlying beds of that formation, some of the siliceous fragments of which have been transported further southwards, and spread over the regions occupied by the newer red and oolitic deposits. Thus, as all the larger and harder blocks can be shown to have been carried from the mountains on the N.N.W., so in passing to the S.S.E. the finer ingredients, or matrix of the detritus, is found to change by the successive additions of materials derived from the denudation of the different members of the palæozoic series. There is no instance of any substance having been transported from S. to N., except by the modern action of streams, and by local causes dependent on the present configuration of the land. Near Nijnii Novogorod large blocks of a very peculiar trappean conglomerate were detected, which had been derived from a rock *in situ* N. of Petrazowodsk, a distance of nearly 600 miles. In endeavouring to account for the immense distances to which these blocks had been transported, the authors expressed their belief that they had been floated in former icebergs, which breaking loose from *ancient glaciers, which they suppose may have existed in Lapland and the adjacent tracts*, were dislodged upon an elevation of the northern chain, and impelled southwards *into the sea* of that period, in which the post pleiocene shells, to which allusion has been made, were accumulated. In the relation of the blocks to the sea shells, they conceive that Central Russia presents an exact parallel (though on a much grander scale) to the phænomena described by one of the authors in the central counties of England, where a similar collocation was accounted for, by supposing that the northern blocks were borne thither in vessels of ice, which in melting dropped them upon what was then a *sea bottom**.

Glacial Action.—After alluding to the works of Sefström† and Bötlingk upon the supposed "diluvial" currents of Scandinavia and Lapland, as evidenced by the parallel striæ and polishing of the surface of the hard rocks of these regions, the authors describe the most southerly of the scratches, which came under their notice near Petrazowodsk, on the lake Onega, no such markings having anywhere been observed in Central Russia. They then examine the applicability of the glacial theory, as proposed by M. Agassiz, to the tracts of Russia under review. Starting from what they conceive to be an axiom, that the advance of every modern glacier depends upon the superior altitude of the ground behind it, they show, that if certain parallel striæ, observed by M. Bötlingk, and others noted by themselves, are to be taken as proofs of the *overland* march of glaciers, such bodies must often have been propelled *from lower to higher* levels. For the proofs of this they refer to the eastern sides of the Bothnian Gulf, where M. Bötlingk found the striæ ("*diluvial schrammen*") directed in common with the boulders from N.W. to S.E.; and yet any glaciers which bore these blocks must have advanced from Scandinavia, across the Baltic Sea, and then have ascended the rocky tract in question. Again, near Petrazowodsk, in the isles of the lake Onega, the authors observed such striæ exactly parallel to the major axis of the lake, N.N.W. and S.S.E., even from a good many feet under the clear fresh water, and thence rising to the height of twenty feet above the summer level of the lake on the sloping surfaces of the rock. They then argue, that in this tract there are no hills of sufficient altitude on the N.N.W. to account for the determined forward direction to the S.S.E.; and as a still further reason for rejecting the application of the "Alpine glacial theory" to this country, they add, that as the striæ in one region have all a given and parallel direction, so must the supposed glacier not only have moved on as it were without a cause, but also have maintained an incredibly enormous advancing front of many hundred miles in length!

---

* Silurian System, p. 535 *et seq.*

[† A translation of M. Sefström's paper on this subject will be found in Part ix. of Taylor's Scientific Memoirs.—Edit.]

Without pretending to offer a complete solution of so difficult a problem, and after stating that many additional and even experimental researches are required in relation to the power of water, drift, and ice, they cannot avoid suggesting as a probable explanation of the chief phænomena in the North of Russia, that currents strongly determined in given directions by the elevation of the northern continental masses, might dislodge and set in movement icefloes and detritus, which, *grating upon the bottom of a sea, may have produced the parallel striæ*. They are the more confirmed in this hypothesis, by the fact, that the longer axes of the lakes and stony ridges of Northern Russia have generally the *same direction*; so that the supposed icebergs and *land detritus* would necessarily be borne in that direction. By adopting this view, the existence of the post pleiocene shells of the Vaga and the Dwina, and their relations to the overlying drift from the North, are in harmony; and whilst admitting so much of the glacial theory as to allow, that in former days glaciers probably advanced further to the South and occupied many insulated tracts, and to a much greater extent than at the present day, the geologist, they conceive, is alone called upon to define and limit the area of *land* in Scandinavia and Lapland, once covered with solid ice, in doing which he must of course exclude from such agency the vast countries now covered by erratic blocks, which he can demonstrate were deposited upon the *bottom of the sea*.

Angular block-ridges on lake and river Banks.—On the western shore of the great lake of Onega, the attention of the authors was directed, by Colonel Armstrong*, to three parallel ridges of large angular blocks of hard grit (old red sandstone?), which occur at heights, varying from 20 or 30 to 150 feet or more above the level of the water. As these blocks were identical in composition with the solid subjacent rock, and also quite angular, it was at once evident that they had not been drifted, but simply rent from the solid rock which forms that side of the lake. On a first inspection, the authors were disposed to think that these appearances might have been caused by upheaving or vertical shocks of earthquakes, which they presumed might be among the last signs of the great igneous action which had once been so dominant in these northern tracts; and they were unable to account for them satisfactorily, until they detected the results of modern action of river ice, which completely explained the lacustrine case.

About 80 miles above Archangel they met with a ridge of large angular blocks of white limestone piled up between the road on which they travelled and the river edge, and about 20 or 30 feet above the stream. Having ascertained that this great river was periodically subject to occasional extraordinary rises in the spring, and that on those occasions it bursts and throws up upon its banks blocks of ice to heights of 20 or 30 feet above its ordinary level, they had at once a solution of the phænomenon; for the blocks of white limestone had evidently formed parts of the subjacent strata, which, projecting into the mud and water on the edge of the Dwina, had been first entangled in ice, and rent off at their natural joints upon the expansion of the ice by which they were upheaved into their present position, taking their present irregular talus shape when the ice melted away from them. Believing, therefore, that the angular ledges on the lake of Onega were similarly formed, the authors see in them the proofs of the lakes of Northern Russia having formerly stood at much higher levels, from which the waters, they suppose, have been let off by successive elevations of the land; and they further think, that the diminution of shallow lakes, and the conversion of marshes into land within the historic period in Northern Russia, strongly corroborate the rise of this portion of the earth.

---

* Director of the Imperial Iron Foundries of Petrazowodsk.

Conclusion.—In recapitulating the chief point of the first and practical part of their Memoir, wherein they establish, they trust, on a sound basis, the general classification of the Palæozoic Rocks of Russia in Europe, the authors remark, that the fact of some of the deposits of such high antiquity being found to stretch in horizontal and almost unbroken sheets over spaces of a thousand miles in length, in a very slightly solidified or lapidified state, is the more interesting when coupled with the absence, throughout the same regions, of all plutonic or igneous rocks. This phænomenon must, it is conceived, exercise considerable influence upon geological theory, it being now apparent, that the lithological nature of the most ancient subsoil of Russia in Europe is such as to compel geologists to reject the conclusion, that in proportion to their antiquity the strata have been hardened or crystallized by any general radiation of central heat; for in these wide tracts such crystalline and hardened state is clearly seen to be purely metamorphic, and dependent exclusively on the vicinity of rocks of igneous protrusion, in receding from which to the South all the strata described are at once found in their normal soft condition.

In taking leave of the Society, the authors explained some of the chief objects of their journey to the Ural Mountains, Orenburg, &c., on which they were about to proceed.

Note.—After these sheets were sent to press, Mr. Murchison received letters from his friends and fellow-travellers, the Baron A. de Meyendorf and Count A. Keyserling, in which the researches of these gentlemen in the South of Russia are explained. These letters communicate important additions to the results already offered to the Geological Society, particularly in regard to the extension and development of the carboniferous system. The geological map which has been prepared by their labours, and from those of other Russian authorities, agrees with that of Mr. Murchison and M. de Verneuil, exhibited to the Society, in the fundamental classification of the rocks which occupy the northern and central governments of Russia, and in the lines of demarcation between the *Silurian*, *Devo-*

nian or *Old Red, Carboniferous, Newer Red,* and *Oolitic Systems*; but it is copiously enlarged, by showing the extension of the carboniferous system over a very wide area, ranging from near Witepsk, by the south of Tula and Kaluga, to the S.E. of Cazan. A vast spread of chalk and tertiary deposits directly overlies these carboniferous limestones, which rise again from beneath these younger formations in the great carbonaceous tract of the Donetz, the southern edge of which consists of the granitic steppe. A section made by Count Keyserling and Professor Blasius to the south of Kaluga, indicates a succession from what these naturalists believe to be the lower beds of the carboniferous limestone, containing *Spirifer Mosquensis*, into superior strata of sand and shale with coal, subordinate to bands of limestone containing the *Productus hemisphericus*, the coal being associated with much red earth, and overlaid by the upper carboniferous limestone. They also express their belief that the millstone grits which have been alluded to near Moscow must be considered of tertiary age, as similar beds overlie true chalk.

Mr. Murchison takes this opportunity, in the name of his friend M. de Verneuil and himself, of recording his sense of the value of the additional data which are due to the labours of Baron de Meyendorf and his associates, and trusts that after an exploration of the flanks of the Ural, and other tracts near Orenburg and in the South, all the chief facts will have been obtained for the construction of a general geological map of Russia in Europe.

Count Keyserling, who has traced the shales with *Ammonites* near Ust-Sisolsk (N. Lat. 61°, E. Long. 51°*), has indeed contributed most powerfully to these results, both by his patient observation, sound knowledge of natural history, and by his barometrical admeasurement of heights,—a point of great geological importance in those central parts of the country where the strata are not deranged. By one of his observations, it appears, that the younger pleiocene deposits on the Dwina, which he detected in company with M. de Verneuil and Mr. Murchison, are about 150 feet above the White Sea. Count Keyserling, now at St. Peterburgh, will accompany the authors in their journey to the Ural Mountains this summer.—*March* 26.

LXXIV. *Intelligence and Miscellaneous Articles.*

MR. BRAYLEY'S LECTURES ON IGNEOUS METEORS AND METEORITES.

THE following is the Syllabus of a Course of Lectures on Igneous Meteors and Meteorites which I am about to deliver at the London Institution, as announced in our last Number, p. 414, as it has been printed for distribution among the Proprietors of the Institution and other attendants on the Lectures. It contains indications of certain views on the subjects of the Course, to which I may hereafter have occasion to refer.

---

* Similar Jurassic beds had been previously observed by M. Strajeske in the N. Ural, Lat. 64° north, and their fossils are described by M. Leopold de Buch in his recent work, 'Beitrage zur bestimmung der Gebirgs-formationen in Russ-land.'

LECTURE I. Thursday, November 11, 1841.—Introduction: connexion of the subject with that of the course on Igneous Geology delivered last year; the operation of *Heat* on certain kinds of matter, whether in Meteors or in the crust of the earth, produces crystalline mineral aggregates or *rocks*. History of our knowledge respecting Meteorites or Meteoric rocks. The fall from the heavens of stones and blocks of iron recorded by the historians and naturalists of Classical Antiquity and the Middle Ages. The statements of those and subsequent writers disbelieved in more recent times; but their truth demonstrated, and the descent of such bodies from Igneous Meteors established as a fact in science early in the present century. Fall of a large stone in Yorkshire in 1795—of a shower of stones from a Meteor, near Benares, in India, in 1798—of a shower of several thousand stones at L'Aigle, in Normandy, in 1803.

LECTURE II. Nov. 18.—Phænomena of those Meteors which assume the form of Balls of Fire, and are thence called *Igneous Meteors* and *Fire-Balls*. The Meteor which passed over Great Britain in 1783 taken as an example. Figure and aspect of Igneous Meteors in general, with pictorial illustrations. Their light more intense than that from any other source except the Sun: their velocity equal to that of the planets and relatively much greater. Their height above the Earth's surface—their explosions—the luminous tracks they leave in the sky. Some throw down red-hot stones, others blocks of iron. Fall of a shower of stones from a great Meteor which appeared in North America in 1807. More recent instances in India, Russia, Germany, the British Islands, the South Sea Islands, Brazil, and Southern Africa. The most recent authenticated instance—that of a large stone which fell at Château-Renard, near Orleans, on the 12th of June, 1841. Other Phænomena attending the fall of Meteorites described and illustrated.

LECTURE III. Nov. 25.—The nature of Meteoric Stones, their mineralogical characters and constitution, and chemical composition, illustrated by specimens. The immense blocks of metallic iron, some many tons in weight, found on the surface of the earth in various countries, especially in Africa and America, all cast down by Meteors. Observed fall of such a block, in Hungary, in 1751. The peculiar nature of Meteoric Iron illustrated. Meteoric stones and iron form one class or series of bodies, collectively termed METEORITES, or Meteoric rocks. Some meteorites mineralogically identical with certain volcanic rocks: these two classes of rocks still perfectly distinct: both of igneous origin, but the production of the one attended by *meteoric* phænomena in the atmosphere or the planetary spaces, and of the other by *volcanic* phænomena within the Earth's crust. Final illustrations of the peculiar characters of Meteorites.

LECTURE IV. Dec. 2.—The problem of the origin of Meteorites discussed. Examination of some of the hypotheses by which its solution has been attempted, and in which meteorites have been supposed to originate in the Earth or its atmosphere—the volcanos of the Moon—other planets—the Sun—the planetary spaces—more distant parts of the Universe—to be themselves minute planets or satellites, fragments of planets which have been destroyed, or matter preparing for the formation of new worlds. Relations of this subject to that of the identity or similarity of Fire-Balls and the meteors called *Shooting Stars*: recent simultaneous appearance at distant parts of the globe of showers of shooting stars—their alleged periodicity. Manifest connexion between the physical and chemical characters of Meteorites and the phænomena which attend their fall; the former illustrated from the molecular constitution of the stony lavas, in the flowing state: resulting inferences as to the intimate nature of Igneous Meteors. Connexion of this branch of the subject with many important objects of inquiry in Meteorology, Astronomy, and Geology. Conclusion.

The illustration of the characters of meteorites from the molecular constitution of the stony lavas, in the flowing state, to be alluded to in the concluding lecture, arises from the application to the subject of Meteorites, of certain considerations derived from Mr. Scrope's inductions respecting the constitution of lava and the nature of its apparent fluidity. I have, for the last four years, been in the habit of explaining and supporting those inductions, in lectures on Igneous Geology. The following is the manner in which I have most recently treated this subject, as stated in the Syllabus of some lectures on Igneous Geology delivered at Clapham in June last, in sequence of others given by Dr. Mantell on those departments of geological and palæontological science to which his own discoveries and researches have so greatly contributed.

"Currents of *Lava*—their particular Nature and History—the Minerals of which they are constituted.—Stony Lavas—Graystone—Trachyte—Basalt.—Glassy Lavas—Obsidian—Pitchstone—Pearlstone—Pumice.—Alternate production of Stone and Glass from the same Materials, both in Volcanic and in Artificial Operations.—Circumstances under which this takes place.—Difference between the Molecular Constitution of the Stony Lavas and that of the Glassy, in the flowing state.—Important theoretical bearings of this branch of the subject." E. W. B.

London Institution, Nov. 8, 1841.

ACTION OF SULPHURIC ACID ON PROTOSULPHATE OF IRON.

M. Callond states, that when cold concentrated sulphuric acid is added to protosulphate of iron, the acid becomes of a red colour, which is more or less intense. When the sulphuric acid, on account of its deficient density, or any organic matter which it may contain, does not produce the effect, a drop or two of nitric acid rapidly produces it, without the application of heat.—*Journal de Chimie Médicale*, June, 1841.

BOOKS.

An Experimental Inquiry concerning the relative Power of, and useful Effect produced by, the Cornish and Boulton and Watt Pumping Engines, and Cylindrical and Waggon Head Boilers. By Thomas Wicksteed, M.I.C.E.

The Elements of Mechanics; designed for the Use of Students in the University. By James Wood, D.D., late Master of St. John's College, Cambridge, and Dean of Ely. A new Edition, revised and re-arranged, with considerable alterations and additions, by J. C. Snowball, M.A., Fellow of St. John's College, Cambridge.

Illustrations of Arts and Manufactures; being a selection from a Series of Papers read before the Society for the Encouragement of Arts, Manufactures, and Commerce. By Arthur Aikin, F.L.S., F.G.S., &c.

The Elements of Descriptive Geometry. By the Rev. Professor Hall, King's College, London. Eighty Illustrations.

Examples of the Processes of the Differential and Integral Calculus. Collected by D. F. Gregory, M.A., Fellow of Trinity College, Cambridge.

Cruveilhier's Anatomy, Vol. I. translated by Dr. Maddon; with Notes by Prof. Sharpey, Professor of Anatomy and Physiology in University College.

On the Remote Cause of Epidemic Diseases. By John Parkin.

Transactions of the Manchester Geological Society, vol. i.

METEOROLOGICAL OBSERVATIONS FOR OCT. 1841.

Chiswick.—October 1. Thick haze: cloudy. 2. Very fine. 3. Cloudy: rain. 4. Fine: rain at night. 5. Cloudy: rain. 6. Fine: cloudy: clear at night. 7. Fine: rain. 8. Overcast: slight rain. 9. Cloudy: rain. 10. Overcast: rain. 11. Very fine: rain. 12. Heavy showers. 13. Fine: stormy with rain at night. 14. Boisterous. 15. Heavy rain. 16. Rain: densely clouded: rain at night. 17. Overcast: stormy. 18. Clear and windy: rain at night. 19. Cloudy: fine. 21. Clear: frosty at night. 22, 23. Overcast: rain at night. 24. Cloudy. 25. Overcast: foggy. 26. Foggy: stormy with rain at night. 27. Boisterous with very heavy rain. 28—30. Rain. There were only four days in the month on which rain did not fall, leaving a proportion of wet days perhaps unequalled in the neighbourhood of London.

Boston.—Oct. 1. Fine: rain yesterday P.M. 2. Fine. 3, 4. Fine: rain P.M. 5. Rain: rain early A.M. 6. Cloudy: rain early A.M. 7. Fine. 8. Fine: rain P.M. 9. Fine. 10. Cloudy: rain P.M. 11. Fine. 12. Cloudy: rain early A.M. 13. Fine: rain P.M. 14. Cloudy. 15. Cloudy: rain early A.M. 16. Rain: rain early A.M. 17. Rain: rain early A.M.: stormy night. 18. Stormy. 19. Cloudy: rain early A.M. 20. Stormy: rain at night. 21, 22. Fine. 23. Rain. 24. Cloudy: rain early A.M. 25, 26. Fine. 27. Cloudy: rain P.M. 28, 29. Stormy: rain early A.M. 30. Stormy: rain early A.M.: rain P.M. 31. Cloudy.

Applegarth Manse, Dumfries-shire.—Oct. 1. Rain for an hour. 2. Fine clear sunshine. 3. Dull, cold and rainy. 4. Fair but dull. 5. Showery afternoon. 6. Wet nearly all day. 7, 8. Dropping day. 9. Fair but threatening. 10. Wet all day. 11. Wet afternoon. 12. Heavy rain A.M.: cleared up. 13, 14. Heavy rain P.M. 15—17. Showers. 18. Fair throughout. 19. Showery afternoon: frost A.M. 20. Very heavy rain. 21. Fair and frosty. 22. Frost A.M.: cloudy and moist P.M. 23. Showers. 24. Fair but cloudy. 25. Fair and clear. 26—28. Fair and bracing. 29. Fair and bracing: frost. 30. Fair A.M., but showers P.M. 31. Slight showers.

Days of Month. 1841. Oct.	Barometer.						Thermometer.								Wind.				Rain.				Dew-point.
	London: Roy. Soc. 9 a.m.	Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc.			Chiswick.		Boston. 8½ a.m.	Dumfries-shire.		London: Roy. Soc. 9 a.m.	Chiswick 1 p.m.	Bost.	Dum-fries-shire.	London: Roy. Soc. 9 a.m.	Chiswick.	Boston.	Dumfries-shire.	Lond.: Roy. Soc. 9 a.m.
		Max.	Min.		9 a.m.	8½ p.m.	Fahr. 9 a.m.	Self-register. Max.	Self-register. Min.	Max.	Min.		Max.	Min.									
1.	29·478	29·598	29·424	28·94	29·28	29·48	55·7	64·3	53·6	64	43	51	55	49	ENE.	N.	calm	SW.	·100	·20	·14	...	56
2.	29·812	29·773	29·730	29·26	29·63	29·77	53·3	61·0	49·0	63	42	52	52	35	SW.	W.	calm	SW.	·313	...	...	...	53
3.	29·832	29·792	29·745	29·30	29·83	29·75	52·5	60·8	47·7	63	51	49	53	34	N.	NW.	calm	SE.	...	.13	...	...	57
4.	29·718	29·703	29·329	29·17	29·72	29·41	54·9	58·4	51·3	61	49	55·5	62½	42	N.	NE.	calm	E.	·125	·36	·09	...	53
5.	29·160	29·124	29·888	28·65	29·12	28·82	53·2	60·5	51·7	62	43	53·5	53	46	S.	S.	calm	NE.	·272	·22	·63	...	54
6.	28·848	29·925	29·808	28·33	28·78	28·79	52·3	58·8	49·0	61	41	52	50	44	SSE.	W.	calm	ENE.	·205	·04	·09	...	53
☾ 7.	29·040	29·038	29·991	28·45	28·85	28·90	53·3	59·7	48·6	61	45	49	54	47	S.	SW.	calm	NNE.	·063	·04	...	0·82	52
8.	29·200	29·224	29·027	28·69	29·13	29·38	52·7	59·3	48·8	57	45	50	54½	47	S.	SW.	calm	NNE.	·012	·03	...	...	51
9.	29·778	29·872	29·712	29·18	29·54	29·61	50·7	58·2	48·5	57	42	51	54½	44	WSW.	W.	NW.	W.	·027	·03	·11	...	50
10.	29·878	29·835	29·612	29·35	29·51	29·20	54·3	57·2	48·4	59	50	48·5	55	47	S.	S.	calm	SE.	...	·21	...	...	49
11.	29·660	29·596	29·441	28·99	29·28	29·11	53·8	58·7	52·7	63	45	55	55	47	SSW.	SW.	calm	S.	·150	·34	·08	...	52
12.	29·254	29·596	29·202	28·70	29·15	29·38	52·7	60·0	49·3	57	42	51	53	44	S.	W.	SW.	SSE.	·352	·24	·02	...	52
13.	29·922	29·973	29·859	29·30	29·72	29·47	49·7	55·7	46·0	58	50	48	54	40½	W.	NW.	NW.	W.	·186	·04	...	...	47
● 14.	29·860	29·821	29·698	29·16	29·42	29·25	58·3	59·8	49·8	64	53	57	57	49½	S.	SW.	calm	SSW.	...	·02	·14	2·50	53
15.	29·550	29·799	29·492	28·84	29·24	29·53	56·4	63·7	54·2	60	41	55	53	47	SW.	NW.	W.	W.	·022	·50	·04	...	53
16.	29·474	29·520	29·349	28·95	29·13	29·40	51·8	60·2	46·0	58	42	45	52½	40	S.	W.	calm	NW.	·441	·04	·39	...	50
17.	29·516	29·519	29·409	28·89	29·07	28·88	54·7	58·8	47·8	63	48	47	53½	36	SW var.	W.	calm	SSW.	·033	·01	·11	...	51
18.	29·704	29·829	29·591	28·96	29·48	29·62	52·5	62·6	50·5	58	43	48·5	53	40	W.	W.	NW.	NNW.	...	·42	·03	...	49
19.	29·628	29·962	29·524	29·12	29·57	29·55	46·3	56·3	47·0	51	31	44	52	30	W.	SW.	NW.	SW.	·427	...	·30	...	46
20.	29·926	29·911	29·611	29·35	29·40	29·07	47·8	51·7	43·2	56	37	44	52½	43	S.	SW.	SW.	SW.	...	·04	...	...	44
21.	29·896	30·162	29·818	29·32	29·63	29·87	42·7	55·0	40·7	50	26	39	44½	31	W.	W.	W.	WNW.	·019	...	·12	1·60	42
☽ 22.	30·170	30·124	29·861	29·74	29·86	29·60	41·8	49·0	37·7	57	38	36	46	30½	S.	SE.	calm	ESE.	...	·01	...	...	36
23.	29·456	29·449	29·034	29·06	29·15	28·88	53·8	55·5	42·0	57	45	46	51	42	S.	S.	SE.	E.	...	·21	·01	...	45
24.	28·944	29·964	29·924	28·37	29·73	28·80	49·3	58·3	48·6	56	31	48	50	40	S var.	W.	W.	NNW.	·250	...	·06	...	47
25.	29·098	29·234	29·061	28·59	29·00	29·25	44·2	55·6	44·0	54	35	43	47	41	S.	SW.	calm	N.	...	·01	...	...	46
26.	29·352	29·460	29·318	28·96	29·45	29·63	39·8	52·8	37·2	51	42	45	46	37	W.	NE.	calm	N.	...	·11	...	...	39
27.	29·460	29·465	29·406	29·13	29·71	29·73	47·5	51·2	40·0	49	45	48	46½	36½	N.	NE.	NE.	NE.	·066	·92	...	...	45
28.	22·642	29·820	29·601	29·30	29·83	29·92	46·8	50·2	47·2	48	43	48·5	49	37	N.	NE.	NE.	NE.	·930	·10	·24	0·18	46
29.	29·848	29·818	29·766	29·45	30·00	30·03	46·7	49·6	44·7	50	45	47	49	33	N.	NE.	NE.	NE.	·069	·09	·05	...	45
○ 30.	29·784	29·776	29·721	29·45	30·02	29·85	45·7	50·5	46·0	49	45	47·5	46	37½	N.	NE.	NE.	NE.	·058	·06	·02	...	44
31.	29·838	29·877	29·798	29·36	29·70	29·71	49·3	50·5	46·0	53	44	51	50	41½	N.	SE.	NE.	NE.	·122	·19	·30	...	47
Mean.	29·572	29·695	29·572	29·0	29·417	29·407	50·5	56·9	47·0	57·10	42·64	48·5	51·7	40·6					Sum. 4·142	4·61	2·97	5·10	Mean. 49

THE
LONDON, EDINBURGH AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.
SUPPLEMENT TO VOL. XIX. THIRD SERIES.

LXXV. *On an extraordinary Magnetic Disturbance recently observed at Greenwich. By* G. B. Airy, *Esq., M.A., F.R.S., Astronomer Royal*[*].

On the 25th of September of the present year, a most extraordinary disturbance of the magnetic instruments was observed at the Magnetic Observatory attached to the Royal Observatory of Greenwich. The disturbed state of all the instruments attracted the attention of Mr. Glaisher (chief assistant in the magnetic department), at an early hour of that day, and he immediately commenced a series of observations on all the instruments, at short intervals. After a time, the disturbance became so small that the extraordinary observations were discontinued; but it again increased, and observations were again immediately made with all the instruments, and repeated as rapidly as it was possible for one observer to do so. But for the promptitude and judgement displayed by Mr. Glaisher on this occasion, the record of this disturbance, unprecedented in this magnetic latitude, would probably have been lost. The following statement will give an idea of the magnitude of the disturbance:—that within eight minutes of time, the declination needle changed its position *more than* 2¼° (having passed in both directions the range of the observing telescope, which includes that angle); that the vertical force was increased by *more than* 1-40th of its whole value, the instrument having then reached the extremity of its range; and that the horizontal force was increased about 1-30th of its whole value.

The following more detailed account is taken principally from the abstract furnished by Mr. Glaisher. It is to be remarked, that the time throughout is Göttingen mean solar time, civil reckoning: that the readings of the theodolite for the declination needle increase as the north end of the needle moves towards the east, and that the reading for the astronomical meridian is 269° 51′ 45″ nearly; that the increasing readings of the vertical-force magnetometer imply increasing vertical force, one division being equal to 0·000471 of the whole vertical force; and that the increasing readings of the horizontal-force magnetometer imply increasing horizontal force, one division being equal to 0·002214 of the whole horizontal force.

---

[*] From a paper printed for private circulation, and kindly communicated by the Author.

Early in the morning of Sept. 25, the needles were in an agitated state. During the appearance of an aurora, additional observations were taken; and the declination needle in less than three hours traversed an arc of 34′. After this the needles were in a tolerably quiet state, and extraordinary observations were discontinued.

The observation at 10^h a.m. showing a change of 17′ of arc from the previous reading, at 8^h a.m., extra observations were again resumed, and continued till 11^h a.m.; nothing remarkable appearing during this time, they were again discontinued.

At 2^h p.m., Göttingen mean time, it was evident that all the needles were affected by some unusual cause of disturbance, and incessant observations were at once commenced, the instruments being observed successively, as quickly as one observer could take them. At 8^h 53^m Mr. Hind joined, and subsequently Messrs. Dunkin and Paul. From this time to the discontinuance of the observations two persons were constantly engaged, one taking the observations with the vertical-force magnetometer, the other, those of the declination needle and of the horizontal-force magnetometer. About 2^h 40^m p.m., the motions of the needles became very peculiar; the vibrations of the declination needle and of the vertical-force magnetometer being quite destroyed; and those of the horizontal-force magnetometer being reduced to a continuous succession of jerks or starts. It will perhaps contribute to clearness if the motions of the three instruments are now separately described.

Declination Needle.—Every change in this instrument from one position to another was by sudden impulses; after each of these it was stationary, for intervals varying from 5^s to 20^s; then it was forced again to another position, and was again stationary without the slightest motion; and so on successively.

At 3^h 30^m the changes from one position to another became very decided, but the motions still partook of the same character; absolute rest and jerking motion followed alternately:

in 2^s or 3^s the needle frequently moved through many minutes of arc, and then suddenly became stationary again.

At $3^h\ 36^m\ 20^s$ p.m., a bold sweep carried the north end of the needle towards the west 56′ of arc, in one minute of time; at $3^h\ 37^m\ 20^s$ the theodolite read 245° 22′—; and for two minutes after this time the cross carried by the needle was out of range of the telescope *, being in a position, in which, if the telescope had been exactly directed to it, the theodolite would have read less than 245° 22′. At $3^h\ 40^m\ 20^s$ p.m., Göttingen mean time, the needle had moved back 25′; at $3^h\ 43^m\ 50^s$ the theodolite read 246° 19′; at $3^h\ 44^m\ 20^s$ the needle had moved so far (north end towards the east), that the theodolite reading was 247° 38′+, and the cross carried by the needle was out of range of the telescope on the opposite side.

This extraordinary magnetic stride of more than $2\frac{1}{4}$°, was traversed in about 8^m, the extremes being only separated by that interval of time.

At both extremities the needle was without swing, all momentum from such a wide sweep being quite destroyed.

After remaining at this place for more than two minutes, the needle slowly returned, and at $3^h\ 48^m\ 54^s$ the theodolite read 246° 27′; at $3^h\ 53^m\ 21^s$ it read 246° 37′. Within 5^s after this, another bold sweep carried the cross out of the range of the telescope; when found, at $3^h\ 54^m\ 40^s$, the theodolite read 245° 31′±; instantaneously it rushed back, with a violently agitated motion, across and out of the field on the other side; the circle-reading, at $3^h\ 55^m\ 0^s$, was 246° 43′±, and therefore this arc of 1° 12′± was passed over in 20^s of time.

This was the last very great excursion; the needle was incessantly watched for 9^h longer, during which time it was much agitated. The most remarkable changes are the following:—

	h	m	s		°	′
At	4	25	36	p.m., Göttingen mean time, theodolite read	246	8
		31	36	... ...		34
		49	36	... ...		8
		55	36	... ...		21
	5	46	37	... ...		10

---

* This arose from the great inclination of the axis of the collimator (carried by the magnet) to the magnetic meridian; in consequence of which, the pencil of rays passing through the object-glass of the collimator was thrown entirely on one side of the object-glass of the theodolite. In ordinary cases, the change of angle has been sufficiently slow to allow the observer to shift the suspension of the magnet, but the rapid changes on Sept. 25 did not allow the observer to leave his place. Observations with the mirror in Gauss's manner are free from this inconvenience.

	h	m	s			°	′
At	5	50	37	p.m., Göttingen mean time,	theodolite read	246	30
	6	0	30	...	...		8
		33	37	...	...		26
		46	36	...	...		32
	7	16	38	...	...		25
		31	38	...	...		46
		56	38	...	...		37
	8	53	38	...	...		28
	9	18	38	...	...		43
		38	38	...	...		51
	10	10	39	...	...		53
		41	39	...	...		39
		46	39	...	...		46
	11	45	40	...	...		30
	12	52	40	...	...		47

It is to be remembered, that increasing readings denote that the north end of the needle moves towards the east.

Vertical-Force Magnetometer.—The vertical-force magnetometer was as much affected as the declination instrument. The disturbances of its vibrations were very similar, except that it was frequently much longer in the stationary state, its times of rest varying from 5^{s} to 2^{m}; at the end of each of these it jumped on, then was again still, and so on.

This instrument had been much affected early in the morning; the extent of its excursions being between $37^{d}\cdot9$, and $44^{d}\cdot1$ on the scale. The following are scale-readings, commencing with noon:—

	h	m	s				d
At	0	0	0	(noon),	Göttingen mean time,	scale-reading	44·0
	1	47	30	p.m.	...	...	47·8
		57	30		...	...	48·1
	2	7	30		...	...	50·0
	3	15	21		...	...	55·0
		16	21		...	...	55·9
		21	26		...	...	57·9
		22	21		...	...	58·3
		22	51		...	...	59·0
		23	21		...	...	60·3
		23	51		...	...	60·3
		24	21		...	...	60·4
		26	21		...	...	63·0
		27	51		...	...	63·2
		31	21		...	...	67·0
		32	21		...	...	69·5
		32	51		...	...	71·2
		34	21		...	...	81·3
		34	51		...	...	95·0

At 3^{h} 35^{m} 21^{s}, the marked end of the needle (which is towards the east, the needle being transverse to the magnetic meridian) was resting on the eastern Y, or apparatus intended for guarding the needle, dipping as much as the frame permitted it, and held there apparently by a great power; in this situation it remained for ten minutes, till 3^{h} 45^{m}.

	h	m	s				d
At	3	45	42	p.m.,	Göttingen mean time,	scale read	73·1
		46	23		...	...	68·9
		50	31		...	...	63·4
		57	38		...	...	57·7
		58	58		...	...	55·6
	4	13	36		...	...	62·9
		29	26		...	...	66·9
	5	21	37		...	...	55·6
		44	37		...	...	60·6
		55	37		...	...	53·2
	6	19	7		...	...	68·3
	7	17	8		...	...	53·8
		35	8		...	...	50·3
	9	22	9		...	...	45·6
Sept. 25.	11	0	39		...	...	40·9
Sept. 26.	0	58	25	a.m.	...	...	33·7
	1	3	25		...	...	36·1

About 6^{h} 19^{m} p.m., the needle assumed a steadier appearance, having a motion something like vibration, but exceedingly irregular.

Horizontal-Force Magnetometer.—The horizontal-force magnetometer was not less affected than the other two instruments. The effect on its vibrations was however peculiar to itself: in the midst of its swings it was suddenly checked, and held in that place for a second or two, then hurried backwards, again suddenly stopped, and then forced back in the contrary direction; these checks were sometimes at 5^{s}, sometimes at 10^{s}, after arriving at its limit. They were most striking. From experience with this instrument we are accustomed to look for an almost imperceptible motion at the extremities of the vibrations, then to see it move slowly in the opposite direction, quickly in the centre of swing, slowly towards the end, then return, and so on; but to see it, almost in the same instant, moving rapidly and absolutely motionless, was very striking and strange to the observers.

This instrument was less affected in the early part of the day than either of the others. About noon it showed signs of being under the influence of some disturbing cause: the scale then read $55^{d}{\cdot}8$; at 1^{h} 50^{m} the reading increased to $58^{d}{\cdot}6$; at 2^{h} 0^{m} to $59^{d}{\cdot}3$; at 2^{h} 10^{m} to $60^{d}{\cdot}1$; it then oscillated between 59^{d} and 61^{d}, till $3^{h}\ 11^{m}$, the scale then reading $59^{d}{\cdot}7$. The disturbances now became as surprising as those with the other needles; a steady and rapid increase of scale-reading took place, with a strange jerking motion.

	h	m	s		d
At	3	14	48	p.m., Göttingen mean time, the scale read	61·4
		21	3	... ...	61·2
		27	26	... ...	64·2
		28	6	... ...	64·4
		28	46	... ...	64·8
		38	16	... ...	73·6
		38	56	... ...	73·2

Then there was a remarkable start to $78^{d}{\cdot}0$; and here it remained stationary from $3^{h}\ 39^{m}$ to $3^{h}\ 43^{m}$, when it started back.

	h	m	s		d
At	3	47	41	p.m., Göttingen mean time, the scale read	69·9
		56	31	... ...	65·6
	4	0	51	... ...	63·7
		8	55	... ...	63·1
		15	11	... ...	64·6
		47	41	... ...	61·4

Then the reading increased a little, and then gradually decreased, till $5^{h}\ 53^{m}\ 12^{s}$ p.m., Göttingen mean time, when it read $57^{d}{\cdot}8$; it then increased to $6^{h}\ 16^{m}\ 12^{s}$, when the scale read $62^{d}{\cdot}1$.

It then oscillated between 58^{d} and 60^{d}, until $7^{h}\ 25^{m}\ 13^{s}$, when a sharp diminution to $53^{d}{\cdot}9$ took place. The readings alternately increased and decreased for the next six hours, oscillating all the time between 53^{d} and 57^{d}.

From the preceding account it will be seen, that the magnetometers were more affected on this day than ever before, and that the great disturbance affected the three instruments at the same time.

The most abrupt and violent fluctuations of the meridian needle occurred between $3^{h}\ 36^{m}$ and $3^{h}\ 46^{m}$. At $3^{h}\ 35^{m}\ 21^{s}$ the vertical-force magnet went beyond the scale; at $3^{h}\ 45^{m}$ it again came on. The horizontal-force needle began its great deflexion before $3^{h}\ 38^{m}\ 16^{s}$; it was over at $3^{h}\ 48^{m}$; and, probably, had there been an observer at each instrument, it would have been found that the effects on all were simultaneous. Such was Mr. Glaisher's impression while observing; but (as he remarks) the startling and bewildering effect of so great changes was such, that all that he could do was to make such observations as seemed practicable, with a view of discovering the *extent* of such extraordinary disturbances.

After this great disturbance the irregularities were still very great; the horizontal-force and the vertical-force magnetometers partook of similar disturbances throughout. With a few trifling exceptions, an increase of force in one was accompanied with an increase of force in the other, and so also with respect to the decrease of force.

The declination magnet has certainly, however, one great deflexion, in which the others have no share; that which occurred between $3^h\ 54^m\ 40^s$ and $3^h\ 55^m\ 0^s$, where an arc of $1^\circ\ 12'$ was described in 20^s. Nothing similar, at this time, is shown by either of the other needles.

In addition to the observations contained in this account, ample and abundant observations were taken to obtain every variation of the vertical force, and also observations of the declination and horizontal-force magnetometer, sufficient to show every great change and nearly all the small ones, until Sept. $26^d\ 1^h$ a.m., when the observations were discontinued.

At $4^h\ 40^m$ an observation of the dip was taken; giving a result greater than ordinary.

The day (Sept. 25) was cloudy throughout: about 9^h p.m., a few bright streamers were seen through the clouds, then nothing more till 11^h p.m., when an auroral arch, about 24° high, was visible for a short time.

It is desirable that accounts of corresponding observations of this disturbance should be collected. I invite observers who may have made observations on the same day, to send to me abstracts of their observations, or to communicate them, in the way which they think best, to the public, or to the persons who are interested in magnetical phænomena.

Royal Observatory, Greenwich, 1841, Oct. 26. G. B. AIRY.

LXXVI. *Table of the successive Strengths of Pyroxylic Spirit, corresponding to its successive Specific Gravities, with some Introductory Observations. By* ANDREW URE, *M.D., F.R.S., &c.*[*]

HAVING been professionally employed by an eminent manufacturing chemist, about eighteen months ago, in experimental researches upon the above spirit, the *holzgeist* of the Germans, I found it necessary to construct the following table, in order to ascertain the commercial value of the article at various densities. The principal use of wood-spirit, as extracted by distillation from pyrolignous acid, or from liquid pyrolignite of lime, is for dissolving shell-lac and sandarac into a varnish for stiffening the bodies of hats, and rendering them impervious to water. Hats imbued with this varnish exhale in the hot apartments where the process is conducted the vapours of the wood-spirit very copiously, and thereby occasion a painful irritation to the eyes of the workmen. Some kinds of the spirit are much more injurious to the eyes and the health than others, even when they have all been rectified to apparently the same pitch of purity and strength by the same operations. One purpose of my researches was to discover the causes of these variations, which affect the comfort of the operatives, and another was to discover the causes of the variations in the solvent qualities of wood-spirit of the same strength by the hydrometer. Having hitherto but partially succeeded in the attainment of these two objects, I shall not occupy the time of the Society at present with an account of the experiments made with that view, but shall reserve them for a future communication.

---

[*] Communicated by the Chemical Society, having been read June 1, 1841.

The researches of Berzelius, Gmelin, Weidmann, Schweitzer, Kane, Liebig, Dumas, and Peligot, concur to prove that the ordinary wood-spirit of commerce, even in its most highly rectified state, is not like spirit of wine, merely an alcoholic liquor more or less diluted with water, but that it consists of different compounds mingled together, and very difficultly separable from each other. Wood-spirit, xylite, and mesite, are three of these liquid compounds usually associated in pyroxylic spirit. When the common wood naphtha of the druggist is distilled three or four times from pulverized unslaked quicklime, by the heat of a water-bath, the oily impurities and water are got rid of, and an anhydrous fluid is obtained which is not liable to become brown on exposure to light, like the ordinary wood naphtha, and which does not become turbid or milky when mixed with water. This purified spirit, however, still acts as painfully almost as the original cruder article, upon the eyes of the hatters, as I ascertained by trial. One mode of separating *wood-spirit* from xylite and mesite, is founded upon the property possessed by wood-spirit, of forming a compound with chloride of calcium not decomposable at the heat of boiling water, while similar compounds with xylite and mesite are decomposable at that temperature. I did not find that pyroxylic spirit was essentially improved as to its employment in the arts, by being rectified by distillation from its combination with chloride of calcium.

Methol is the name which has been given to the oil formed by the action of sulphuric acid upon wood-spirit, xylite, and mesite; and I believe the same oil is generated by the simple combustion of pyroxylic spirit; for I have observed that when the pyroxylic spirit, which has been treated with both quicklime and chloride of calcium, is burned in a platinum capsule till fully one-half be consumed, the residuum becomes oily and opalescent.

The spirit used for the construction of the following table was purified by distillation from pulverized quicklime, and was drawn over with the heat of a water-bath, at such a temperature that its specific gravity at 60° was 0·8136. When the specific gravity becomes 0·847 by the dissipation of the lighter spirit, the boiling point is 145° Fahr. I believe that a useful criterion of the nature of pyroxylic spirit would be obtained by comparing its boiling temperatures at different degrees of density. To this point I shall also direct my further investigations.

The temperature of the pyroxylic spirit when the specific gravities were taken, was exactly 60° Fahr.

Spec. Grav.	Spirit per cent.	Over proof of Excise scale.	Spec. Grav.	Spirit per cent.	Over proof of Excise scale.
0·8136	100·00				
0·8216	98·00	64·10	0·9032	68·50	13·1
0·8256	96·11	61·10	0·9060	67·56	11·4
0·8320	94·34	58·00	0·9070 ?	66·66	9·3
0·8384	92·22	55·50	0·9116 x	65·00	7·10
0·8418	90·90	52·50	0·9154	63·30	4·20
0·8470	89·30	49·70	0·9184	61·73	2·10
0·8514	87·72	47·40			Under proof.
0·8564	86·20	44·60	0·9218	60·24	0·6
0·8596	84·75	42·20	0·9242	58·82	2·5
0·8642	83·33	39·90	0·9266	57·73	4·0
0·8674	82·00	37·10	0·9296	56·18	7·00
0·8712	80·64	35·00	0·9344	53·70	11·00
0·8742	79·36	32·70	0·9386	51·54	15·30
0·8784	78·13	30·00	0·9414	50·00	17·80
0·8820	77·00	27·90	0·9448	47·62	20·80
0·8842	75·76	26·00	0·9484	46·00	25·10
0·8876	74·63	24·30	0·9518	43·48	28·80
0·8918	73·53	22·20	0·9540	41·66	31·90
0·8930	72·46	20·60	0·9564	40·00	34·20
0·8950	71·43	18·30	0·9584	38·46	35·60
0·8984	70·42	16·60	0·9600	37·11	38·1
0·9008	69·44	15·3	0·9620	55·71	40·6

LXXVII. *On the Ferrocyanides.* *By* [*the late*] R. CORBETT CAMPBELL. *Communicated by Dr.* CLARK*.

IT is well known that the yellow prussiate of potash, heated by itself in close vessels, is decomposed into cyanide of potassium, carburet of iron, and nitrogen gas. I have observed that, heated in contact with the air, the products of the decomposition are very different. The cyanide of potassium takes oxygen from the air, and is thereby converted into cyanate of potash, while the cyanide of iron is decomposed, converting the iron into an oxide. The absorption of oxygen is caused by the presence of the cyanide of iron, for cyanide of potassium, heated by itself in contact with the air, does not become changed into cyanate of potash. I believe that a process for cyanate of potash may be founded on the above observation, preferable to the common one with oxide of manganese; for it has been proved that this oxide is not essential in the process, and it not unfrequently happens that a large quantity of the cyanide of potassium is converted into carbonate of potash. Sometimes also a little manganese dissolves along with the cyanate.

---

* Communicated to this Journal by the Chemical Society, having been read June 1, 1841.

For the preparation of this salt, then, without the use of manganese, the powdered and dried prussiate of potash is heated almost to redness, in a flat iron vessel, with constant stirring of the melted mass. Some ammonia is evolved, which results from the action of the moisture of the air, for the substance itself contains no hydrogen. The melted mass should be taken out with an iron spatula, allowed to cool, reduced to powder, and again fused; because the melting cyanate of potash is apt to protect little bits of the yellow prussiate from the action of the air. If the heating be properly conducted, not a particle of cyanide of potassium will be formed. The cyanate of potash is dissolved out with hot alcohol filtered and crystallized. The undecomposed prussiate of potash remains undissolved.

The double salt of ferroprussiate of potash and lime acts under heat exactly like the ferroprussiate of potash itself. Well dried and set on fire, it continues to burn, until the alkaline and earthy cyanides are converted into cyanates, and the iron into oxide. The reason that the double salt continues to burn is to be found in the porous state of the mass, which offers no obstacle to the free access of air; whereas the yellow prussiate alone fuses, and prevents the progress of the combustion.

The ferrocyanide of zinc, which always contains some ferroprussiate of potash in chemical combination, likewise continues to burn, and affords cyanate of potash and the oxides of the metals.

When the double salt of potash and lime is heated in the air as above mentioned, and then dissolved in water, the solution possesses the remarkable property of becoming pink in the sun's rays, and again becoming colourless in darkness.

Neither cyanate of lime nor cyanate of potash, together or singly, or mixed with prussiate of potash, show this reaction. It is hence probable that this solution owes the above-mentioned property to the admixture of some foreign substance, present probably in a very small quantity. All attempts to isolate any such substance were fruitless.

As the shade of colour in the above solution is exactly that of a solution of permanganate of potash, the solution and the substances used were tested for manganese. They contained none. The reactions of this solution too stand in contradiction with some of those of the permanganates. The presence of ferrocyanide of potassium is essential to the production of the pink in the sun's rays, but the action of the same salt on permanganate of potash is first to reduce it to the green manganate, and by further addition of the prussiate to oxide of manganese. As the prussiate of potash of commerce often contains traces of sulphocyanide of potassium, the experiments were repeated with prussiate of potash, that had been washed with hot alcohol, and by this means all sulphocyanide removed; but no difference was observed in the result.

It is further clear, that this salt, even if it were present in the prussiate of potash, could take no part in the above-mentioned reaction, for it gives no precipitate with a lime salt, with or without the presence of prussiate of potash. An alkaline state of the liquid is essential to the production of the pink in the sun's rays; so is likewise the presence of a ferrocyanide. If a solution of nitrate of copper be added to a solution of the heated double salt, till all ferrocyanide be removed, and then the excess of copper precipitated by carbonate of potash, the solution will have lost entirely the property of becoming coloured in the sun's rays, but will recover it on the addition of a few drops of a solution of yellow prussiate of potash.

The air exercises no influence on these changes; they take place equally distinctly in closed vessels.

A temperature of 120° Fahr. destroys the colour, but on cooling and re-exposure to the sun's rays, the colour again appears. By evaporation in the rays of the sun, a pink salt is obtained. The presence of cyanate of potash is not essential to the production of the colour; if muriatic acid be added to the solution until all cyanic acid be destroyed or removed, and then supersaturated with alkali, the solution possesses the colouring property as strong as before. Another proof that cyanate of potash is not essential, is that the double salt of ferroprussiate of lime and potash heated in closed vessels, and dissolved in water, shows likewise the same reaction, although containing no cyanate of potash.

I have already stated, that the investigation undertaken with the view of isolating the colouring substance in these experiments, was without success; nevertheless some observations were made which seem to stand in relation to this subject.

It has often been remarked, that, on adding an acid to the solution of the heated double salt, a minute quantity of a reddish powder is precipitated. This powder contains iron; it is decolorized by carbonate of potash, but the colour is not reproduced by exposure to the sun. The solution from which this powder has been precipitated, on being saturated with alkali, and exposed to the sun's rays, is found not to have lost the colouring property. I suspect that this red powder is the same as is observed to be precipitated from some specimens of commercial prussiate of potash, on the addition of strong acetic acid. Prussiate of potash is sometimes observed in commerce of a darker tint than usual, and it is this variety which frequently shows the above-mentioned reaction.

If a solution of cyanide of ammonium be added to a solution of acetate of copper, a compound of copper and cyanogen is precipitated, not analogous in its composition to the cyanide from which it is precipitated. The supernatant solution becomes pink-coloured for a few seconds. In order to see if this reaction result from the action of the free cyanogen on the cyanide of ammonium, a stream of this gas was sent through a solution of the salt. It became first yellow, and then red, but a different red from that mentioned in the reaction with the copper salt.

Professor Liebig pointed out to me another instance of a fugitive pink colour occurring in a cyanogen compound, which Prof. Gmelin of Heidelberg has mentioned in his System of Chemistry. When strong nitric acid is added to pounded yellow prussiate of potash, and slightly heated, much cyanogen gas is evolved; when the heat is not too strongly applied, neither prussic acid nor nitrous acid are remarked. The mass becomes black, and is quite soluble in water: if there be added to this solution, first, an excess of potash, and then some drops of sulphuret of potassium, the solution becomes beautifully pink-coloured, disappearing after the lapse of a few seconds.

It differs from the pink colour observed as occurring in the solution of the heated double salt when exposed to the sun, in its action to sulphuretted hydrogen. This gas, or an alkaline sulphuret, instantly destroys the colour resulting from the action of light, while an alkaline sulphuret is essential to the production of the other. The pink colour of the solution to which sulphuret of potassium has been added, becomes soon purple, and afterwards deposits a blue substance, which long kept in the solution becomes white. This blue precipitate cannot be prussian blue, for it is produced in an alkaline solution, and is instantly decolorized by acids. It is possible that the above pink solution owes its colour to the same substance as is present in Gregory's solution of sulphuret of azote in caustic potash.

I have brought forward these different instances of coloration occurring in cyanogen compounds, because they seem to point out the existence of a yet unexamined class of prussiates.

On the Acid Cyanurate of Potash.

Mineral acids, as well as acetic acid, were found to have the power of converting the neutral cyanate of potash into the acid cyanurate. The essential circumstance in this process is that the solution of the cyanate of potash be concentrated. The process above described for making cyanate of potash by calcining in the air the yellow prussiate of potash, affords a ready means of making this salt easily, and in quantity. The roasted prussiate is digested with cold water, filtered, and muriatic acid, added to the precipitated salt, is dissolved in hot water, and crystallized by cooling.

The foregoing experiments were partly confirmed and partly originally performed in the laboratory at Giessen, in 1838.

This paper is the only scientific memorial of its amiable author, who died about two years ago, at an early age, but mature enough to have endeared him to many men of science, and to have indicated promise of scientific eminence. He will be most remembered by his friends for a marked rectitude of mind, combined with an uncommon ardour; evincing itself in the warmth of kindly feelings and in the enthusiasm of scientific pursuits.—T. C.

LXXVIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 489.]

June 17, 1841. (*Continued.*) "ON the Corpuscles of the Blood." Part III. By Martin Barry, M.D., F.R.SS. L. and E.*

After remarking that no clear conception has hitherto existed of the mode in which the floating corpuscles of the blood conduce to nou-

---

[* Abstracts of Parts I. and II. of this memoir will be found in Phil. Mag., S. 3. vol. xvii. p. 300, and vol. xviii. p. 310.—Edit.]

rishment, the author states that he has found every structure he has examined to arise out of corpuscles having the same appearance as the corpuscles of the blood. The following are the tissues which he has submitted to actual observation, and which have given the above result, namely, the cellular, the nervous, and the muscular; besides cartilage, the coats of blood-vessels, several membranes, the tables and cells of the epithelium, the pigmentum nigrum, the ciliary processes, the crystalline lens itself, and even the spermatozoon and the ovum.

The author then traces the nucleus of the blood-corpuscle into the pus-globule; showing that every stage in the transition presents a definite figure. The formation of the pus-globule out of the nucleus of the blood-corpuscle is referable to the same process, essentially, as that by means of which the germinal spot comes to fill the germinal vesicle in the ovum. This process, which, in a former memoir, he had traced in the corpuscles of the blood, he now shows to be universal, and nowhere more obvious than in the reproduction of the tables of the epithelium. The epithelium-cylinder seems to be constituted, not by coalescence of two objects previously single, as has been supposed, but by division of a previously single object. Certain objects, called by the author *primitive discs*, exhibit an inherent contractile power, both when isolated, and when forming parts of a larger object; an incipient epithelium-cylinder having been observed by him to revolve by this means. Molecular motions are sometimes discernible within corpuscles of the blood. The author has noticed young blood-corpuscles exhibiting motions, comparable to the molecular, and moving through a considerable space; and he has met with the nuclei of blood-corpuscles endowed with cilia, revolving, and performing locomotion. In his first paper on the Corpuscles of the Blood, he described certain instantaneous changes in form which he had observed in blood-corpuscles, and afterwards expressed his belief, that these changes were referable to contiguous cilia, although he had not been able to discern any such cilia. He now states that subsequent observation inclines him to think that these changes in form arise from some inherent power, distinct from the motions occasioned by cilia. The primitive disc, just mentioned, seems to correspond, in some instances, with the "cytoblast" of Schleiden. Thus the very young corpuscle of the blood is a mere disc; but the older corpuscle is a cell. The author minutely describes the mode of origin of the pigmentum nigrum; showing that it arises in a similar manner in the tail of the tadpole, and in the choroid coat of the eye. He had before described the Graafian vesicle as formed by the addition of a covering to the previously-existing ovisac: this covering, he afterwards stated, becomes the corpus luteum. He now confirms these observations, with the addition, that it is the blood-corpuscles entering into the formation of the covering of the ovisac, which give origin to the corpus luteum. The spermatozoon appears to be composed of a few coalesced discs. The fibres of the crystalline lens are not elongated cells, as supposed by Schwann; but coalesced cells, at first arranged in the same manner as beads in a necklace.

The author concludes with the following recapitulation:—1. The nucleus of the corpuscle of the blood admits of being traced into the pus-globule. 2. The various structures arise out of corpuscles having the same appearance, form, and size as corpuscles of the blood. 3. The corpuscles having this appearance, and giving origin to structures, are propagated by division of their nuclei. 4. The corpuscles of the blood, also, are propagated by division of their nuclei. 5. The minuteness of the young blood-corpuscles is sometimes extreme; and they are to be found in parts usually considered as not being permeable by red blood.

In a postscript, the author adds, that blood found in the heart immediately after death by bleeding, presents incessant alterations in the position of its corpuscles. Among these, when a single corpuscle is examined very attentively, it is seen to change its form; and the author is disposed to think it is this change of form that produces the alterations in position. The changes in form are slight, compared with those previously described by him as observed in blood elsewhere, and are not seen without close attention. The motions resemble those called molecular; and in the minutest corpuscles, which are mere points, nothing besides molecular motion can be discerned. It may be a question, the author thinks, whether molecular motion differs in its nature from the motion of the larger corpuscles just referred to. The division of the blood-corpuscles into corpuscles of minuter size, though apparent in blood from either side of the heart, has seemed more general in that from the left side; which, it is suggested, is perhaps deserving of notice in connexion with the subject of respiration.

"A new Theory of Physics, with its application to important phænomena hitherto considered as ultimate facts." By Thomas Exley, Esq., A.M.

The theory of the author is founded on the two following propositions, namely, that

1. Every atom of matter consists of an immense sphere of force, varying inversely as the square of the distance from the centre; this force being attractive at all distances, except in a small concentric sphere, in which it is repulsive.

2. Atoms differ from each other in their absolute forces, or in the extent of their spheres of repulsion, or in both these respects.

The author assumes that there are four classes of atoms, the *tenacious*, the *electric*, the *ethereal*, and the *aromatic*. The existence of the last-named class of atoms he infers from the phænomena of vegetation, the miasmata of marshes, the aroma of plants, various noxious effluvia, the disinfecting property of some bodies, and facts relating to animalcules, and their ova, &c. He regards the two propositions which constitute the great principles of his theory, as presenting, at once, a complete explication of the general attributes of matter and body, with the Newtonian laws of motion, not otherwise theoretically explicable.

After pursuing at some length his theoretical speculations, founded on the above-mentioned propositions, the author concludes his paper with the following sentence:—

"The several partial theories of philosophers, as far as concerns the leading facts on which they are based, are contained in the simple principles here developed: thus, the theory of universal gravity is here carried out to its ultimatum; Newton and Boscovich's theories of alternate attractions and repulsions are derived from facts which depend on the alternate atmospheres, and neutral spaces of tenacious atoms; Sir Humphry Davy's theory of electrical energies, Dr. Dalton's atomic theory, and the theory of the diffusion of gases, Dr. Black's theory of latent heat, Gay-Lussac's theory of volumes, Newton's theory of light, or the theory of the emission of light, the undulatory theory, and very many others are here united in the most simple principles, which are, therefore, strongly recommended to the notice of philosophers."

"On the Organs of Reproduction, and on the Development of the Myriapoda." By George Newport, Esq. Communicated by P. M. Roget, M.D., Sec. R.S.

The author commences his paper by stating that great interest attaches to the study of the Myriapoda, from the already known fact that their mode of development, by an increase in the number of segments, is directly the reverse of that of true insects in which the development of the perfect individual is accompanied by an apparent diminution in the number of these parts. He remarks, that although the development of the Myriapoda has already been examined by several eminent naturalists, such as Degeer, Savi, Gervais, and Waga, some of the most important facts relating to it have, nevertheless, escaped their notice, and he proposes, therefore, to lay before the Society the result of his own investigations on this subject, and also his examinations of the organs of reproduction.

The paper is divided into four sections. In the first, the author describes the organs of reproduction, and shows that the parts described by Treviranus, both in the male and female Julus, are only the efferential ducts in the male, and the oviduct in the female; that in the former there are developed, from the sides of the efferential ducts, a large number of sacs, the structure of which he describes, and states his opinion that these are the proper secretory organs in the male, but remarks that he has not been able to follow out the organs to their fullest extent. In the female, he shows that the oviduct described by Treviranus is covered by an immense number of ovisacs, each secreting only a single ovum; that many hundreds of these exist around the duct, a large proportion of which never reach maturity, being retarded in their growth by the development of others immediately around them; and that the ova, when matured, are passed from the ovisacs into the duct, and are then all deposited at one time. He adverts especially to the remarkable condition of the female oviduct being a single organ, throughout the greater part of its extent, but having a double outlet; and shows its analogy in the internal portion of the organs to those of some insects, and in its double outlet to the Crustacea and Arachnida. He also institutes a comparison between the structure of the male and female organs in this Myriapod, which, from their simplicity, admirably illustrate the uniformity of origin of these structures; more especially the analogy between the ovisacs in the female and the cæca in the male, and also their conformity in the absence, in the latter, of separate vesiculæ seminales, and, in the former, of spermatheca.

The second section is occupied by a short account of the structure of the ovum, in which the author observes the germinal vesicle and macula. He notices especially the presence of the yelk in the earliest stages of development, together with the vesicle and the membranes of the ovum at a later period, as showing in this low form of animal the conformity of structure and laws with those which prevail in the higher forms.

In the third section, the author speaks of the deposition of the ova, and of the habits of the species, as observed in specimens collected and preserved by him for that purpose. These habits he regards as particularly curious. The female excavates for herself a burrow, by digging with her mandibles in the soil, which she has previously moistened with a fluid, supplied, as the author believes, by her immense salivary glands. With this she forms a soft pellet, which she removes from the burrow with her mandibles and anterior legs; and thence, after being brought to the top of the hole, it is passed on to the next pair, and by these on to the next in succession, until it is entirely removed out of the way; after which, she deposits her eggs and closes the burrow with moistened clay. Great difficulty was experienced in preserving the eggs during the observations, from the circumstance that their shell is soft, and dries quickly when exposed to the air. To avoid this, the author had recourse to the plan of inclosing the eggs in a glass tube, filled with clay, and closed with a cork; the eggs being placed in a cell next to the glass.

The fourth section, which constitutes the most important part of the paper, gives the history of the evolution of the embryo. The process is divided by the author into different periods. After a few observations on the earlier changes of the egg, and the proof that they consist in an alteration in the size and appearance of the cells out of which the embryo is formed, he states his having observed that the egg bursts at the end of twenty-five days, by means of a fissure along the dorsal surface, as described by Savi and Waga; and that, in opposition to the remarks of Degeer, the young Julus, as first stated by Savi, is perfectly apodal. The author has also discovered a singular fact, entirely overlooked by all who have attended to the development of these animals, namely, that the young Julus at this time is still an embryo, and is completely inclosed in a shut sac, which terminates in a distinct *funis* at the extremity of the body, and in the proper *amnion*, or fœtal envelope of the animal. He finds, also, that the funis enters at the posterior penultimate segment of the dorsal surface of the body, and not at the dorsal surface of the thoracic region, as seen by Rathke in the Crustacea. The embryo, he says, is retained in connexion with the shell, between the two halves of it, for seventeen days, by means of the funis, which is continuous with a second, or external membrane, *the chorion*, which lines the interior of the shell. He states that the liberation of the embryo from the shell is not effected by any effort of its own, but by the expansive force of the growth of its body. He describes, also, another important fact which had been overlooked by previous observers, relating to the mode and place of origin of the new segment of the body in the Julidæ. The new segments are always produced in a *germinal membrane* immediately before the penultimate segment, which segment, with the anal one, remains permanent throughout the life of the animal. The production of the first set of new segments is commenced even before the animal has burst from the amnion. After leaving the amnion, the young Julus possesses six pair of legs, as stated by Savi and Waga; but the author remarks, in addition, that, notwithstanding this, it is still inclosed in another tunic, the proper skin of the embryo, beneath which new segments are being formed, and which begins to be detached before the embryo has left the amnion. He suggests whether this may not be the representative of the proper tunic of the germinal vesicle. After minutely describing the embryo, and showing that its body is still formed of cells, he states that four pairs of new legs are forming beneath this tunic, and that, on the twenty-sixth day, the young animal throws off this covering, and the legs are developed, and also the six new segments, to a further extent. The animal then takes food, the segments become developed to the same extent as the original ones, until the forty-seventh day, when it again changes its skin, new segments are again produced, and new legs to those segments last formed. In this way it passes through several changes, developing first segments and then legs.

One remarkable circumstance stated is, that the production of segments is *sextuple* in the Julidæ; but this does not hold in other genera, in some of which it is *quadruple*, and in others *double;* but these peculiarities appear in all cases to be characteristic of each distinct genus. In conclusion, he confirms the observation already made by M. Gervais, that the number of eyes is increased as the animal advances in its transformations. The author concludes by stating that he proposes continuing these observations on the Myriapoda at some future period.

The paper is accompanied by drawings of the parts described, and of the successive changes which take place during the development of the animal.

GEOLOGICAL SOCIETY.

[Continued from p. 500.]

April 7, 1841.—A paper was first read, entitled "A Notice on the Occurrence of Triassic Fishes in British Strata," by Sir Philip Grey Egerton, Bart., M.P., F.G.S.

Were the muschelkalk abstracted from the continental series of beds called the Trias, and the keuper made to rest on the bunter sandstone, Sir Philip Egerton says, it would be difficult, if not impossible, to define the proper limits of these formations. The new red sandstone of England, the equivalent of the trias, presents this difficulty, every endeavour to find the muschelkalk having failed; and therefore geologists are compelled either to consider the keuper, the upper member of the trias, to be also wanting, or to be merged in the mass of alternating marls and sandstones comprising the new red series. Lithological structure, consequently, being of no value, palæontological evidence, the author says, becomes of great importance. The beautiful results arrived at by Mr. Owen respecting the Batrachian remains found near Warwick, tend, Sir Philip Egerton states, to render the existence of the keuper extremely probable, though a specific identification with the analogous fossils of the German keuper has not been ascertained. The only instances on record of muschelkalk fishes found in Great Britain, are scales from the Bone Bed at Aust Cliff, and referred by Professor Agassiz to *Gyrolepis Albertii* and *G. tenuistriatus,* common continental muschelkalk fishes. This bed it is well known occurs at the base of the lias, and rests conformably on the green and red marls of the new red sandstone. A thin stratum replete with remains of saurians and ichthyolites occupies a similar stratigraphical position near Axmouth; and Prof. Agassiz, during his visit to England in the autumn of 1840, identified in a series of specimens obtained by Miss Mary Anning, one Placoid, two Lepidoid, and one Sauroid fish, with well-known muschelkalk species. He also determined the existence of fifteen other species from this deposit, none of which have been yet noticed in the continental Triassic group. Two, if not three, of the above muschelkalk ichthyolites are also found at Aust; and a comparison of the Aust and Axmouth species gives five as common to the two localities, twelve as confined to the former, and two to the latter. The only conclusion, Sir Philip Egerton states, which he feels justified in advancing from the facts adduced in this communication is, that the beds in question, hitherto considered as belonging to the lias, must be removed from that formation, inasmuch as they present a series of fishes not only specifically distinct from those of the lias, but possess in the Ganoid genera the heterocerque tail, an organism confined to the fishes which existed anterior to the lias.

Appended to the paper is a systematic catalogue, compiled from the 'Poissons Fossiles,' of the Ichthyolites hitherto described, from the keuper and muschelkalk of the Continent, together with those recently discovered at the Aust Passage and near Axmouth. The following extract from that document contains the species common to the Continent and England:—

Order.	Genus and Species.	English Localities.	Continental Localities and Formations.	
Placoid.	*Hybodus plicatilis.*	Axmouth.	Passim.	Muschelkalk.
Ganoid.	*Gyrolepis Albertii.*	*Ibid.*—Aust.	Passim.	Ibid.
,,	——— *tenuistriatus.*	*Ibid.*—*Ibid.*	Passim.	Ibid.
,,	*Saurichthys apicalis.*	*Ibid.*	Bayreuth.	Ibid.

A letter, dated Helsingförs, January 5th, 1841, from Professor Nordenskiöld to Mr. Lyell, "On Furrowed Rocks in Finland," was then read.

In consequence of Sefström's observations on the lines which traverse the surface of the Scandinavian mountains*, Professor Nordenskiöld has been induced to attend to similar phænomena in Finland, and he states, that he has noticed lines on almost all mountains from Lapland to the south of Finland, ranging with few deviations from N.N.W. and N. to S.S.E. and S. On the highest cliffs they are seldom visible on account of the surface being worn, but wherever the rocks are overlaid with sand and earth the lines are easily discovered on the covering being removed. Professor Nordenskiöld has likewise discovered shallow furrows, from three to six feet wide, on the surface of the north and south sand-ridges or plateaux which separate the water systems of Finland. He has traced them for more than fifty fathoms maintaining the same directions as the lines upon the mountains, and he has noticed that they are sometimes near each other. The localities mentioned in the letter are—near the church of Kemi; between Antila and Raukula post-stations on the road from Torneå to Uleåberg; and at a place in Carelia, some miles from the iron-works at St. Anna in Suojerfoi parish.

In searching for iron-ore near Helsingförs, a shaft twenty feet deep was sunk in alluvial soil; and Prof. Nordenskiöld observed on the surface of some fragments which had been blasted from a rock at the bottom of the shaft, similar lines to those which occur on the mountains, but he was unable to determine their direction, the excavation being filled with water and mud. As the rock was twenty feet below the surface of the water in the Finnish Gulf, this fact, Professor Nordenskiöld says, proves that the lines which traverse the mountains exist also at least twenty feet beneath the level of the Gulf. Another phænomenon of this nature lately observed by him is a furrowed rock of gneiss, not far from Porkala, but six wersts from the shore. The rock is flat and not very large, and at the height of nine feet above the medium level of the water, is one of the round holes called by the Swedes, "Giants' Pots," but of an unusually large size, being about three feet by two and a half feet. It was somewhat larger within, and sixteen feet deep. Professor Nordenskiöld had the water and detritus which it contained taken out, and found at the bottom numerous perfectly rounded stones mixed up with the mud. The sides of the Pot were exactly ground, and as resplendent as gneiss can be made. At the east side the brim was somewhat rounded, and well marked with a number of large, flat, east and west furrows, showing, in Professor Nordenskiöld's opinion, "that the stones and waves had on that side driven in the brim at the time of its formation." On the opposite side the margin was quite sharp, as if the rock had been broken away since the cavity had been ground. On the surface of the rock were north and south lines similar to those on the mountains, and they intersected the east and west furrows mentioned above; Professor Nordenskiöld therefore infers that the lines were made subsequent to the formation of the Giants' Pot.

---

* [A translation of which appeared in Taylor's Scientific Memoirs, Part 9.—Edit.]

With respect to the level of the water in the Finnish Gulf, the following changes are shown to have taken place. On the little island of Fussaro, some miles from Hangövdd, and in the open sea, a mark which was made in the year 1754 is now twenty Swedish (19½ English) inches above the medium height of the water; another which was cut in 1800 is about nine inches; and a third excavated in 1821 is about five Swedish inches. At St. Petersburg and Cronstadt it is believed that no change has taken place since 1645.

A letter, addressed to Dr. Buckland by Mr. Thomas Bailey, "On the Gravel Deposits in the Neighbourhood of Basford," was next read.

The parish of Basford is situated in a valley ranging nearly north and south where it enters the great Trent vale. On the eastern boundary, which is a very elevated district, commences an extensive argillaceous bed containing comparatively few pebbles; on the west are the coal-fields of Radford and Bilborough; on the north-west occurs the magnesian limestone which extends beyond Mansfield; and on the north is the elevated tract or ancient forest of Hurwood, occupied by great accumulations of gravel and sand, agreeing in character with those in the neighbourhood of Basford. In the midst of the valley in which Basford is situated are lower ridges of hills, mostly ranging in the direction of the valley, and containing beds of gravel quite as thick (two to eight feet), and interspersed with boulders as large as those found in the hollows or lower parts. Mr. Bailey is of opinion that none of these deposits were accumulated by fluviatile action, or by any uniform agent operating during long periods, but by a tumultuous commotion, when the surface of the earth was in a different state to that which now prevails with respect to hill and dale—the deposits being very unequal in thickness, contorted in position, and composed of materials very irregularly associated as regards nature and size. The transport of the drift in one direction, the author says, appears to have been sometimes checked by a rush from an opposite point, by which means the materials were forced into ridges having an axis of loose sand. Some of these ridges, he conceives, may have been produced by intermediate hollows having been scooped out, and subsequently filled with gravel.

Mr. Bailey does not offer any positive opinion respecting the direction by which the detritus arrived at its present situation, but he thinks it could not have been transported from the S. or S.E., as it contains no pebbles of Charnwood and Mount Sorrel sienite, or of lias or lias-fossils, or of gypsum; nor from any point between S.W. and N.W., on account of the absence of mountain-limestone pebbles, and as the fragments of chert which it contains differ from the chert of the Derbyshire strata. Had the drift come from the west, he states, it ought to contain detritus from the coal-fields which occupy the whole district between Basford and the Derbyshire limestone hills, whereas he has found only one or two small pieces of what might be called jet. He has also never obtained any specimens of magnesian limestone, though that formation occupies almost the whole country to the north-west and north. Mr. Bailey therefore suggests, that the gravel was drifted from a district between the north and east points.

The mass of these deposits consists of fragments of coarse quartzose rock, frequently tinged in a great variety of ways. Many of the pebbles of sandstone are traversed by white veins which project above the general surface: other specimens are rolled portions of quartzose conglomerates, and the greater part of the materials composing them appear to have been much worn, before they were inclosed in the cement; but some of the fragments have sustained very little abrasion by removal from their native bed, preserving all the sharpness of recent fractures. Small masses of iron-ore are not of unfrequent occurrence. Mr. Bailey has seen only one specimen of mica-slate. Many fragments of trap are found in these deposits and some of them are of considerable size, constituting the largest blocks in the deposit, and sometimes weighing two or three hundred pounds; they are often much worn as well as decomposed on the surface. Fragments of porphyry likewise are not rarely met with; and masses of greenstone, or a compound of hornblende and felspar, are also mentioned by the author.

No freshwater or marine shells have been discovered in these accumulations.

Organic remains derived from other deposits are very rare, with the exception of casts of vegetable origin, Mr. Bailey having found only two impressions of shells. Siliceous fragments of stems of Sigillaria and Stigmaria occur in every pit, retaining more or less indubitable marks of their origin, and occasionally exhibiting on the surface a smooth, coffee-brown-coloured coating. One specimen in the author's possession, measuring nearly four and a half feet in circumference, and weighing about 200 pounds, is stated to retain what appears to be a portion of the original bark.

A large series of specimens accompanied the memoir, and was presented by the author.

A letter, dated February 1840, from Mr. Thompson of Yarrells, near Poole, in Dorsetshire, and addressed to Dr. Buckland, was afterwards read.

The object of this communication is to give an account of a boring in search of water at the Union Workhouse, Longfleet, near Poole.

The first land-spring was tapped at the depth of 36 feet, the surface of the ground being about 90 feet above low tide in Poole Harbour. The water was abundant and rose four feet. The next spring occurred at the depth of 127 feet, and others burst forth at 131 feet, 140 feet, 142 feet, 150 feet, 156 feet, 165 feet, 167 feet and 185 feet, from the surface. They all flowed to the same height, and appeared, Mr. Thompson states, to have been fed from the same source. The next spring was encountered at the depth of 235 feet, in a bed of white sand; it flowed six feet higher, and was more abundant than any of the preceding. The spring which issued from the bottom bed was still stronger, and rose 2½ feet higher than any of the others, or to within 24 feet of the surface. The lowest pipe introduced into the boring had a diameter of three inches; it was worked with "two-lift pumps," the section-pipe of each being two inches bore, for nine hours incessantly, during which 25,728 gallons were discharged. An analysis of the water was made by Mr. J. H. Cooper, and a gallon was found to contain 9½ grains of solid matter, or 4½ grains of oxide of iron, the remainder consisting principally of common salt with a small admixture of sulphate and carbonate of lime, and a trace of oxide of manganese.

At Hamworthy, near Poole, another boring has been made, within 100 yards of the harbour, to the depth of 314 feet, through a series of beds similar to those penetrated at Longfleet, and a similar series of springs was tapped; but as the boring was commenced at 80 feet lower level, or only 10 feet above low tide in Poole Harbour, all the springs flowed over the surface, though not strongly. This boring was abandoned on account of the sand which accumulated in the pipe.

The following is a section of the strata at the Longfleet Union Workhouse:—

No. of the Bed.	Nature of the Bed.	Thickness.		Depth from the Surface.	
		Feet.	In.	Feet.	In.
1.	Black dirt and sand	4	0	4	0
2.	Gravel	4	0	8	0
3.	Fine yellow sand	15	0	23	0
4.	Clay	2	3	25	3
5.	Fine yellow sand	3	6	28	9
6.	Fine brown sand	2	3	31	0
7.	Coarse brown sand and water	5	6*	36	6
8.	Clay	1	6	38	0
9.	Hard fine blue sand	19	0	57	0
10.	Ditto, with white pebbles	2	9	59	9
11.	Hard coarse blue sand	1	3	61	0
12.	Fine blue sand	22	0	83	0
13.	Coarse dark red sand	1	0	84	0
14.	Sulphuret of iron	0	3	84	3
15.	Dark brown clay	1	6	85	9
16.	White clay and sand	3	9	89	6
17.	Light blue clay	21	0	110	6
18.	Dark blue clay	2	0	112	6
19.	Light blue clay	4	0	116	6
20.	Light blue rock	2	0	118	6
21.	Red sandstone	1	6	120	0
22.	Light blue rock	2	0	122	0

---

* The strata marked with an asterisk are those which threw out springs.

No. of the Bed.	Nature of the Bed.	Thickness. Feet.	In.	Depth from the Surface. Feet.	In.
23.	Light blue clay	1	0	123	0
24.	Coarse light blue sand*	4	0*	127	0
25.	Dark blue clay	1	0	128	0
26.	Coarse light brown sand	3	6*	131	6
27.	Dark blue clay	3	0	134	6
28.	Black sand and petrified wood	2	0	136	6
29.	Light brown clay	3	0	139	6
30.	Fine blue sand	0	6*	140	0
31.	White clay and yellow sand	2	0*	142	0
32.	Rock (metallic)	0	8	142	8
33.	Light brown sand	7	6*	150	2
34.	Very hard light rock	1	0	151	2
35.	Fine blue sand	5	0*	156	2
36.	Fine blue sand and clay (with pebbles)	5	0	161	2
37.	Light brown sand-rock	4	0*	165	0†
38.	Dark brown sand	2	0*	167	0
39.	Yellow sand and dark brown clay	4	0	171	0
40.	Yellow sand and white clay	0	6	171	6
41.	Dark brown clay	6	6	178	0
42.	Dark clay and black sand	7	0*	185	0
43.	Light brown clay (hard crust every 6 in.)	44	0	229	0
44.	Hard blue *sand-rock* (blue clay in laminæ).	2	0	231	0
45.	White sand	4	3*	235	3
46.	Pipe-clay	15	0	250	3
47.	Black clay	6	9	257	0
48.	Brown clay	3	0	260	0
49.	Stone	1	0	261	0
50.	White sand	5	0	266	0
	Total	266	2		

A letter, dated Glasgow, January 16, 1841, from Mr. Craig to Dr. Buckland, "On the Boulder Deposits near Glasgow," was also read.

The sand- and gravel-beds of the banks of the Clyde are found, Mr. Craig states, in many places besides the adjacent districts; and though wherever he has examined them they are superimposed on the till, yet he does not know if they always occupy that position. At Chapel Hall, at the height of 350 feet, a bone of a Mammoth or Mastodon was found in a bed of laminated sand containing quartz pebbles with fragments of coal-measures and overlying till. Similar beds of sand occur near Eagleham, twelve miles south of Glasgow; and near Galston in Ayrshire, at the height of 500 feet. The sand-beds near Toll Cross on the Hamilton road extend nearly to Broom House Toll, where they rest on till. East of Glasgow the sands lie in the form of a dyke between beds of clay, and extend from the river to the College, where they are cut off by the whinstone dyke which ranges through the city. On the other side of the Clyde they reach as far as Mr. Dixon's iron-works, but further down the river their thickness is not great. At Mr. Smith's property of Whiteinch twelve feet of sand overlie thirty of soft clay and sand.

---

† The inches are omitted in the MS.

The bed containing recent shells at the entrance of the Arkleston Tunnel*, near Paisley, is 80 feet above high-water level; and a similar bed at Port Glasgow is 40 feet. In both instances it is overlaid by laminated sands similar to those at Toll Cross, and on which the greater part of the city is built. Their highest level in Glasgow is about the same as that at Arkleston.

The boulders are found almost throughout the basin of the Clyde where denudation has not taken place subsequent to their deposition, except in the elevated trap districts, where they are very rare; the only instances mentioned in the paper are the Baker's Reservoir on the summit of the trap range of Kilpatrick, and Cochney Loch; nor do they appear on those strata which have been upheaved by trap. On the side of the road near Cartlane Bridge is an accumulation of very large primary and transition boulders without till, forming a kind of escarpment on the brow of a very precipitous bank leading to the valley of the Tee. The opposite slope is crowned with beds of sand. The level of these beds is stated to be 550 feet above the sea.

Near the source of the Avon is a deposit of sand and gravel 50 feet thick; and similar beds occur at Greenock Mains, on the Ayr road from Muirk Kirk. West of these deposits, on White Haugh Water, are enormous beds of till. Boulders or fragments of compact flesh-coloured felspar and reddish porphyries are very rare in this district, but are common in the higher parts of Lanark and Ayr, where these rocks form dykes or beds in old red sandstone. Mr. Craig has never found the slightest trace of the coal-measures north of the trap which forms its northern boundary,

The following is a summary of the author's observations respecting the nature of the boulders at Bell's Park, and the extent to which they have been scratched:—

Greywacke, similar to that which is associated with slate near Rose Neath, and mica-schist above Lass on the borders of Loch Lomond. Blocks very abundant, generally smooth, angles rounded, scratches longitudinal, seldom or never crossing each other.

Porphyritic traps and basalts are next in abundance; a few of the basalt-blocks are scratched, but none of the porphyritic.

Granite.—The felspar large-grained; masses few in number, much rounded, very smooth, not scratched.

Old red sandstone and conglomerate.—Abundant, much rounded, never scratched. The conglomerate blocks are very like a variety near Glen Sannox in Arran.

Quartzose rock.—Blocks not abundant, very smooth; more rounded than any other, not scratched.

Coal sandstone.—Blocks angular, scratched longitudinally. Fragments of iron-stone rare and angular, but smooth.

---

* At this tunnel a bed of coal is stated to have been changed by trap into a bed of pyrites; but a stratum of limestone, though only two feet from the trap, is reported to retain its organic remains.

Carboniferous limestone.—The masses belonging to this formation are next in abundance to those derived from the traps. When large they are much scratched longitudinally and transversely; and the angles are sharp.

"A note by Mr. Murchison on a Section and a List of Fossils from the State of New York," by James Hall, Esq., was likewise read.

Mr. Murchison says, that in consequence of the researches of Mr. Featherstonaugh, Mr. Conrad, Mr. Hall, Mr. Vanuxem, Mr. R. C. Taylor, and other geologists, large tracts in the British colonies in North America and in the United States have been for some time known to be composed of formations containing Silurian, old red sandstone and carboniferous fossils. Mr. Hall's section, presenting a tabular view of the succession of formations, commences with the red sandstone of Blossburgh in Pennsylvania, proved to be the representative of the old red sandstone or Devonian system of Great Britain, in consequence of its inclosing remains of *Holoptychius* and *Coccosteus*. This deposit is succeeded in descending order by others, referable, on account of their testaceous remains, to the lower part of the same system, and these are again underlaid by limestones and shales, especially at Lockport and Rochester, charged with *Ptilodictya lanceolata* and other Silurian corals and fossils. The lowest deposit alluded to by Mr. Hall is the Medina sandstone. The following sectional list, in descending order, is copied from his communication:—

Red sandstone.
Sandstone and shale, abundance of fossil shells.
Shale, with thin layers of sandstone; Fucoides, abundance; few shells.
Green and black shale, several hundred feet thick.
Black shale.
Moscow shale.
Encrinal limestone.
Ludlowville shale.
Thin mass, with Bellerophons.
Shale.
Thin limestone, with fossils.
Marcellus shale.
Limestone, with hornstone.
Onondaga limestone.
Onondaga saliferous group, containing gypsum and salt-springs.
Lockport limestone.
Rochester shale.
Limestone.
Green shale, with fossils.
Pentamerus limestone.
Green shale and iron ore.
Red and grey sandstone, Medina sandstone*.

With respect to the Onondaga saliferous group, Mr. Murchison points out its extremely low geological position, resting upon a calcareous stratum, which has been proved by its organic remains to be the equivalent of the Wenlock limestone; and he states that it is of higher antiquity than the oldest salt-bearing beds of Russia. Mr. Murchison also alludes to the great value of Mr. Hall's communication in proving the wide application of the palæozoic succession established in Great Britain.

---

* For detailed accounts of the New York Devonian and Silurian Systems and their Organic Remains, see the Geological Reports of the State for 1838, 1839, 1840.

April 21.—A paper "On the Geological Phænomena in the Vicinity of Cape Town, Southern Africa," by the Rev. W. B. Clarke, F.G.S., was read.

Mr. Clarke commences by stating, that having derived no advantage from the labours of previous geologists, his remarks must be regarded as independent of any prior description. He arranges his details under the heads of Physical aspect, Mineral structure, and Geological changes.

1. *Physical aspect.*—The leading physical features are the magnificent serrated mountains called Blue Berg or Hottentot's Holland, which stretch northward for many miles into the interior; and the promontory which extends from Table Mountain to the Cape of Good Hope. Each of these ranges consists of flat-topped masses interspersed with pyramidal or pointed peaks, and separated by deep ravines; and Mr. Clarke states that their outline is evidently owing to deep vertical fissures intersecting horizontal strata, proving also that the Table Mountain is not a solitary example of the feature to which it owes its name. A level area extends from the base of the Blue Berg to the shore; and between the southern termination of that range and Table Mountain, is the low sandy district called the Table Flats, forming an isthmus between Table Bay and False Bay. A prominent but subordinate physical feature is the Lion's Hill, situated below Table Mountain; at the entrance of the Bay is Robben's Island, and between the base of the Blue Berg and the shore is a low range of hills of limited extent.

False Bay is bounded on the west by the Cape Promontory, and on the east by a continuation of the Blue Berg, presenting the same physical structure and geological aspect as the Promontory.

Mineral structure and position.—In detailing the composition of the rocks and the associated phænomena, Mr. Clarke describes separately, and in the following order, first, each of the principal physical masses; namely, the Lion's Rump, the district between Green Point and Camp's Bay, that between Camp's Bay and Cobler's Hole, the Kloof, and Table Mountain; and secondly, the modern deposits, springs and detritus.

The Lion's Rump.—The lowest deposit at the Lion's Rump varies in characters from a glossy soft slate to a hard and siliceous, as well as a crystalline schist, and occasionally to a rock as close-grained as bassanite. The partings of the masses are stated to be frequently lined with a substance resembling soapstone and serpentine; and the intersection of the joints giving the strata a somewhat columnar structure, the rock presents a basaltic aspect. The true line of bedding is not easily to be distinguished, but the author is of opinion that it ranges between north and south. The cleavage, where it can be detected, is nearly perpendicular to the horizon. Overlying these schists and composing the upper part of the hill, is a yellowish, argillaceous and sandy laminated rock, which presents the same jointed structure and cleavage as the schist; and owing to the intersection of the joints, the beds divide into wedge-shaped masses, or regular prisms with pyramidal terminations. Mr. Clarke is of opinion that the subjacent schistose rocks have been intruded into or amongst these upper beds, and he states that the grooved and fluted surfaces betray the intensity of the forces with which the slaty masses were ground against each other. He mentions a quarry below the Lion's Rump at the back of Cape Town, as an example of the disturbed position of the schist and overlying sandy rock. He suggests that the schists may belong to the Cambrian, and the superjacent beds to the lower portion of the Silurian system.

The schistose rocks occur also in Robben's Island; and on the other side of the Lion's Rump they form a reef of hard rock along the shore, occurring at intervals at the bottom of Table Bay, and re-appearing in the rounded low range upon the opposite coast. Grooves and scratches, as well as ripple-marks, are very prominent on many of the slabs.

2. *District from Green Point to Camp's Bay.*—The rocks which form the general base of the Lion's Hill are stated to be best examined along the flat shore which skirts it, and where the successive formations crop out. The slate rocks gradually attain a nearly vertical dip as they recede from the Lion's Rump; and between their outcrop in the sea, where they form the first line of barrier rocks, and Green Point, they first change into mica-slate, which soon becomes charged with hornblende, then presents a mottled aspect, and gneiss is ultimately exposed in contact with granite. At the immediate junction of the gneiss with the granite the former is stated to be in some places superficially black and vitreous, extremely hard, as vesicular as lava, and to be most curiously contorted. Masses of true Lydian stone and other metamorphic rocks are stated to be intercalated between the ridges of slate. The true beds in the vicinity of the gneiss range from S.E. to N.W., but where that rock first appears, the strata, as well as the sandstone, dip under the Lion's Rump at an angle of 82° towards the N.E. One line of joints, called by the author cleavage-joints, is stated to be inclined 18° to the W.S.W.; and some of the intercalated beds are said to have similar joints dipping 23° to the N.W. Directly under the Lion's Head, where the gneiss is in contact with the granite, the beds alter in their direction about 5° to the west, the cleavage joints changing also to a range of 30° to the west; and the strata on the shore are in utter confusion. At this point commences a series of highly curious quartz veins, which intersect the gneiss, passing in some places through the joints, as if of posterior origin to the change which produced that structure in the rock, and they throw off from each side numerous branch veins, often at right angles to the main vein. The gneiss is described as overlaid by granite, and the quartz veins to be

most numerous adjacent to it. Veins of granite are likewise visible on the shore, intersecting the gneiss near the junction of the two formations; and numerous instances of the entanglement of the granite and gneiss were noticed by the author, the fragments of the latter, inclosed in the former, being almost invariably coated by quartz. It is also stated, that veins of quartz traverse the entangled portions exactly in the same manner as the solid mass of gneiss; and that the entanglement is nearly always in connexion with the joints, pointing out, Mr. Clarke is of opinion, that all these phænomena are due to one cause; and he is further of opinion, that the silica in the quartz veins was deposited from a state of vapour. The granite is generally large-grained; but where it forms veins, either in the gneiss or in the great mass of granite, it becomes finely grained.

The whole of the shore from this point is granitic, as well as the Lion's Hill, except the cap of sandstone. The junction of the granite and the gneiss cannot be satisfactorily traced owing to the covering of vegetation, but the granite is flanked by nearly vertical or upturned beds of gneiss and slate, and is capped by horizontal beds of sandstone, which are penetrated by granite veins. These phænomena, Mr. Clarke states, clearly establish the induction, that though the periods may have been distant, the schistose rocks owe their elevation to the up-burst of the granite before the deposition of the sandstone; and that subsequently the granite has been re-heated and further elevated, carrying with it the whole area described to a higher level.

3. *Camp's Bay and Cobler's Hole.*—Granite extends along the shore and around Camp's Bay, lining the edge of the sea with huge rounded blocks, and the masses *in situ* are traversed by deep fissures. Near the cottages situated on the road which winds round the middle portion of the Lion's Head and passes over the Kloof to Cape Town, two trap dykes intersect a soft decomposed granite. Under the Lion's Head at Cobler's Hole, and 400 feet above the level of the sea, the granite ledges, for a vertical height of 10 or 12 feet and 30 yards in horizontal extent, are stated to be covered with shingly soil or an elevated beach, having imbedded shells of the same species as now inhabit the neighbouring ocean; and they are so firmly fixed in the soil or to the granite pebbles, as to require some trouble to extract them entire.

4. *The Kloof.*—A vein of trap near the summit of the pass traverses the granite, which is there also in a state of decomposition. About six feet above the road, the dyke is interrupted by a horizontal shift of eighteen inches to the west. At the Kloof is another dyke, which is described in Dr. Abel's work*.

5. *Table Mountain.*—The eastern side of this mountain is formed of granite for nearly two-thirds of its vertical height. On the flat ground at Wynberg occur large blocks of granite perfectly rounded, and the granite floor has the same smooth and rounded surface. The flat between Wynberg and Constantia has also a substratum of granite, with a covering of blown sand or vegetable soil.

---

* Narrative of a Journey in the Interior of China, &c., 1818.

On the opposite side of False Bay and the Cape Flats granite again rises into lofty mural precipices, capped, as on the west side, by sandstone. It therefore constitutes the fundamental rock of the whole of the district south of the Lion's Head, and is everywhere, except at the flats between Table and False Bays, crested by horizontal beds of sandstone. The soil of the vineyards of Cape Town and Constantia is derived from the decomposition of the granite; and the clay of which it consists is either overlaid or contains a hard layer consisting of quartz pebbles and ferruginous matter. Mr. Clarke does not class this clay, occasionally 100 feet thick, with modern or recent formations, because it occurs at the Lion's Head in gullies, whither it could have been transported only by causes no longer in operation; it is moreover everywhere covered with enormous blocks of sandstone, and occasionally of granite, but is not mingled with them, except at the Kloof, and in the beds of the excavating torrents; he is therefore of opinion that it was accumulated during the period when the whole mass of granite lay beneath the waters of the sea.

The sandstone which enters so largely into the geological constitution of the Cape district, and forms the upper part of Table Mountain, has not yet yielded any organic remains; but in a very similar sandstone, resting upon granite, at Cedar Berg and other parts of the colony, true Silurian Trilobites, with other fossils of that age, have been found*. The Cape sandstone is stated to resemble in mineral character the Caradoc more than the old or new red sandstone, and the altered portions are said to be closely analogous in appearance to the Lickey quartz rock. Many of the beds are soft and white; others are hard, dark-coloured, and very ferruginous; and some are composed of a quartzose conglomerate. Large hollows or excavations, such as exist where the sea beats against a sandstone cliff, appear in all the sandstone escarpments, plainly showing, Mr. Clarke observes, that the sandstone of Table Mountain was once a cliff acted upon by the sea, and the boulders of that rock on the slope beneath bear, he says, unequivocal signs of the action of currents of water. No beds decidedly calcareous were observed by Mr. Clarke; but he mentions a stalactite forty feet long exhibited at Constantia, and the occurrence of similar stalactites on the sides of the mountain at Houts Bay, as indications of the existence of bands of limestone or calcareous sandstone. He also alludes to the two hills of limestone at Cape Agulhas.

Modern Deposits.—These are confined to the dunes along the coast at the foot of the Blue Berg, the sand ridges on the Cape Flats, and the drift sand on the wide space under the slope of the Cape Promontory towards Constantia. Mr. Clarke also includes in them the concretions or calcareous sand tubes formed around the roots and stems of marine plants near Green Point, and at other localities. These accumulations generally assume the form of an elongated tumulus, and are occasionally from 80 to 100 feet thick.

---

* For an account of Dr. Smith's discoveries at Cedar Berg see Mr. Murchison's Silurian System, p. 583. 1839.

The author also alludes to the vegetable and other debris brought together by the rains, and to the commencement by this means of an embryo lignite formation on one side of the Cape Flats.

Springs.—The well-water in Cape Town is considered unwholesome. Under Table Mountain is a spring which rises from the granite, and is computed to throw out daily 150,000 gallons; and at Newlands near Wynberg is a spring of sufficient volume to work two mills, and to discharge daily 850,000 gallons. That these springs are not the result of accumulations from the heights, is proved, Mr. Clarke says, from their not varying with the season, and because the water cannot be made to rise above the level at which it appears.

Detritus.—The accumulations described under this head are entirely local, being derived from the subjacent or neighbouring rocks. The smooth and rounded granite boulders also do not extend beyond the range of the granite, but Mr. Clarke is of opinion that the ancient currents which flowed over the Cape Flats may have assisted in their partial removal, and may have rounded some of them. In the interior, masses of granite, similar to the Tors of Dartmoor, are stated to occur.

Geological changes.—The first points noticed by Mr. Clarke, are the protrusion of the granite through the slates at the Lion's Head, the consequent vertical position of the schistose beds, the occurrence of fragments of granite in blocks of sandstone; and the proofs deducible from the granite veins which penetrate and alter the gneiss, as well as traverse the superincumbent sandstone, of the granite, since its first elevation, having been re-heated. He also alludes to the quartz veins which are crossed by others of the same nature, as evidences of there having been two periods of action during which the rock was fissured and veins formed; and to the trap dykes, as proofs of igneous activity since the consolidation of the granite. He likewise mentions the softening or the decomposition of the granite where traversed by trap dykes.

The author next describes the changes in the relative level of land and sea. Everything, he says, tends to confirm the inference, that the whole country was at a comparatively recent period under water. Thus the shingle beds, resting upon granite, at Cobler's Hole, prove an elevation of at least 400 feet since the present species of testacea inhabited the adjacent seas; and he adds, "The water-worn masses of sandstone and the hollows in the beds of that rock *in situ*, identical with those now produced by sea-waves beating against a cliff, equally prove the condition of previous elevation; and the steep sides of the granite, in parallel lines of coast, also lead to the conclusion that they were so modified by currents acting in lines coincident with their direction." The occurrence of marine shells in the sand at the Cape Flats likewise shows that the sea once covered that district; and the grooves and scratches at the Lion's Rump, Mr. Clarke observes, justly lead to the inference of elevation. Before the commencement of these changes in the relative level of land and sea, False Bay and Table Bay must have been united by a sheet of water more than sixty fathoms deep, extending over the flats, and the Cape Promontory must have been an island. To the action of the sea at that period Mr. Clarke attributes the production of the felspathic clay, and its accumulation at the Lion's Rump; and to the action of currents at an earlier period, when the summit of the Table range lay as islands and reefs not far above the level of the sea, the removal of the sandstone and the excavation of the granite at the Kloof, also the denudation and rounding of the ridge of the Lion's Hill, the denudation of Robben's Island, and the production of those terraces, which from the summit of Table Mountain appear to stretch gradually downwards to the Cape of Good Hope. The separation of the Lion's Rump and the Devil's Mountain from the Table Mountain, and the fissures throughout the range, the author conceives were produced during the elevation of the country. Proofs of changes of relative level of sea and land are stated to be equally apparent in the interior; and Mr. Clarke says, that the inspection of an accurate map will convince the inquirer, that Southern Africa must have been an Archipelago. In conclusion, some general observations are made on the great similarity in the geological composition of Southern Africa and New South Wales.

May 5.—A Memoir "On the Distribution of the Erratic Boulders, and on the contemporaneous unstratified Deposits of South America," by Charles Darwin, Esq., F.R.S., F.G.S., was read.

The extensive regions more particularly noticed in this paper are the plains traversed by the Rio Santa Cruz (lat. 50° S.); Tierra del Fuego, including the coasts of the Strait of Magellan, and the Island of Chiloe (lat. 43° S., long. 73° W.).

Patagonia.—Between the Rio Plata and the Rio Santa Cruz, Mr. Darwin did not observe any boulders, and the only one he noticed in ascending the first 100 miles of the latter river was a mass 7 feet in circumference, about 57 miles from its mouth, or 100 from the Cordillera. At 100 miles from the coast, or 67 from the nearest slope of the Cordillera, transported blocks first occur, and 12 miles nearer the chain they are extraordinarily numerous, consisting of clay-slate, felspathic rocks, chlorite schist and basaltic lava. They are generally angular, and some of them are of immense size, one being 60 feet in circumference, and projecting from 5 to 6 feet above the surface of the ground. The vast open plain on which they lie scattered, is here 1400 feet above the level of the sea, and its surface is somewhat irregular, owing partly to denudation and partly to the protrusion of hummocks and fields of lava. The plain slopes gently and regularly towards the Atlantic, where the sea-cliffs are about 800 feet high; but towards the Cordillera it rises more abruptly, attaining near the chain an elevation of 3000 feet. The highest peaks of the Cordillera in this part of its range do not exceed 6400 feet above the level of the sea. The following section, exhibited in the banks of the Santa Cruz in longitude 70° 50′ W., is given by Mr. Darwin to illustrate the nature of the plain on which the boulders rest.

	feet.
1. Gravel, or well-rounded shingle, coarsely stratified, bearing chiefly on its surface great angular erratic blocks..	212
2. Basaltic lava..	322
3. Variously coloured thin strata, the lower ones containing minute pebbles of the same nature as the boulders, with the exception of the lava	588
	1122
Bed of the Santa Cruz, above the level of the sea	280
	1402

The shingle bed (1.) extends uninterruptedly to the coast, where it is certainly of submarine origin; and from the general similarity of its nature, Mr. Darwin is of opinion, that it was all accumulated under the same circumstances. The contrast in the means of transport between the deposits (3.) and (1.), the former consisting of fine particles and the latter of large pebbles and immense blocks of the same rocks with the former, is noticed by Mr. Darwin as an interesting circumstance.

The valley of the Santa Cruz widens, on approaching the Cordillera, into an estuary-like plain, which has an elevation of only 440 feet; and it is believed by Mr. Darwin to have been submerged within the post-pleiocene period, because existing sea-shells were found near the mouth of the plain, and because terraces, which, near the coast, certainly are of recent submarine origin, extend far up the valley. Around this estuary-like plain, and between it and the great high plain, is a second plain, 800 feet in height, the surface of which, as well as the bed of the river in this part, consists of shingle with great boulders. Some of these are of granite, sienite and conglomerate, rocks, which were not observed by Mr. Darwin on the high plain; and on the contrary, the boulders of basaltic lava which were so numerous there, were entirely absent from this lower plain and the river-course. From these circumstances, and likewise from the immense quantity of solid matter which must have been removed in excavating the valley of the Santa Cruz, the author infers that the boulders on the intermediate plain and in the bed of the river, between 30 and 40 miles from the Cordillera, are not derived from the wreck of the high plain, but were transported from the Cordillera subsequently to the modelling of the country, and within, or not long before, the period of existing shells.

Mr. Darwin did not observe erratic blocks in any other part of Patagonia, but he states, on the authority of Capt. King, that large fragments of primary rocks occur on the surface of the great plain which terminates at Cape Gregory, in the Strait of Magellan.

Tierra del Fuego, and Strait of Magellan.—The eastern portion of Tierra del Fuego is formed of large outliers of the Patagonian formation, fringed by deposits of more recent origin. These lower plains, varying in height from 100 to 250 feet, have been elevated within the post-pleiocene period; and they consist of finely grained argillaceous sandstone arranged in thin horizontal or inclined laminæ, and often associated with curved layers of gravel. On the eastern borders of the Straits of Magellan, and at Elizabeth Island, Cape Negro, Nuestra Señora de Gracia, all within the Straits, as well as along the line of coast extending to Port Famine, the sandstone passes into, or alternates with, great unstratified deposits, either of an earthy nature and whitish colour, or of a hardened coarse-grained mud of a dark colour, both containing angular and rounded fragments as well as great boulders of sienite, greenstone, felspathic rocks, clay-slate, hornblende-slate, and quartz. These are arranged without the slightest indication of order, and are derived from mountains at least 60 miles distant to the west or south-west. Sometimes the mass is divided by beds of stratified shingle. North of Cape Virgins, near the entrance of the Strait, it alternates with beds of argillaceous, horizontally laminated sandstone, often thinning out and becoming curvilinear at each end. The inclosed fragments must, in this case, have been transported at least 120 miles. Though Mr. Darwin observed only two boulders imbedded in this deposit, yet as he did not notice any scattered on the surface of the country, he concludes that the boulders which occur in vast numbers on all the beaches have generally been washed out of the cliffs: in St. Sebastian's Bay, however, on the east coast of Tierra del Fuego, he found many blocks in a protected position at the base of a naked cliff 200 feet high, entirely composed of thin strata of finely grained sandstone; he therefore infers that, in this instance, they must have been derived from a thin superficial deposit. From the form of the land where these boulders occur, it is clear, Mr. Darwin states, that long anterior to the present total amount of elevation, a wide channel must have connected the middle of the Strait of Magellan with the Atlantic; and from the occurrence of boulders on the low neck of land near Elizabeth Island, that at the same period a straight channel must have existed between Otway Water and the eastern arm of the Strait. As the present currents off Cape Horn set from the west, Mr. Darwin says, it is probable that the ancient currents had a similar direction, and this inference, he adds, is in accordance with the fact, that the boulders and smaller fragments have been transported from mountains to the west. Navarin Island, and several adjacent islets off the extreme southern parts of Tierra del Fuego, are fringed at about an equal height by an unstratified boulder deposit, very similar to that of the Strait of Magellan; and in Beagle Channel, which separates Navarin Island from Tierra del Fuego, it occasionally alternates regularly with layers of shingle. This extensive deposit resembles, Mr. Darwin states, the "Till" of Scotland, and the boulder formation of Northern Europe and the East of England. The interstratification of regular beds, the occasional appearance of stratification in the mass itself, the juxtaposition of rounded and angular fragments of various sizes and kinds of rock derived from distant mountains, and the frequent capping of gravel, indicate some peculiar but similar origin in this deposit of the above widely separated regions. Mr. Darwin follows Mr. Lyell in believing that floating ice, charged with foreign matter, has been the chief agent in its formation; but he adds that it is difficult to understand how the finest sediment was arranged in horizontal laminæ, and coarse shingle in beds, while stratification is totally, and often suddenly; wanting in the closely neighbouring till, if it be supposed that the materials were merely dropped from melting drift ice; and he is disposed to think that the absence of stratification, as well as the curious contortions described in some of the stratified masses, are mainly due to the disturbing action of icebergs when grounded. He believes also that the total absence of organic remains in these deposits may be accounted for by the ploughing up of the bottom by stranded icebergs, and the impossibility of any animal existing on a soft bed of mud or stones under such circumstances: In confirmation of the disturbing action of icebergs, Mr. Darwin refers to Wrangell's remarks on their effects off the coast of Siberia.

Chiloe.—North of latitude 47° and between it and the southern extremity of Chiloe, the author landed at several points, but saw no boulders; and he explains their absence by the coast being at a distance from the Cordillera, and separated from it by intervening high land. At Chiloe erratic boulders, often of great size and consisting of granite and sienite, occur in vast numbers along the whole line of the eastern and northern beaches, as well as on the islets parallel to the eastern coast, and on the land at the height of upwards of 200 feet; but the author did not observe any on the western coast at the two points which he examined, nor during an excursion of 30 miles across the high central portion of the island. Chiloe consists, as far as Mr. Darwin ascertained, of mica-slate and volcanic formations, extensively bordered, but chiefly on the eastern and northern sides, by a horizontally-bedded tertiary sandstone and volcanic grit. On the eastern coast, the land is indistinctly modelled into successively rising plains, the surfaces of the upper and the whole thickness of some of the lower being in general composed of stratified shingle. A few boulders occur in this gravel; and as the shores have been extensively denudated, Mr. Darwin infers that most of the very numerous blocks on the beaches were originally included in it. At the northern end of the island, the granitic and sienitic boulders are intermingled, but 30 miles to the southward, the author noticed only granite blocks. The parent rock he believes lies in the Cordillera; and several of the varieties of granite and sienite at the northern end of the island are stated, on the authority of an intelligent resident, to form whole mountains in Reloncavi Sound, on the opposite part of the main land. The larger masses were quite angular, and resembled fragments at the foot of a mountain. One block measured 15 feet in length, 11 in breadth, and 9 in height; another, of a pentagonal form, 11 feet on each side, and at one part projected 16 feet above the sand, in which it was partly buried.

At the extreme northern point of Chiloe, a headland 250 feet high is joined to Lacuy peninsula by a low neck of land; and from its composition, height and stratification, Mr. Darwin ascertained that it was once continuous with the opposite coast. The boulders were much more numerous on the isthmus and its sides at the height of 150 feet, than on any other part of the surrounding country; and as the sea must have flowed over this isthmus in a channel, previous to the amount of elevation, ascertained to have taken place here within the post-pleiocene period, the position of these boulders proves, according to Mr. Darwin, even more clearly than the cases occurring in Tierra del Fuego, the evident relationship between their distribution and the lines of anciently existing sea-channels. In the southern half of Chiloe, and on one of the Chonos islands, the author discovered a deposit of hardened mud, including far transported, angular and rounded fragments, and resembling the till of the Straits of Magellan. In a layer of loose sand at the base of the cliff in the latter locality, he noticed a quantity of comminuted marine shells with a fresh aspect; and at Chiloe he also observed, at a point where a mass of till passed into finely grained laminæ, small fragments of a Cytheræa.

With respect to the age of the boulder formation of Chiloe, Mr. Darwin offers no precise remark, but he says that it probably occurs within the post-pleiocene period, because at a height of 350 feet on the peninsula of Lacuy, and therefore considerably above the level of this formation, a great bed of existing sea-shells was observed, and neither the boulder nor accompanying beds appear to have been of deep-water origin. Similar evidence was adduced respecting the age of the till of Tierra del Fuego. North of 41° 47′ S. lat., Mr. Darwin did not observe on the Pacific side of South America either boulders or till; nor any north of the Straits of Magellan, on the shores of the Atlantic side; and he accounts for the absence of erratic blocks in the latter region by its great distance from the Cordillera. He is also strongly of opinion that till will be found to be limited to the latitudes in which true boulders occur.

Glaciers, &c.—In the concluding part of his memoir, the author offers a few remarks on the glaciers of Tierra del Fuego, and on the transport of the boulders. He did not disembark on any glacier, but in the Beagle and Magdalen channels he passed within 2 miles of several. The mountains were covered with snow, and the glaciers formed many short arms, terminating at the beach in low perpendicular cliffs of ice. Their surface, to a considerable height on the mountains, was perfectly clean and of a bright azure colour; and the former condition he ascribes to their shortness, to their not being flanked by overhanging precipices, and to their not being formed by the junction of two or more smaller streams. The descent of the glaciers, Mr. Darwin states, cannot be very slow, as vast masses continually break off with a great noise, and produce a tumultuous surf on the adjacent beaches. The glaciers in the Beagle Channel were generally bordered by a tongue of land composed of huge fragments of rock, and many boulders were strewed on the neighbouring shores. The glacier which he approached most closely descended to the head of a creek formed on one side by a wall of mica-slate, and on the other by a broad promontory from 50 to 60 feet high, on which he landed: it appeared to consist entirely of enormous masses of granite. This promontory, he conceives, was once a lateral moraine, and as it projects nearly half a mile beyond the extremity of the glacier, and is covered with old trees, he infers that the glacier has diminished in length to that extent.

Mr. Darwin says it is impossible to explain the distribution of boulders without the agency of ice, but he adds, that neither the till of the Strait of Magellan which passes into, and is irregularly interstratified with, a laminated sandstone containing marine remains, nor the stratified gravel of Chiloe, can have been produced like ordinary moraines. The boulders, likewise, on the lower levels at the head of the Santa Cruz river, he considers, could not have been distributed in their present position by glaciers, the surface having been modelled by the action of the sea; and the little inclination of the high plain from the ridge of the Cordillera to where the boulders occur, as well as the absence of mounds or ridges on it, and the form of the fragments, render it very improbable that they were propelled from the mountains by ancient glaciers. Hence, he concludes, that the blocks of Tierra del Fuego and Chiloe were certainly transported by floating ice, and most probably those of the low and high plains of Santa Cruz. Finally, he is of opinion, from the general angularity of the blocks, and from the present nature of the climate of the southern parts of America, which favours the descent of glaciers to the sea in latitudes extraordinary low, that it is more probable that the boulders were transported on the surface of icebergs, detached from glaciers on the coast, than imbedded in masses of ice, produced by the freezing of the sea.

May 19th.—A paper "On the Agency of Land Snails in corroding and making deep Excavations in compact Limestone Rocks," by the Rev. Professor Buckland, D.D., F.G.S., was first read.

During the meeting of the Geological Society of France at Boulogne, in September 1839, Dr. Buckland's attention was called by Mr. Greenough to a congeries of peculiar hollows on the under surface of a ledge of carboniferous limestone rocks. They resembled at first sight the excavations made by Pholades, but as he found in them a large number of the shells of *Helix aspersa*, he inferred that the cavities had been formed by snails, and that probably many generations had contributed to produce them[*].

A few years since, the Rev. N. Stapleton informed the author that he had discovered at Tenby, in the carboniferous limestone on which the ruins of the castle stand, perforations of Pholades 30 or 40 feet above high-water level; but having recently examined the spot, Dr. Buckland ascertained that these excavations were the work of the same species of Helix as that which had formed the cavities in the limestone near Boulogne, and he found within them specimens of the dead shells as well as of the living. The mode of operation by which the excavations were made, he conceives, is the same as that by which the common limpet (*Patella vulgata*) corrodes a socket in calcareous rocks, and he is of opinion that the corrosion is due to the action of some acid secreted from the body of the limpet or helix.

---

[*] See Bulletin Geol. Soc. France, vol. x. p. 434, 1839.

That the perforations, both at Boulogne and Tenby, were not the work of Pholades, Dr. Buckland says, is evident,

1st. From their size and shape, which, instead of the straight and regular form accurately fitting the shell of the animal by which each hole was perforated, are tortuous, irregularly enlarging and contracting, and rarely continuous in a straight line. The holes moreover are often separated by only a thin partition, or are confluent.

2ndly. Because they are wanting on the upper surface of the projecting ledges of limestone, whilst on the sides and lower surfaces of the ledges they are excavated to considerable depths.

The above reasons, Dr. Buckland says, against the excavations having been made by any marine lithophagous animal, are favourable to the hypothesis which refers the production of them to snails. These animals, he observes, could find shelter only on the margin and lower surface of the projecting rock, and the irregular form of the confluent cavities correspond with that of the clusters of snails in their ordinary latitat and hybernation; and if to these reasons be added the fact of finding both living and dead shells in the excavations, the evidence, the author conceives, is decisive as to the agency of snails in producing the phænomena under consideration.

In conclusion, the author offers some remarks on the means by which these hollows have been corroded having been overlooked, in consequence, he suggests, of their having been probably referred to the action of the weather, or water, or to original irregularities in the composition of the stone.

A paper "On Moss Agates and other Siliceous Bodies," by John Scott Bowerbank, Esq., F.G.S., was then read.

In a paper "On the Origin and Structure of Chalk-flints and Greensand Cherts*," Mr. Bowerbank inferred that the sponges from which he conceives those bodies originated, differed from recent keratose sponges only in having possessed numerous siliceous spicula. Since that paper was read, the author, however, has found in true keratose sponges from Australia†, as well as in the sponges of commerce from the Mediterranean and the West Indies‡, siliceous spicula in great abundance. All discrepancies, therefore, between the extinct and modern types of a portion of the animals under consideration, he says, is now removed. In these prefatory remarks, Mr. Bowerbank likewise states that there is at present only one known species of recent sponge (*S. fistularis*) the fibre of which is truly tubular.

The author then proceeds to detail the evidences of the existence in moss agates from Oberstein and other parts of Germany, as well as from Sicily, and in green jaspers from India, of the remains of sponges, in the following order: 1st, the proofs of the fibrous structure; 2nd, of the preservation of gemmules; and 3rd, those of the existence of vascular structure. The specimens were examined as opake objects, with direct light concentrated by a convex lens. The number of agates amounted to nearly 200, and that of green jaspers to about 70.

---

* See Geol. Trans., 2nd Series, vol. vi. Part 1, 1841. Proceedings, vol. iii. p. 278, 1840. [and Phil. Mag., Third Series, vol. xviii. p. 220.]

† Annals of Nat. Hist., April 1841.

‡ Microscopic Journal, vol. i. No. 1, p. 8, 1841.

1. *Fibrous structure.*—Though polished agates afforded Mr. Bowerbank, in almost every specimen, strong evidence of spongeous origin, yet the structure and arrangement of the fibres were seldom perfectly preserved throughout, presenting every intermediate state from complete decomposition to the most distinct spongeous tissue. The siliceous matrix of these remains exhibited a clear and frequently crystalline aspect, but the prevailing tint of the enclosed organic matter was bright red, brown, or ochreous yellow; occasionally, however, the fibre was milk-white or bright green. The colouring matter was generally confined within the bounds of the animal tissue, leaving its surface smooth and uninterrupted; sometimes it occurred only in the interior of the tubular fibre, the sides being semipellucid or milk-white; whilst in other cases not only the fibre was completely charged with colouring matter, but the surface was also slightly encrusted with it. In an agate believed to be from Sicily, the greater part consisted of a confused mass composed of innumerable bright red fibres with no perceptible remains of surrounding structure, but near the margin of the specimen the tubuli were as perfectly preserved as in a recent sponge, presenting a semi-pellucid and horny-looking substance enveloping red fibres. In those instances in which the red pigment did not appear to have entered the tube, the structure was best preserved, and Mr. Bowerbank states that such ought to be the case, as the fibres of the *Spongia fistularis*, though hollow throughout, are closed near the natural termination. The tubes in the Sicilian agate anastomosed in the same manner as the fibres of the Mediterranean sponge of commerce, and in the places where they were intersected they frequently exhibited the internal cavity. These characters, the author remarks, prove that the red fibre is the cast of the interior of the tube, and its diameter, he adds, is as nearly as possible the same as that of the hollow of the tube. In a moss agate from Oberstein the walls of the best-preserved tubuli were charged with red pigment, and the internal cavity was filled with pellucid silex, while the portion which had suffered most from decomposition was a confused bright red mass with obscure traces of fibrous structure.

In the green jaspers from India the organic remains were found to be generally better preserved than in the moss agates of Germany and Sicily, and admitted of being recognised as distinct species. The green colouring matter was confined, with very few exceptions, within the boundaries of the sponge-fibre, the surrounding matter consisting of minute pellucid radiating crystals. Some of the specimens examined by Mr. Bowerbank were furnished with minute contorted tubuli, very similar to those which are described in his former paper* as occurring upon the surface of chalk-flints. In other specimens the fibres were not disposed in the same manner as in the sponge of commerce, but in a series of thin plates, resembling very much the macerated woody fibres of the leaves of some endogenous plants. Only one recent species, from Australia, is known to Mr. Bowerbank to exhibit this structure.

---

* Geol. Trans., 2nd Series, vol. vi. Part I. 1841.

No spicula are mentioned by the author in either the agates or jaspers, and but one instance of the occurrence of foraminifera. The whole of the sponges contained in the green jaspers, Mr. Bowerbank refers to that division of the keratose which he has called *Fistularia*.

2. *Gemmules.*—A specimen of Indian green jasper, which had undergone so great decomposition as to prevent the original fibrous structure from being detected, presented innumerable globular vesicles of nearly uniform size. Many of them were simple and transparent, and could be recognised as organic only by the regularity of their size and form, and by having universally dispersed over their outer surface minute irregular black particles; but by far the greater number of them had in their interior a globular opake body, about one-third their own diameter. Associated with these vesicles were numerous small fibrous masses resembling minute keratose sponges, the largest of which were five or six times the diameter of the vesicles; but the smallest were identical in nature with the nucleus, though in a higher state of development. In other specimens from the same mass of jasper, larger vesicles were found more sparingly imbedded amidst the fibrous tissue of the sponge. From these characters and their resemblance to those of the ova of some recent sponges, Mr. Bowerbank has little doubt that the vesicles are the fossilized gemmules of the sponges which gave the form to the siliceous masses in which they are imbedded. An agate supposed to have come from Oberstein, presented characters which, Mr. Bowerbank is of opinion, indicated gemmules in an immature state, or in different stages of development, fixed to the fibre of the sponge; and in another specimen, believed to have been received from the same locality, gemmules in different conditions were sparingly scattered amid the tissue.

If this idea of the development of the gemmules *in situ* be correct, it will account, the author thinks, for the frequent occurrence of small detached patches of minute sponge-fibre in well-developed and large-sized tissue. Several other specimens, considered by Mr. Bowerbank to contain gemmules in different stages of development or decomposition, are described in the paper, particularly an agate from Antigua in the possession of Mr. R. Brown; and one from Oberstein, which contained vast numbers of small, pellucid, yellow globules, bearing a strong resemblance to the minute granules which occur in the gelatinous or fleshy sheath surrounding the fibres of the sponge of commerce, and which are probably incipient germs. In accounting for the preservation of the gemmules in a fossil state, Mr. Bowerbank refers to the covering of the ova of birds, fishes and reptiles; and he says, it is natural to expect that the gemmules of the sponge should be similarly protected, and therefore preserved after the decay of the sponge from which they derived their origin.

3. *Vascular structure.*—In a species of recent Turkey sponge, and

in some others from Australia*, Mr. Bowerbank detected in the horny sheath which invested the solid fibre, minute anastomosing vessels; but he has not observed a similar vascular covering on the external surface of the two specimens of *Spongia fistularis* which he has examined. The co-existence, however, of this sheath with a tubular fibre, he states, he has discovered in specimens of Indian green jasper. On examining with a power of 60 linear a thin polished slice, he found that some well-preserved tubes, of greater size than the rest, had, on their external surface, a coating of a darker colour than the other parts of the fibre, and were evidently analogous to the vascular sheath of the keratose sponges of commerce. On employing a power of 500 linear, the presence of a reticulated vascular structure was exhibited as distinctly as in the recent sponge, particularly where a portion of the originally horny or fleshy part of the sheath had undergone a slight degree of decomposition. This structure Mr. Bowerbank has also detected in two fragments of flint-pebbles.

The characters exhibited by this external coating are not the only evidences of vascular structure which the author found during his examination of the organic remains inclosed in moss agates and Indian green jaspers, for he discovered in the centre of the tube which exhibited the sheath, a dark thread penetrating the cavity for a considerable distance, and when examined with a power of 500 linear, it assumed the appearance of a spiral tubular thread, frequently obscured by irregular patches of a substance which the author conceives may have been glutinous animal matter. In another specimen of green jasper the spiral course of this curious tissue was much less obscure, and when examined with a power of 800 linear its tubular nature was evident. The same tissue also lined the cavity of almost every fibre of the sponge which was stated to exhibit a structure composed of foliaceous plates, like the skeletons of the leaves of some endogenous plants. In an agate, probably from Oberstein, Mr. Bowerbank says, he detected other evidences of tissue of an exceedingly remarkable character. The fibre, which was very large, had been apparently surrounded by a villose coat, and wherever, by polishing, a longitudinal section had been exposed, one or two minute vessels of uniform diameter and simple structure were visible in the centre of the fibre, and ranging in the direction of its axis. At irregular distances within these vessels the author discovered pellucid round globules, the diameter of which varied from the 1000th to the 2380th of an inch, the diameter of the vessels ranging from the 1000th to the 2000th of an inch. In other parts of the interior of the fibre were opake or semi-pellucid spheres, and in different portions of the agate were considerable numbers of larger, opake, round bodies, the whole of which Mr. Bowerbank considers to be gemmules in various states of development; and he thinks it is extremely probable that the vessels containing the globules were true ovarian ducts. In support of this inference Mr. Bowerbank describes another agate, in which there were no appearances of well-defined anastomosing fibres, but which exhibited numerous long and simple thread-like fibres apparently much decomposed, as their substance consisted sometimes of a congeries of minute separate particles, and sometimes of straight or curved lines composed of minute black bodies. In other cases these strings of incipient gemmules were contained within the boundaries of the tubes, and then presented rarely more than a row of single gemmules; but occasionally the diameter of the vessels appeared to have been much enlarged, and the gemmules were indiscriminately dispersed within its cavity. In some instances also they exceeded in diameter the vessel or its remains, as if they had outgrown and burst their natural boundary, or the walls of the latter had contracted. From the close resemblance in the structure and contents of these vessels to those contained in the large sponge-fibre first described, Mr. Bowerbank has little doubt, whatever may have been their original nature, that they are the same kind of tissue, under somewhat different conditions.

---

* Microscopic Journal, vol. i. No. 1, p. 10.

In all the agates and jaspers which have been microscopically investigated by the author, the spaces not occupied by remains of spongeous texture were filled with silex or chalcedony arranged in bands which conformed more or less to the outline of the enclosed fossil. Where, however, the matrix consisted of radiating crystals, the decayed animal remains frequently appeared to have been impelled forward, in the same manner as the decomposed cellular portions of fossil wood have often yielded to the crystallizing process of the associated mineral matter.

Egyptian jaspers, Mocha stones, &c.—The author has examined also numerous specimens of polished Egyptian jaspers, which, when viewed as opake objects, by direct light and with a power of 150 linear, were found to consist of finely comminuted light buff or brown irregular granules, cemented by semi-transparent silex, very much resembling the state in which it exists in chalk-flints and greensand cherts, and to the variations in its colouring matter the banded appearance of the jaspers is due. Imbedded, but very unequally in the layers composing the jaspers, Mr. Bowerbank discovered hundreds of beautiful foraminifera closely resembling those found in chalk-flints, and often difficult to distinguish from the species found in the Grignon sand of the calcaire grossier.

The Mocha stones which the author has examined, presented no indications of organic structure, the moss-like delineations and other appearances, resembling beautiful, thin, reticulated tissues, being due to dendritical or metallic infiltrations.

In the larger pebbles of a mass of Herefordshire pudding-stone, Mr. Bowerbank discovered the characteristic spongeous structure of chalk-flints.

In conclusion, the author dwells upon the difficulties attending the study of the bodies which he has examined and described, in consequence of the little attention which has been paid, with few exceptions, to the structure of recent sponges; and he states that the aspect of the latter, when viewed by the unassisted eye, is so different from that which it presents when seen under a high microscopic power, that those who have not been accustomed to study recent sponges with that aid would never recognise a similar structure in the fossils described by him. He also shows that the prevalence of keratose sponges over those belonging to the genus *Halichondria* is what might naturally be expected, as the spicula which form the skeleton of the latter would be less likely to be preserved in their original position than the horny fibres of the former.

Lastly, the author alludes to the great share which sponges have had in the production of the solid strata of the earth's crust.

ROYAL ASTRONOMICAL SOCIETY.

[Continued from p. 327.]

January 8, 1841.—Remarks on the Present State of our Knowledge relative to Shooting-Stars, and on the Determination of Differences of Longitude from Observations of those Meteors. By Mr. Galloway.

After adverting to some of the earlier opinions which have been entertained on the nature of fire-balls, shooting-stars, and other igneous meteors, the author remarks that no very definite theory was formed respecting them till towards the end of the last century; for although the cosmical origin of the more remarkable bolides and fire-balls had been suspected, the shooting-stars were generally regarded as atmospherical phænomena, which were ascribed by some to electricity, and by others to the inflammation of hydrogen gas accumulated in the higher regions of the atmosphere. In 1794, Chladni published his celebrated work, in which he gave a catalogue of all the recorded observations of fire-balls; and, from a comparison of the different descriptions, inferred that these meteors have not their origin in our atmosphere, but are cosmical masses moving through the planetary spaces with velocities equal to those of the planets, which, when they encounter the earth's atmosphere, are inflamed by the resistance and friction, and become luminous, sometimes bursting into pieces, and scattering masses of stone and iron on the ground. This opinion was at first greatly ridiculed; but the repeated and even not unfrequent fall of meteoric stones, and the discovery by Howard that all of them present an almost perfect similarity of constitution, widely different from that of any substance found on the earth, at length forced conviction even on the most sceptical. From the close resemblance between fire-balls and shooting-stars, and, indeed, the impossibility in many cases of distinguishing the one class of meteors from the other, Chladni was led also to ascribe a cosmical origin to the latter phænomena. At this period, however, there were no observations from which precise or certain conclusions could be formed respecting the altitudes, velocities, or paths described by the shooting-stars—the elements by which the question of their existence within or beyond the atmosphere could be solved. In the year 1798, the first series of observations for determining these points was undertaken in Germany by Brandes and Benzenberg. Having selected a base-line of about nine English miles in length, and stationed themselves at its extremities, they began to observe on nights previously agreed on; and when a meteor was seen, they immediately traced its apparent path on a celestial map, noting carefully the exact times of its appearance and extinction, with any other circumstances likely to assist in identifying it. The meteors observed simultaneously at both stations were in this manner recognised with considerable certainty; and the comparison of their paths on the two maps afforded data for the determination of their parallaxes and altitudes. The results were as follows:—Between the 11th of September and the 4th of November, 1798, only twenty-two corresponding observations were obtained from which the altitudes could be computed. The altitude of the lowest was about six English miles; there were seven under 45 miles; nine between 45 and 90 miles; six above 90 miles; and one had an altitude of about 140 miles. There were only two observations from which the velocity could be deduced: the first gave 25 miles, and the second from 17 to 21 miles in a second. The most remarkable result was, that at least *one* of the meteors moved upwards, or away from the earth. By these observations, the perfect similarity between fire-balls and shooting-stars, in respect of velocity and altitude, was completely established.

Another attempt, on a more extensive scale, to determine the altitudes and velocities of shooting-stars by means of simultaneous observations, was made by Brandes in 1823, assisted by a number of associates resident in Breslaw and the neighbouring towns. The observations were continued from April to October, and during this interval about 1800 shooting-stars were observed at the different places, out of which number ninety-eight were found which had been observed simultaneously at more than one station. The altitudes of four of these were computed to be under 15 English miles; of fifteen between 15 and 30 miles; of twenty-two between 30 and 45 miles; of thirty-five between 45 and 70 miles; of thirteen between 70 and 90 miles; and of eleven above 90 miles. Two of these last had an altitude of about 140 miles; one of 220 miles; one of 280 miles; and there was one whose height was computed to exceed 460 miles. Thirty-six orbits were obtained; in twenty-six of which the motion was downwards, in one horizontal, and in the remaining nine more or less upwards. In three cases only the observations were so complete as to furnish data for determining the velocity; the results were respectively 23, 28, and 37 English miles in a second, the last being nearly double the velocity of the earth in its orbit. The trajectories were frequently not straight lines, but incurvated, sometimes horizontally, and sometimes vertically, and sometimes they were of a serpentine form. The predominating direction of the motion was from north-east to south-west, contrary to the motion of the earth in its orbit,—a circumstance which has been generally remarked, and which is important in respect of the physical theory of the meteors.

A similar set of observations was made in Belgium in 1824, under the direction of M. Quetelet, the results of which are published in the *Annuaire de Bruxelles* for 1837. M. Quetelet was chiefly solicitous to determine the velocity of the meteors. He obtained six corresponding observations from which this element could be deduced, and the results varied from 10 to 25 English miles in a second. The mean of the six results gave a velocity of nearly 17 miles per second, a little less than that of the earth in its orbit.

The last set of corresponding observations referred to in the paper was made in Switzerland on the 10th of August, 1838; a circumstantial account of which is given by M. Wartmann in Quetelet's *Correspondance Mathématique*, for July 1839. M. Wartmann and five other observers, provided with celestial charts, stationed themselves at the Observatory of Geneva; and the corresponding observations were made by M. Reynier and an assistant at Planchettes, a village about 60 miles to the north-east of that city. In the space of seven and a half hours, the number of meteors observed by the six observers at Geneva was 381; and during five and a half hours the number observed at Planchettes by two observers was 104. All the circumstances of the phænomena—the place of the apparition and disappearance of each meteor, the time it continued visible, its brightness relatively to the fixed stars, whether accompanied with a train, &c., were carefully noted. The trajectories were then projected on a large planisphere. The extent of the trajectories described by the meteors was very different, varying from 8° to 70° of angular space, and the velocities appeared also to differ considerably; but the average velocity concluded by M. Wartmann was 25° per second. It was found, from the comparison of the simultaneous observations, that the average height above the ground was about 550 miles; and hence the relative velocity was computed to be about 240 miles in a second. But as the greater number moved in a direction opposite to that of the earth in its orbit, the relative velocity must be diminished by the earth's velocity (about 19 miles in a second). This still leaves upwards of 220 miles per second for the absolute velocity of the meteor, which is more than eleven times the orbital velocity of the earth, seven and a half times that of the planet Mercury, and probably greater than that of the comets at their perihelia.

From the above results, it is obvious that the heights and velocities of the shooting-stars are exceedingly various and uncertain; but if the observations are in any respect worthy of confidence, they prove that many of these meteors (according to Wartmann's observations, by far the greater number) are, during the time of their visibility, far beyond the limits to which atmosphere is supposed to extend, and that their velocities greatly exceed that which is due to bodies moving at the same distance from the sun under the influence of solar gravitation.

It is perhaps impossible to form any correct estimate of the absolute magnitudes of the meteors. Their apparent magnitudes differ greatly; the greater number resembling stars of the third or fourth magnitude, while many are equal to stars of the first, and some even surpass Jupiter and Venus in brilliancy. It is remarkable that the largest are those which have the greatest altitudes, and only the smaller ones appear to come within 20 or even 40 miles of the earth.

With respect to the casual observations of the phænomena, the accounts of which are very numerous, the most interesting conclusion which has been inferred from them is the periodical recurrence of shooting-stars in unusual numbers at certain epochs of the year. Of these epochs, the most remarkable is that of November, on account of the prodigious number of meteors which have been seen in some years at that time. The principal displays were in 1799, 1832, 1833, and 1834. On the 11th of November, 1799, thousands were observed within a few hours by Humboldt and Bonpland at Cumana; and on the same night by different persons over the whole continent of America, from the borders of Brazil to Labrador, and also in Greenland and Germany. On November 12th, 1832, they were seen over the whole of the north of Europe; and on November 12th, 1833, the stupendous exhibition took place in North America which has been so often described. From the accounts of this phænomenon collected by Prof. Olmsted, M. Arago computed that the number of meteors on this night amounted to 240,000. In 1834, a similar phænomenon recurred on the night of November 13th, but on this occasion the meteors were of a smaller size. In 1835, 1836, and 1838, shooting-stars were observed on the night of November 13th, in different parts of the world; but though diligently looked for on the same night in the last few years, they do not appear to have been more numerous than on other nights about the same season,—a circumstance which has shaken the faith of many in their periodicity.

The second great meteoric epoch is the 10th of August, first pointed out by M. Quetelet; and although no displays similar to those of the November period have been witnessed on this night, there are more instances of the recurrence of the phænomena. In the last three years shooting-stars have been observed in great numbers, both on the 9th and 10th; but they appear in general to be unusually abundant during the two first weeks of August. The other periods which have been indicated are the 18th of October, the 23rd or 24th of April, the 6th and 7th of December, from the 15th to the 20th of June, and the 2nd of January; and it is not improbable that further observations will add to the number.

The different theories which have been given to explain the origin and phænomena of the shooting-stars are next stated. The following are the principal:—

1. That the shooting-stars and fire-balls are substances projected from volcanoes in the moon. It is known that a body projected vertically from the moon with a velocity of about 8500 feet in a second would not fall back upon the lunar surface, but would recede from it indefinitely; and, in order to reach the earth, the projectile would only require, under the most favourable circumstances, to have a velocity of about 8300 feet. Such a velocity, which is only about four or five times greater than that of a cannon-ball, is quite conceivable; but the extraordinary exhibitions of 1799 and 1833, to

say nothing of their supposed periodicity, is [are] utterly irreconcilable with the theory of a lunar origin. Benzenberg, however, adopts this theory, and supposes the shooting stars to be small masses of stone, from one to five feet in diameter, which are projected from lunar volcanoes, and circulate about the earth or about the sun when their projectile velocity exceeds a certain limit.

2. Dr. Olbers, and some other astronomers, have supposed the shooting-stars to be the *débris*, or fragments of a large planet, burst into pieces by some internal explosion, of which Ceres, Pallas, Juno, and Vesta, are the principal remaining portions. The smaller fragments continue to circulate about the sun in orbits of great eccentricity, and when they approach the region of space through which the earth is moving, they enter the atmosphere with great velocity, and by reason of the resistance and friction are rendered incandescent, and emit a vivid light so long as they remain within it.

3. It has been suggested by Biot that the extraordinary displays observed in November may be explained by supposing the meteors to have their origin in the zodiacal light. The extent of this lens-shaped nebulosity is not well ascertained; but as the plane of its principal section is not parallel to the ecliptic, if the earth passes through it at one season, it must be remote from it at another. But shooting-stars are observed at all times of the year; and the November meteors differ from those of other seasons in no respect excepting in their greater multitude.

4. The hypothesis first suggested by Chladni is that which appears to have met with most favour, having been adopted by Arago and other eminent astronomers of the present day to explain the November phænomena. It consists in supposing that, independently of the great planets, there exist in the planetary regions myriads of small bodies which circulate about the sun, generally in groups or zones, and that one of these zones intersects the ecliptic about the place through which the earth passes in November. The principal difficulties attending this theory are the following:—First, that bodies moving in groups in the circumstances supposed must necessarily move in the same direction, and consequently, when they become visible from the earth, would all appear to emanate from one point and move towards the opposite. Now although the observations seem to show that the predominating direction is from north-east to south-west, yet shooting-stars are observed on the same nights to emanate from all points of the heavens, and to move in all possible directions. Secondly, their average velocity (especially as determined by Wartmann) greatly exceeds that which any body circulating about the sun can have at the distance of the earth. Thirdly, from their appearance, and the luminous train which they generally leave behind them, and which often remains visible for several seconds, sometimes for whole minutes, and also from their being situated within the earth's shadow, and at heights far exceeding those at which the atmosphere can be supposed capable of supporting combustion, it is manifest that their light is not reflected from the sun; they must therefore be self-luminous, which is con-

trary to every analogy of the solar system. Fourthly, if masses of solid matter approached so near the earth as many of the shooting-stars do, some of them would inevitably be attracted to it; but of the thousands of shooting-stars which have been observed, there is no authenticated instance of any one having actually reached the earth. Fifthly, instead of the meteors being attracted to the earth, some of them are observed actually to rise upwards, and to describe orbits which are convex towards the earth; a circumstance, of which, on the present hypothesis, it seems difficult to give any rational explanation.

5. The most recent hypothesis is that of Capocci of Naples, who regards the aurora borealis, shooting-stars, aerolites, and comets, as having all the same origin, and as resulting from the aggregation of cosmical atoms, brought into union by magnetic attraction. He supposes that in the planetary spaces there exist bands or zones of nebulous particles, more or less fine, and endued with magnetic forces, which the earth traverses in its annual revolution; that the smallest and most impalpable of these particles are occasionally precipitated on the magnetic poles of our globe, and form polar auroras; that the particles a degree larger, in which the force of gravitation begins to be manifested, are attracted by the earth and appear as shooting-stars; that the particles in a more advanced state of concretion give rise in like manner to the phænomena of fire-balls, aerolites, &c.; that the comets, which are known to have very small masses, are nothing else than the largest of the aerolites, or rather, *uranolites*, which in course of time collect a sufficient quantity of matter to be visible from the earth. This theory of Capocci differs from Chladni's only by the introduction of magnetic forces among the particles, and it is obvious that all the objections to the former theory apply with equal force to this. It may be remarked, however, that some physical connexion between the phænomena of shooting-stars and aurora had been already suspected, and the observations adduced by M. Quetelet afford reason to suppose that the latter phænomenon is also periodical.

From the difficulties attending every hypothesis which has hitherto been proposed, it may be inferred how very little real knowledge has yet been obtained respecting the nature of the shooting-stars. It is certain that they appear at great altitudes above the earth, and that they move with prodigious velocity; but everything else respecting them is involved in profound mystery. From the whole of the facts M. Wartmann thinks that the most rational conclusion we can adopt is, that the meteors probably owe their origin to the disengagement of electricity, or of some analogous matter, which takes place in the celestial regions on every occasion in which the conditions necessary for the production of the phænomena are renewed.

The concluding part of the paper contains an account of the different attempts which have been made to deduce differences of longitude from the observation of shooting-stars. That meteors which appear and are extinguished so suddenly, and which by reason of their great altitude and brilliancy are visible over considerable portions of the earth's surface, would afford excellent natural signals, provided they could be identified with certainty, was an obvious thought; but so long as they were regarded merely as casual phænomena, it could scarcely be hoped that they would be of much use, in this respect, to practical astronomy. As soon, however, as their periodicity became probable, the observation of the phænomena acquired a new interest. In observing the meteors for this purpose, it is assumed that they appear instantaneously to observers stationed at a distance from each other, and that the meteors seen by different observers so placed are identically the same. These points are not altogether free from uncertainty; but the results of the trials that have been already made may be regarded as favourable, and as showing that among the other methods of determining astronomical positions, the observation of shooting-stars is not to be disregarded. At the November meeting of this Society, in 1839, an account was given of Professor Schumacher's observations at Altona on the night of the 10th of August, 1838. On the same night, corresponding observations were made at several observatories in Germany; but those at Breslaw appear to have been the most successful. From twelve coincident observations at Altona and Breslaw, Professor Boguslawski computed the difference of longitude of the two places to be 28^{m} 22^{s}·07, which differs less than a second from that which had been previously adopted. In Silliman's American Journal for October 1840, an account is given of simultaneous observations made on the 25th of November, 1835, at Philadelphia, and at the College of New Jersey, at Princeton. Seven coincidences were observed, and the mean result gave a longitude differing only 1^{s}·2 from the mean of other determinations; the whole difference being two minutes. This appears to have been the first actual determination of a difference of longitude by meteoric observations. In the corresponding observations of Wartmann and Reynier at Geneva and Planchettes, the differences of longitude deduced from three of the meteors, which were attended with peculiarities so remarkable as to leave no doubt of their identity, were respectively 2^{m}, 2^{m} 3^{s}, 2^{m} 5^{s}, whence it would seem that a single observation may be in error to the amount of several seconds of time. In the *Bibliothèque Universelle de Genève* for August 1840, there is given an account of the determination by this method of the difference of longitude between Rome and Naples. The corresponding observations were begun in November 1838, and were continued at intervals under the direction of Father Vico at Rome, and of Capocci and Nobili at Naples. The apparent paths of the meteors were traced on a celestial globe, and the times of appearance and extinction compared with clocks regulated by astronomical observations. The observed times of the extinction of the phænomena presented a very satisfactory agreement, inasmuch as it is stated that there was in general a difference of only a few tenths of a second of time between the partial results for a difference of longitude amounting to 7^{m} 5^{s}·7.

The merit of first suggesting the use of shooting-stars and fire-balls as signals for the determination of longitudes is claimed by Dr. Olbers and the German astronomers for Benzenberg, who published a work on the subject in 1802. Mr. Baily, however, has pointed out a paper published by Dr. Maskelyne twenty years previously, in which that illustrious astronomer calls attention to the subject, and distinctly points out this application of the phænomena. The paper, which is printed on a single sheet, is entitled "A Plan for observing the Meteors called Fire-balls, by Nevil Maskelyne, D.D., F.R.S., and Astronomer Royal," and is dated Greenwich, November 6th, 1783. After recounting some observations, from which he infers that such meteors appear more frequently than is commonly imagined, and stating the particulars to be attended to in observing them, he adds:—

"It would be well if those persons who happen to see a meteor would put down the time by their watch when it first appeared, or was at its greatest altitude, or burst, or disappeared, and again when they hear the sound; and as common watches are liable to vary much in a few hours, that they would, as soon after as may be, find the error of their watch by a good regulator; for, if *the exact time could be had at different places*, the absolute velocity of the meteor, *the velocity of the sound propagated to us from the higher regions of the atmosphere, and the longitudes of places, might be determined.*"

Extracts from the Report of the Council of the Society to the Twenty-first General Annual Meeting, held February 1, 1841.

"The recurrence of this annual meeting of the Society affords the Council its usual opportunity of referring to the principal events of the past session. These, although few in number, are full of interest to those who prosecute the varied branches of astronomical science. For, since our last anniversary, a comet has been discovered at Berlin, by that indefatigable astronomer, M. Galle, which appears to be the same that was observed in the year 1097, and again in the year 1468; thus performing its revolution in about 371 years. And, more recently, another comet has been discovered also at Berlin, by M. Bremicker; which is now visible to those who are possessed of a powerful telescope, but which, however, from the elements at present deduced, has never been previously seen by any human being. The Council cannot but congratulate the Society that this branch of the science appears now more likely to be prosecuted than it has recently been, and thus lead to discoveries that may eventually have an important bearing on our knowledge of the laws and physical constitution of the universe. The subject of parallax also has engaged more than ordinary attention within the last year from the interest excited by M. Bessel's valuable observations on the double star 61 Cygni, which now appear to be brought to a close, and which the Council have considered of sufficient importance to entitle him to the award of the medal of this Society, as will be fully explained by the President in his address at the close of this meeting."

Amongst the instruments alluded to in the statement of the property of the Society, "the Council have the satisfaction of bringing before the notice of the members, the valuable joint present, from Sir John Herschel and his aunt Miss Caroline Herschel, of a seven-feet reflecting telescope made by the late Sir William Herschel, and used by her in many of her observations. This token of respect and attention will be duly estimated by the Society, who will doubtless preserve this interesting memorial of science with more than ordinary care.

"Amongst the losses by death, during the past year, the Council have to regret the decease of Capt. Drummond, Dr. Gregory, Prof. Leybourn, and Mr. Best, on the home list; and MM. Olbers, Poisson, and Littrow, on the foreign list.

"Capt. Thomas Drummond was born at Edinburgh in October 1797, and entered the corps of Royal Engineers in July 1815. In this department his talent for mechanical combinations became conspicuous, which, together with his close attention to the study of chemistry, rendered his services of considerable value. In 1819, he took part in the trigonometrical survey which was then carrying on in Great Britain, under the superintendence of Col. Colby: and whilst in this employment, he suggested the happy idea of applying the brilliant lamp, which goes under his name, to rendering visible the distant stations. An account of this valuable invention is printed in the Philosophical Transactions for 1826*.

"Capt. Drummond was an early member of this Society, and took great interest in promoting its welfare. During the comparisons of the standard scale of the Society with the parliamentary standard, he frequently attended the meetings of the Committee, and aided them with his opinion and advice on several important points. His previous knowledge and experience on such subjects had been well matured by the laborious and extensive comparisons of the new standard bars, used in the trigonometrical survey, that had just before been carried on at the Tower, principally under his management; and where he had alternately to encounter the heat of an oven and the cold of an ice-house: no method being left untried for attaining the greatest degree of accuracy, both in the measures themselves, and in determining the rate of expansion.

"It is supposed that the first shock which Capt. Drummond's constitution received, was in laying down the base-line at Loch Foyle, in Ireland; where he was oftentimes exposed to the inclemencies of the weather, and himself frequently standing in deep water, earnestly intent on the operations that were then carrying on, as a foundation for the future survey of the country. In fact, he entered with so much ardour and zeal into whatever he undertook, that he might be considered the life and soul of every enterprise in which he was engaged. But he was withdrawn from his geodesical pursuits by the Lord Chancellor (Brougham), who placed him at the head of the Boundary Commission that was established as a preliminary to the

---

[* See Phil. Mag., First Series, vol. lxvii. p. 373. 453.—Ed.]

Reform Bill. In this new employment Capt. Drummond exerted himself with his usual zeal and ability, which led to a more intimate connexion with the ministers then in power; and he was, in April 1832, appointed private secretary to Lord Althorp, then Chancellor of the Exchequer. In this situation he aided the cause of this Society, and was the principal means of our obtaining the grant of the present apartments which we now occupy. In July 1835, he was appointed under-secretary to the Earl of Mulgrave, Lord Lieutenant of Ireland. The arduous duties of his office, united with other employments which he undertook, more especially as first commissioner of the Irish Railway Commission, in which his usual good sense and indefatigable exertions were manifest, proved too much for his physical strength. His constitution gradually gave way, and it was soon apparent that the scene was about to close upon him for ever. At length, 'in the plentitude of mental power and the maturity of knowledge, beloved in private and esteemed in public life,' he expired on the 15th of April last, and was followed to the grave by thousands who revered his memory and mourned his loss.

"Dr. Olinthus Gregory was one of the earliest members of the Society, and for some time held the office of secretary. Though educated in the old English school of mathematics, his acquaintance with the continental methods was much more extensive than would have appeared from his writings, which were almost altogether intended for those who had studied the older English writers. He knew generally what was going on abroad, particularly in the extensions of geometry made by the school of Monge. As editor of the well-known Ladies' and Gentleman's Diaries, he was brought into communication with young students who were desirous of distinguishing themselves in the exact sciences. The protection and encouragement which he afforded to those who were pursuing the path which he himself had trodden, will be gratefully remembered by many; and the period of his superintendence of those useful works will be remembered as that in which every contributor of merit found a friend in the editor. His manners were altogether in accordance with what might have been expected from the preceding account; all he did and said was dictated by benevolence of feeling.

"Dr. Gregory's occupations were numerous and engrossing, and his power of application was very great. About thirteen years ago he was attacked with severe illness, from which it was hardly expected that he would finally recover. Although more or less of an invalid from that time till his death, he continued his numerous avocations with all but, if not altogether, his accustomed energy. On the removal of the tax on almanacs, he was the first to see that the publications which he superintended might be made still more useful in diffusing the spirit of scientific inquiry; and, from that time till his death, original treatises, or useful reprints, were made to form part of them.

"The writing by which he is best known to the public at large is his letters on the evidences of Christianity, a work of large circulation. His treatise on mechanics had a considerable celebrity, and was translated into German; it is not improbable that some here present may have learned their first ideas of that science from it. His edition of Dr. Hutton's Course of Mathematics was considerably augmented by himself, and is the best of them all. As an accurate observer, he is known by his experiments on the velocity of sound, which agree almost exactly with those of the first French and three Dutch observers, who were almost simultaneously employed on the same subject*.

"We have frequently to record losses which bear more directly on practical astronomy, but it is not often that we have to regret the termination of a more energetically useful career, and never of a more zealous one. The principal works known to have been written or edited by him are as follow:—

1793. Lessons, Astronomical and Philosophical, 1 vol.
1801. Treatise on Astronomy, 1 vol.
1802. The Gentleman's Diary, under his editorship.
1806. Treatise on Mechanics, 3 vols.
1807. Translation of Haüy's Natural Philosophy, 2 vols.
1808. Pantalogia, of which he was the general editor, and the contributor of about one half, 12 vols.
1810. Third volume of Dr. Hutton's Course of Mathematics, of which he wrote about one half; and he afterwards edited an edition of the whole course.
—— Letters on the Evidences, &c. of Christianity, 2 vols.
1815. Tracts on the Trigonometrical Survey.
1816. Plain and Spherical Trigonometry, 1 vol.
—— Dissertation on Weights and Measures.
1817. An account of his Pendulum Experiments and Astronomical Observations made at Shetland, in the Philosophical Magazine [First Series, vol. liii. p. 426.].
1818. Appointed editor of the Ladies' Diary and general superintendent of the Stationers' Company's Almanacs.
1825. Mathematics for practical Men, 1 vol.
1839. Address to the Cadets of the Royal Military Academy on resigning the Professor's Chair.
1840. Hints to Mathematical Teachers, 1 vol.
—— Tables to be used with the Nautical Almanac.

"He was also, at one period of his life, a large contributor to the leading reviews.

"Thomas Leybourn, Esq. Fellow of the Royal Society, late senior Professor of Mathematics in the Royal Military College, was one of the original members of this Society, having been admitted by the founders in 1820. Mr. Leybourn was born in 1769, at Bishop-Middleham, in the county of Durham. Although he had not the advantages of a regular education, he appears to have devoted himself to mathematical studies from an early period of life; for, soon after 1790, he was a correspondent of Dr. Hutton in the Ladies' Diary; and, in 1795, he became known to the public as the editor

---

[* Dr. Gregory's paper on this subject will be found in Phil. Mag., vol. lxiii. p. 401.—Ed.]

of the Mathematical Repository—a publication which made its appearance at irregular intervals, and of which the object was to afford a channel for giving publicity to lucubrations of greater length, and to the solutions of problems of a higher order of difficulty, than could be admitted into the Diaries. At that time he was employed as land-agent on the estate of a nobleman in Wales. In 1802 he was appointed, on the recommendation of Dr. Hutton, one of the mathematical instructors in the Military College then recently established at Great Marlow—an office which he continued to fill with much ability and advantage to the institution, until within a few months before his death, which took place in March 1840.

"The first series of the Mathematical Repository terminated in 1802, when the work had extended to five duodecimo volumes. Another series was begun in 1803, which was continued, though with some long intermissions, until 1835, when it had extended to six volumes 8vo. To this publication some of the most distinguished mathematicians of the country were occasional contributors; and although, as in all other periodicals, the contributions are of very unequal merit, the work is on the whole valuable to the student of geometry, particularly the earlier volumes of it, which contain many very beautiful specimens of the application of the ancient geometrical analysis. Besides the Mathematical Repository, Mr. Leybourn was editor of the 'Questions proposed in the Ladies' Diary,' published in 1817, in four vols. 8vo; and of the Gentleman's Diary from 1824 till his death. To these several works he contributed little of his own; but his merit in bringing them forward, and the liberality he showed in defraying the expense of their publication (for, with the exception of the Gentleman's Diary, they were all attended with a considerable pecuniary sacrifice), will secure for his name the respect of those who wish well to the propagation of useful knowledge.

"Mr. Leybourn's distinguishing merit, however, consisted in the ability and zeal with which, during the long period of thirty-seven years, he discharged his laborious public duties in the Military College. As an instructor he was highly respected and esteemed by the governors of the institution, and much beloved by his pupils, in whose progress and advancement he took the warmest interest. He was a man of a kind and benevolent disposition; ready at all times to extend a helping hand to those who stood in need of assistance, more especially if they came recommended to him as possessing a taste for the mathematics.

"Richard Best, Esq. was one of the earliest members of this Society, and, while his health permitted, he was a regular attendant at the monthly meetings. He had a sort of hereditary connexion with our science through his mother, who was a niece of Dr. Bradley, being the daughter of his sister Rebekah, the wife of Mr. John Dallaway; and he possessed some books and instruments which had once been the property of that distinguished astronomer. The popular branches of astronomy were to him a source of amusement; he was a diligent observer of telescopic phænomena, and he occasionally furnished an account of them to the periodicals of the day. He was greatly respected in his own neighbourhood, having long been treasurer to most of the local charities, and a liberal supporter of them all. He was a native of Greenwich, where he resided till within a few months of his death; and, for the last thirty-five years, lived in a house which was built by his grandfather, and occupied by him for the first time on the day his son, Mr. Best's father, came of age.

"Mr. Best retired to Henley-on-Thames, where he died on the morning of May 19, 1840, the day on which he would have completed his seventy-third year, regretted by all who had the pleasure of his acquaintance.

"The important discovery of two new planets in our system (Pallas and Vesta) has rendered the name of Olbers familiar to every lover of astronomy. The circumstances that led to this discovery were as singular as they were fortunate; and show the happy results that may arise from a zealous association of individuals in the steady pursuit of some definite object. The detail of those circumstances has been so recently given in the obituary of this distinguished astronomer and mathematician, read at the last anniversary meeting of the Royal Society, that it appears almost needless to repeat them in this place. Yet it may here be stated, that the discovery of Pallas at nearly the same distance from the sun as Ceres (which had been discovered in the preceding year) led Olbers to conjecture that they were fragments of a larger planet, which might have been scattered by some great catastrophe, and that, probably, some other portions of the original mass might be found in nearly the same orbit. His diligence was rewarded by the discovery of Vesta, about five years afterwards; and nearly in the position in which he expected it would be found. The instrument with which he made these discoveries was a very small telescope, and his observatory was a room in the upper part of his house; thus showing to the world with what slender means the most important results may be obtained. But Olbers was not merely a practical astronomer; his treatise on the best mode of determining the orbit of a comet, and his improvements and investigations of various astronomical formulæ, exhibit him as a mathematician of considerable talent. One of the latest papers relative to astronomy, on which he was employed prior to his death, was on a reform of the constellations, both as to their nomenclature and the arrangement of the stars which limit their boundaries: to which subject he was excited by a passage in the Report of the Council at our last anniversary, wherein it was stated that a revision of this kind was about to be made under the superintendence of a committee appointed by the British Association. The views of Olbers are in perfect coincidence with the object proposed; and in the paper above alluded to, and which has been transmitted to the President, he laments the confusion that has been introduced by his predecessors, and suggests some useful hints for a remedy *.

[* For another obituary notice of Dr. Olbers, see Phil. Mag., S. 3., vol. xviii. p. 72.—Ed.]

"Simeon Denis Poisson (born June 21, 1781, died April 25, 1840) was placed, by common consent, at the head of European analysts on the death of Laplace. He was of humble birth, and was admitted in 1793 a student of the *Ecole Polytechnique*, then newly established. It is stated, by the historian of this school, that, at the age of eighteen, he submitted to his professor some ameliorations in the method of demonstrating the binomial theorem; which that teacher, who was no other than Lagrange, read publicly at his next lecture, and which he declared his intention of adopting in future.

"The life of Poisson was one of quiet and uninterrupted study. He never held any situation connected with politics, nor was in any way, during thirty years, prevented from pursuing his one great object, the application of the most abstruse and newest developments of the integral calculus to problems of physics. The number of his memoirs is enormous; to which must be added his elementary treatise on mechanics (which stands at the head of all elementary writings on the application of pure analysis to the properties of matter), his treatises on capillary attraction, on heat*, and on the theory of probabilities.

"It is well known that the energies of Euler, Clairaut, D'Alembert, and the younger Bernoullis, had organised the application of mathematics in a manner which made the subsequent triumphs of Lagrange and Laplace seem almost beyond expectation. The power of the pure mathematics seemed to flag, when Fourier first came forward with his applications of definite integrals and periodic series to questions of physics, which seemed to be unconquerable, and of which the difficulties seemed to be altogether inexpressible by ordinary analysis. A new school of mathematicians was rapidly formed, in whose hands the mode of expression by definite integrals added one more to the instances in which the happy enunciation of questions was all but their solution. Poisson was one of the first of this school in point of time, and by far the greatest in power. Throughout the major part of his writings we trace the same capability of explaining the most abstruse points with fluent clearness and rigid accuracy, combined with that of conquering the physical difficulties of his problem by the most happy art of adaptation.

"Many of his memoirs are on the great questions of physical astronomy, and it is here that he shows that he was not the accident of a fortunate epoch, but that he could handle the instruments of his two great predecessors with skill resembling their own. Perhaps his greatest achievement in this line is the extension of our knowledge respecting the stability of the solar system, as far as it may be affected by perturbations of the mean orbital motions, or of the axical rotations. This question does not, as many imagine, owe all its interest either to the predictive power which is sought, or to the grandeur of the problem considered as the path to such a power. It is to be remembered that the connecting constants between the oldest and most recent astronomy are the lengths of the sidereal year and of the day; and that we cannot assume to talk a common lan-

[* See Taylor's Scientific Memoirs, vol. i. p. 122.—Ed.]

guage with Hipparchus and Ptolemy, unless we have reason to know that these elements continue sensibly unaltered. In addition to the imperfect presumptions derived from observation (imperfect on account of the large liability to error of the older astronomers) Lagrange had shown that the mean motions have no secular inequalities depending on the first power of the disturbing forces; or, so far as this first power was concerned, on any powers of the eccentricities or inclinations. Laplace had shown that a certain secular equation, which should in theory be applied to the sidereal day, would always be too small to be of any importance. Poisson extended the conclusions of Lagrange to the second power of the disturbing forces, and, relatively, to any powers of the eccentricities and inclinations; or rather, we may say, that he showed any secular equation of the mean motions to depend only on the fourth power of disturbing forces; for, in the course of the investigation, it appears that no such equation of any odd order can exist. As far as the fourth powers of eccentricities and inclinations, he actually shows the mutual destruction of an infinite number of non-periodic disturbing terms; the rest of the powers are completed by a general and different investigation. In the problem of the rotation of the earth, he generalises the investigation of Laplace, by taking into consideration the actual change of the axis *on the earth*; the former investigation considering only the change of the axis, supposed to be fixed in the earth, relatively to the stars. The result agrees with that of Laplace as to the non-existence of any sensible secular inequality.

"Poisson belongs to a class of investigators of whom many are always wanted, but one is permanently indispensable.

"The applications of the newest powers of mathematical language should always be made as near as may be to the time when they are first exhibited. But such peculiar combination of address and power, with a perfect knowledge of existing materials, is not of every-day occurrence; nor is it permitted to say, *uno avulso non deficit alter**.

"His Majesty the King of Denmark, who was elected an honorary member of this Society at the last anniversary, has been pleased to continue the offer of the gold medal founded by his royal predecessor, for the first discovery of a telescopic comet; subject to the conditions and regulations already inserted in the Monthly Notices of this Society for November 1835 †. One of these conditions (which is indispensable as far as it affects persons resident in Great Britain) is, that notice of such discovery must be sent by the *first* post to Mr. Baily.

"The Council cannot conclude this Report without expressing a hope that the future efforts of the Society for the promotion and encouragement of astronomy, will be marked with the same success as has hitherto attended their career: and that they may continue to show to the world the happy effects of unanimity and zeal in the pursuit of a favourite science."

[* Another obituary notice of M. Poisson will be found at p. 74 of the preceding volume.—EDIT.]

[† See Phil. Mag., Third Series, vol. ix. p. 294.—ED.].

The President (Sir J. F. W. Herschel, Bart.) then addressed the Meeting on the subject of the award of the Medal, as follows:—

Gentlemen,—The Report of the Council has placed before you so ample a view of the state of the Society, of its labours during the last year, of the accessions to its members, and of the many and severe losses it has had to deplore, that little is left for me to add, except my congratulations on its continued and increasing prosperity. It would be inexpressibly gratifying to me if I could persuade myself that my own exertions in its Chair had contributed, even in a small degree, to that prosperity; but, alas! I have felt only too sensibly how very feebly and inefficiently, especially during the last year, owing to a variety of causes, but chiefly to residence at a distance from London, I have been able to fill that most honourable office.

The immediate object of my now addressing you, gentlemen, is to declare the award by your Council of the gold medal of this Society to our eminent associate, M. Bessel, for his researches on the annual parallax of that remarkable double star 61 Cygni *, researches which it is the opinion of your Council have gone so far to establish the existence and to measure the quantity of a periodical fluctuation, annual in its period and identical in its law with parallax, as to leave no reasonable ground for doubt as to the reality of such fluctuation, as something different from mere instrumental or observational error: an inequality, in short, which, if it be *not* parallax, is so inseparably mixed up with that effect, as to leave us without any criterion by which to distinguish them. Now, in such a case, parallax stands to us in the nature of a *vera causa*, and the rules of philosophizing will not justify us in referring the observed effect to an unknown and, so far as we can see, an inconceivable cause, when this is at hand, ready to account for the whole effect.

I say, in the nature of a *vera causa*, since each particular star must of necessity have some parallax. Every *real*, *existing material* body, must enjoy that indefeasible attribute of body, viz. *definite place*. Now place is defined by *direction* and *distance* from a fixed point. Every body, therefore, which does exist, exists at a certain definite distance from us and at no other, either more or less. The distance of every individual *body* in the universe from us is, therefore, necessarily admitted to be finite.

But though the distance of each particular star be not in strictness infinite, it is yet a real and immense accession to our knowledge to have *measured* it in any one case. To accomplish this, has been the object of every astronomer's highest aspirations ever since sidereal astronomy acquired any degree of precision. But hitherto it has been an object which, like the fleeting fires that dazzle and mislead the benighted wanderer, has seemed to suffer the semblance of an approach only to elude his seizure when apparently just within his grasp, continually hovering just beyond the limits of his distinct

[* See Phil. Mag., Third Series, vol. xiv. p. 296.—Ed.].

apprehension, and so leading him on in hopeless, endless, and exhausting pursuit.

The pursuit, however, though eager and laborious, has been far from unproductive even in those stages where its immediate object has been baffled.

The fact of a periodical fluctuation of *some kind* in the apparent places of the stars was recognized by Flamsteed, and erroneously attributed to parallax. The nearer examination of this phænomenon with far more delicate instruments, infinitely greater refinement of method, and clearer views of the geometrical relations of the subject, rewarded Bradley with his grand discoveries of aberration and nutation, and enabled him to restrict the amount of possible parallax of the stars observed by him within extremely narrow limits.

Bradley failed to detect any appreciable parallax, though he considered 1″ as an amount which would not have escaped his notice. And since his time this quantity has been assumed as a kind of conventional limit, which it might be expected to attain but hardly to surpass. But this was rather because, in the best observations from Bradley's time forward, 1″ has been a tolerated error; a quantity for which observation and mechanism, joined to atmospheric fluctuations and uncertainties of reduction, could not be held rigidly accountable even in mean results; than from any reason in the nature of the case, or any distinct perception of its reality. If parallax were to be detected at all by observations of the absolute places of the stars, it could only emerge as a "residual phænomenon," after clearing away all the effects of the uranographical corrections as well as of refraction, when it would remain mixed up with whatever uncertainties might remain as to the coefficients of the former, with the casual irregularities of the latter, and with all the forms of instrumental and observational error. Now these have hitherto proved sufficient, even in the observation of zenith stars, quite to overlay and conceal that minute quantity of which astronomers were in search.

It is not my intention, gentlemen, to enter minutely into the history of the attempts of various astronomers on this problem, whether by the discussion of observations of one star, or by the combination of those of pairs of stars opposite in right ascension; nor with the occasional gleams of apparent success which, however, have always proved illusory, which have attended these attempts. For such a history, and, indeed, for a complete and admirably drawn up monograph of the whole subject, I must refer to a paper lately read to this Society by Mr. Main, and which is now in process of publication in the forthcoming volume of our Memoirs [*]. In whatever reference I may have to make to the history of the subject, I must take this opportunity to acknowledge my obligations to the author of this paper, as well as for his exceedingly luminous exposition of the results of those more successful attempts on the

[[*] See Phil. Mag., Third Series, vol. xviii. p. 597.—Ed.]

problem by Henderson, Struve, and Bessel, which I shall now proceed more especially to consider.

It would be wrong, however, not to notice that the first indication of some degree of impression beginning to be made on the problem seems to be found in Struve's discussion of the differences of right ascension of circumpolar stars in 1819, 20, and 21. The only *positive* result, indeed, of these observations is, that in the case of twenty-seven stars examined, none has a parallax amounting to half a second. But *below* this, there certainly do seem to be indications in the nature of a real parallax, which might at least suffice to raise the sinking hopes of astronomers, and excite them to further efforts.

But the time arrived when the problem was to be attacked from a quarter offering far greater advantages, and exposed to few or none of those unmanageable sources of irregular error to which the determinations of absolute places are liable. I mean by the measurement of the distances of such double stars as consist of individuals so different in magnitude as to authorize a belief of their being placed at very different distances from the eye; or, as Struve expresses it, *optically* and not *physically* double. This, in fact, was the original notion which led to the micrometrical measurements of double stars; but not only was anything like a fair trial of the method precluded by the imperfections of all the micrometers in use until recently, but the interesting phænomena of another kind, which began to unfold themselves in the progress of those measurements, led attention off altogether from this their original application, which thus lay dormant and neglected, until the capital modern improvements, both in the optical and mechanical parts of refracting telescopes, and the great precision which it was found practicable, by their aid, to attain in these delicate measurements, revived the idea of giving this method, what it never before had, a fair trial. The principle on which the determination of parallax by means of micrometrical observations of a double star turns, is extremely simple. If we conceive two stars very nearly in a line with the eye, but of which one is vastly more remote than the other, each, by the effect of parallax, will appear to describe annually a small ellipse about the mean place as its centre. These two ellipses, however, though similar in form will differ in dimension; that described by the more remote star being comparatively much smaller: consequently, the apparent places being similarly situated in each, their apparent distance on the line joining these apparent places will both oscillate in angular position and fluctuate in length, thus giving rise to an annual relative alternate movement between the individuals both in position and distance, which is greater the greater the difference of the parallaxes.

Thus it is not the absolute parallax of either, but the difference of their parallaxes, which is effectively measured by this method; *i. e.* by repeating the measurements of their mutual distance at all times of the year. But, on the other hand, aberration, nutation, precession, and refraction, act equally on both stars, or so very nearly so as to leave only an exceedingly small fraction of these corrections bearing on the results. And when the stars are very unequal in magnitude, there is a presumption that the difference of their parallaxes is very nearly equal to the whole parallax of the nearer one.

The selection of a star for observation involves many considerations. In that pitched on by M. Bessel (61 Cygni), the *large star* so designated, is in fact a fine double star; nay, one that has been ascertained to be physically double. It is in every respect a highly remarkable star. The mutual distance of its individuals is great, being about 16½″. Now this being necessarily less than the axis of their mutual orbit, affords in itself a presumption that the star is a *near one*. And this presumption is increased by the unusually great proper motion of this binary system, which amounts to nearly 5″ per annum, and which has been made by Sir James South the subject of particular inquiry, and found to be *not* participated in by several small surrounding stars, *which, therefore,* are not physically connected with it. Moreover, the angular rotation of the two, one about the other, has been well ascertained.

Now, it fortunately happens, that of these small surrounding stars there are two very advantageously situated for micrometrical comparison with either of the individuals of the binary star, or with the middle point between them. The one of these (*a*), at a distance of 7′ 42″, is situated nearly at right angles to the direction of the double star; the other (*b*) at a distance of 11′ 46″, nearly *in* that direction. Considering (*a*) and (*b*) as fixed points then, and measuring at any instant of time their distances from (*c*), the middle point of the double star, the situation of (*c*) relative to (*a*) and (*b*) is ascertained; and if this be done at every instant, the relative *locus* of (*c*), or the curve described by it on the plane of the heaven with respect to the fixed base-line *a b*, will become known.

Now, on the hypothesis of parallax, that locus ought to be an ellipse of one certain calculable eccentricity and no other. And its major and minor axes ought to hold with respect to the points, *a, b,* certain calculable positions and no other. Hence it follows that the distances *a c* and *b c* will each of them be subject to annual increase and diminution; and *that,* 1st, in a given and calculable ratio the one to the other; and 2dly, so that the maxima and minima of the one distance (*a c*) shall be nearly contemporaneous with the *mean* values of the other distance *b c,* and *vice versâ.*

Thus we have, in the first place, several particulars independent of mere numerical magnitudes; and, in the second place, several distinct relations *à priori* determined, to which those numerical values must conform, if it be true that any observed fluctuations in these distances (*a b*) (*a c*) be really parallactic. So that if they be found in such conformity, and the above-mentioned maxima and minima do observe that interchangeable law above-stated; and if, moreover, all due care be proved to have been taken to eliminate every instrumental source of annual fluctuation; there becomes accumulated a body of probability in favour of the resulting parallax, which cannot but impress every reasonable mind with a strong degree of belief and conviction.

Now, all these circumstances have been found by M. Bessel, in his discussion of the measures taken by him (which have been very carefully and rigorously examined by Mr. Main in the paper alluded to, as have also M. Bessel's formulæ and calculations, for in such matters nothing must remain unverified), to prevail in a very signal and satisfactory manner. Not one case of discordance, in so many independent particulars, has been found to subsist; and this, of itself, is high ground of probability. But we may go much further. Mr. Main has projected graphically the deviations of the distances (*a c*) and (*b c*) from their mean quantities (after clearing them of the effects of proper motion and of the minute differences of aberration, &c.). Taking the time for an abscissa, and laying down the deviations in the distances so cleared as ordinates, two curves are obtained, the one for the star (*a*), the other for the star (*b*). Each of these curves ought alternately to lie for half a year above, and for half a year below, its axis.—*It does so.* Each of them ought to intersect its axis at those dates when the maximum and minimum of the other above and below the axis occurs. With only a slight degree of hesitation at one crossing—*it does so.* The points of intersection with the axis ought to occur at dates in like manner calculated *à priori*; and so they do within very negligible limits of error. And, lastly, the general forms, magnitudes, and flexures of the curves ought to be identical with those of curves similarly projected, by calculation on an assumed resulting parallactic coefficient. This is the final and severe test: Mr. Main has applied it, and the results have been placed before you:—*oculis subjecta fidelibus.* If all this does not carry conviction along with it, it seems difficult to say what ought to do so.

The only thing that can possibly be cavilled at is the shortness of the period embraced by the observations; viz. from August 1837 to the end of March 1840. But this interval admits of five intersections of each curve with its axis; of two maxima and two minima in its excursions on either side; and of ample room for trying its agreement in general form with the true parallactic curves. Under such circumstances, it is quite out of the question to declare the whole phænomenon an accident or an illusion. *Something* has assuredly been discovered, and if that something be not parallax, we are altogether at fault, and know not what other cause to ascribe it to.

The instrument with which Bessel made these most remarkable observations is a heliometer of large dimensions, and with an exquisite object-glass by Fraunhofer. I well remember to have seen this object-glass at Munich before it was cut, and to have been not a little amazed at the boldness of the maker who would devote a glass, which at that time would have been considered in England almost invaluable, to so hazardous an operation. Little did I then imagine the noble purpose it was destined to accomplish. By the nature and construction of this instrument, especially when driven by clock-work, almost every conceivable error which can affect a micrometrical measure is destroyed, when properly used; and the precautions taken by M. Bessel in its use have been such as might be expected from his consummate skill. The only possible apparent opening for an annually fluctuating error seems to be in the correction for temperature of its scale. But this correction has been ascertained by M. Bessel by direct observation, in hot and cold seasons, and applied. Nor could this cause destroy the evidence arising from the simultaneous observation of the two companion stars, since a wrong correction for temperature would affect both their distances proportionally, leaving the apparent parallactic movement still unaccounted for.

The resulting parallax is an extremely minute quantity, only thirty-one hundredths of a second; which would place the star in question at a distance from us of nearly 670,000 times that of the sun!* Such is the universe in which we exist, and, which we have at length found the means to subject to measurement, at least in one of its members, probably nearer to us than the rest.

It becomes necessary for me now to refer to two series of researches on this important subject, which have been held by your Council to merit very high and honourable mention; though neither of them, separately, for reasons which I shall state, would have been considered as carrying that weight of probability in favour of its conclusions, which would justify any immediate decision of the nature which they have come to in the case of M. Bessel's. I allude to M. Struve's inquiries, by the method of micrometric measures, into the parallax of *α* Lyræ; and to Mr. Henderson's, by that of meridian observations, on the parallax of *α* Centauri.

α Lyræ is accompanied by a very minute star, at the distance of about 43″. That this star is unconnected with *α* by any physical relation, is clear from the fact ascertained by Sir James South and myself, that it does not participate in the proper motion of the large star. The mutual angular distance of these stars has been made by M. Struve the subject of a very extensive series of micrometric measures with the celebrated Dorpat achromatic, bearing this object steadily in view, and working it out to a conclusion of the very same kind, and, though materially inferior in the degree and nature of its evidence to that of Bessel, yet certainly entitled to high consideration. M. Struve's observations on this star, and for this purpose, extend from Nov. 1835 to Aug. 1838, and are distributed over sixty nights, averaging twenty per annum; and from their combination, according to the principle of probabilities, he concludes a parallax of 0″·261. Mr. Main has subjected these observations to an analysis and graphical projection, precisely similar in principle to those I have explained in the case of 61 Cygni. The curves so projected have been subjected to your inspection, and that inspection certainly does leave a very strong impression of a real and tolerably well-ascertained parallax *having* been detected in this star. But at the same time an impression no less decided, owing to irregularities in the march of the curve, when compared with the true parallactic curve, is created,—that the errors of observation are far from being eliminated,—that, on the contrary, they bear such a proportion to the parallax itself as to leave room for some degree of hesitation, and to justify an appeal to a longer series of observations, and to concurrent evidence from other quarters, before declaring any positive opinion. The evidence of this kind, in short, is not equal to that afforded by the similar projection of Bessel's observations of *either* of his two comparison stars. And to this it must be added, that only one star of comparison existing in the line of α Lyræ, the possible effect of temperature and *annual* instrumental variation is not eliminated from the result in the way in which it is from the measures of 61 Cygni; while all that great mutual support which the observations of parallaxes of the two comparison stars afford each other in the latter case, is altogether wanting in the former. These considerations, without any under-estimation of the great importance and value of M. Struve's researches, yet formed essential drawbacks on the immediate admission of his results.

---

* The orbit described by the two stars of 61 Cygni about each other will, therefore, be about 50 times the diameter of the earth's about the sun, or 2½ times that of Uranus.

In a word, I conceive the question of discovery as between these illustrious, but most generous and amicable rivals, may be thus fairly stated. M. Struve's meridian observations in 1819–1821 seem to have made the first impression on the general problem, but too slight to authorize more than a hope that it would yield at no distant day. His micrometric measures of α Lyræ commenced more than a year earlier, and have extended altogether over a longer period than M. Bessel's of 61 Cygni. From their commencement they afford indications of parallax, and these indications accumulating with time have amounted to a high degree of probability, and rendered the supposition of parallax more admissible than that of instrumental or casual errors producing the same influence on the measures. On the other hand, M. Bessel's measures commencing a year later, and continued on the whole through somewhat less time, have exhibited a compact and consistent body of evidence drawn from two distinct systems of measures mutually supporting each other, and so steadily bearing on their object as to leave no more reasonable doubt of its truth than in the case of many things which we look upon as, humanly speaking, certain. And this conviction once obtained, reacts on our belief in the other results, and induces us to receive and admit it on the evidence adduced for it; which, without such conviction so obtained, we might hesitate to do until after longer corroboration of the same kind.

The other series of observations to which I must now call your attention are those of Mr. Henderson, made at the Cape of Good Hope, on the great star α Centauri, the third star in brightness which the heavens offer to our view*. It is a magnificent double-star con-

---

[* See Phil. Mag., S. 3., vol. xiv. p. 316.—Ed.]

sisting of two individuals, the one of a high and somewhat brownish orange, the other of a fine yellow colour, and each of which I consider fairly entitled to be classed in the first magnitude*. Their distance is at present about 15″ asunder, but it is rapidly diminishing, and in no great lapse of time they will probably occult one another, their angular motion being comparatively small. Their apparent distance was formerly much greater: how much we cannot say for want of observations, but probably the major axis of their mutual orbit is little short of a minute of space. They, therefore, afford strong indications of being very near our system. Add to which their proper motion is very considerable, and participated in by both, which proves their connexion as a binary system; and an additional presumption in favour of their proximity may be drawn from their situation in what, from general aspect, I gather to be the nearest region of the milky way, among an immensity of large stars.

Mr. Henderson observed these stars with great care both in right ascension and declination with the very fine transit, and (in spite of certain grievous defects in the axis) the otherwise really good and finely divided mural circle of the Royal Observatory in that colony. Since his return to England, he has reduced these observations with a view to parallax, and the result is the apparent existence of that element to what, after what has been said, we must now call the great and conspicuous amount of a full second. Mr. Main, to whom I am so largely indebted for allowing me to draw so freely on his labours, has also discussed these results, and comes to the conclusion that (as might, perhaps, be expected) the right-ascension observations afford a trace, but an equivocal one, of parallax, but that in declination (I use his words) "The law of parallax is followed remarkably well. There is scarcely an exception to the proper change of sign, according to the change of sign of the coefficients of parallax. This is quite as much as can reasonably be expected in a series of individual results obtained from any meridional instrument for observing zenith distances. We cannot expect to find the periodical function regularly exhibited by the differences. On the whole, therefore, we should say that, in addition to the claims of *α* Centauri on our attention with relation to its parallax, arising from its forming a binary system, its great proper motion, and its brightness,—it derives now much additional importance, in this point of view, from the investigation of Mr. Henderson. This we are at least entitled to assume until some distinct reason, independent of parallax, shall have been assigned for the changes in the declinations. Such I do not consider impossible, having before my eyes the results which Dr. Brinkley derived, in the cases of certain stars, from the Dublin circle. For the present it must be considered that the star well deserves a rigorous examination by all the methods which the author himself has so well pointed out; and that, in the event of a parallax at all comparable with that assigned by Mr. Henderson being found, he will deserve the merit of its first discovery, and the warmest thanks of astronomers, as an extender of the knowledge which we possess of our connexion with the sidereal system."

---

* I have seen *both* their images projected on a screen of three thicknesses of stout paper, the eye being on the opposite side of the screen from that on which the images were depicted.

With this view of Mr. Henderson's labours I fully agree, and await with highly excited interest the result of Mr. Maclear's larger and complete series of observations on this star both with the old circle and with that more perfect one with which the munificence of government has recently supplied the observatory. Should a different eye and a different circle continue to give the same result, we must, of course, acquiesce in the conclusion; and the distinct and entire merit of the *first* discovery of the parallax of a fixed star will rest indisputably with Mr. Henderson. At present, however, we should not be justified in so far anticipating a decision which time alone can stamp with the seal of absolute authenticity.

Gentlemen of the Astronomical Society, I congratulate you and myself that we have lived to see the great and hitherto impassable barrier to our excursions into the sidereal universe; that barrier against which we have chafed so long and so vainly—(*æstuantes angusto limite mundi*)—almost simultaneously overleaped at three different points. It is the greatest and most glorious triumph which practical astronomy has ever witnessed. Perhaps I ought not to speak so strongly—perhaps I should hold some reserve in favour of the bare possibility that it may be all an illusion—and that further researches, as they have repeatedly before, so may now fail to substantiate this noble result. But I confess myself unequal to such prudence under such excitement. Let us rather accept the joyful omens of the time, and trust that, as the barrier has begun to yield, it will speedily be effectually prostrated. Such results are among the fairest flowers of civilization. They justify the vast expenditure of time and talent which have led up to them; they justify the language which men of science hold, or ought to hold, when they appeal to the governments of their respective countries for the liberal devotion of the national means in furtherance of the great objects they propose to accomplish. They enable them not only to hold out but to redeem their promises, when they profess themselves productive labourers in a higher and richer field than that of mere material and physical advantages. It is then when they become (if I may venture on such a figure without irreverence) the messengers from heaven to earth of such stupendous announcements as must strike every one who hears them with almost awful admiration, that they may claim to be listened to when they repeat in every variety of urgent instance, that these are not the last of such announcements which they shall have to communicate,—that there are yet behind, to search out and to declare, not only secrets of nature which shall increase the wealth or power of man, but TRUTHS which shall ennoble the age and the country in which they are divulged, and by dilating the intellect, react on the moral character of mankind. Such truths are things quite as worthy of struggles and sacrifices as many of the objects for which nations contend, and exhaust their physical and moral energies and resources. They are gems of real and durable glory in the diadems of princes, and conquests which, while they leave no tears behind them, continue for ever unalienable.

It must be needless for me to express a hope that these researches will be followed up. Already we have to congratulate astronomy on the resolution taken by one of our great academic institutions to furnish its observatory with an heliometer of the same description as Bessel's; nor can we fear but that the research will speedily be extended to other stars, offering varieties of magnitude and other indications to draw attention to them.

On the whole, then, the award of our medal, which the Council have agreed on, seems to me, under the circumstances, fully justified. I will now request the Foreign Secretary to convey it to our distinguished Associate; and in so doing, I will add our hope that, in the painful and distressing visitation with which it has pleased Providence recently to try him, he may find occasion to withdraw his mind awhile from that melancholy contemplation to receive with satisfaction such a tribute to this his last and perhaps his greatest achievement, accompanied as it is by the truest regard for his private worth and the most respectful sympathy for his present distress.

The Meeting then proceeded to the Election of the Council for the Year 1841, *when the following Fellows were elected, viz.*

President: the Hon. John Wrottesley, M.A.—*Vice-Presidents*: George Biddell Airy, Esq., M.A., F.R.S., *Astronomer Royal*; Sir John F. W. Herschel, Bart., K.H., M.A., F.R.S.; John Lee, Esq., LL.D., F.R.S.; Rev. Richard Sheepshanks, M.A., F.R.S.—*Treasurer*: George Bishop, Esq.—*Secretaries*: Rev. Robert Main, M.A.; Lieut. Henry Raper, R.N.—*Foreign Secretary*: Richard W. Rothman, Esq., M.A.—*Council*: Francis Baily, Esq., F.R.S.; Rev. W. Rutter Dawes; Augustus De Morgan, Esq.; George Dollond, Esq., F.R.S.; Bryan Donkin, Esq., F.R.S.; Rev. George Fisher, M.A., F.R.S.; Thomas Galloway, Esq., M.A., F.R.S.; Edward Riddle, Esq.; Captain W. H. Shirreff, R.N.; Lieut. William S. Stratford, R.N., F.R.S.

March 12.—The following communications were read:—

On a Reformation of the Constellations, and a Revision of the Nomenclature of the Stars. By Dr. Olbers. Translated and communicated by Sir J. F. W. Herschel, Bart.

From the earliest periods of history, it has been found that as soon as mankind turned their attention to astronomy, it became requisite to group the stars into different constellations, whose outlines might seem to designate various figures. Hence arose those several constellations that have been handed down to us from the Indians, Chinese, Egyptians, Persians, Arabians, Peruvians, and others; as well as those which the Greeks adopted, and which have survived to our own times. However curious and interesting these constellations may seem, yet few of them will be found to express with any accuracy the figures which they are intended to represent. It is probable that out of the strange mixture of men, animals, and other objects, which the first astronomers invented, the imaginative Greeks made the present combination or selection. That the Greeks derived these constellations from an Asiatic people, and that they did not (as Newton supposed) invent them shortly after the Argonautic expedition, is evident from their not being able, for some time at least, to explain the constellations according to their mythology. I need only mention the Greek constellation *Εν γονασιν*, or kneeling figure, which we now call Hercules, and the *Ορνις*, or bird, which we now designate as the Swan; neither of which are explained in their mythology. Combined with this mythology the constellations were sung by the Greek and Roman poets, and are now become classical. In the present advanced state of astronomy, however, we do not arrange the stars wholly according to the constellations, but according to their right ascensions; yet these constellations are a valuable assistance towards an artificial memory, and afford us an excellent method whereby we are enabled to know and distinguish the various stars in the heavens, and to remember and record their places. The ancient Greeks reckoned only forty-six constellations, or, at most, forty-seven, if we include the Χηλαι, or claws, of the Scorpion as a separate constellation, which had been denoted as Libra before the existence of the Alexandrian school; to which Hipparchus added the forty-eighth, namely, Equuleus. The flattery of some courtiers was exerted to create two other constellations, viz. the Hair of Berenice and Antinous, but without success; till Tycho at last gave them a permanent place in the heavens.

In the fifteenth century, when navigation was extended beyond the equator, and sailors noted those stars in the southern hemisphere which were not visible to the ancients, they also found it convenient and useful to adopt the same plan of grouping the new stars into constellations. They did not, however, adapt them to the Greek mythology, but selected principally such objects as presented themselves in the newly-discovered countries: whence we have, for the southern constellations, the Phœnix, the Toucan, the Little Water-snake, the Sword-fish, the Flying-fish, the Fly, the Chameleon, the Bird of Paradise, the Peacock, the Indian, and the Crane. The ancients took only those parts of the heavens, as the ground-work of the constellations, where the bright stars existed: consequently, there were many places where there were no constellations, and the stars which were scattered over such situations were called *αμορφωτοι*, or *informes*. There was no inconvenience in this: but some of these empty spaces were very great, and exhibited here and there stars that seemed to be as much entitled to be formed into a constellation as several of the existing ones. Therefore modern astronomers, as Bartschius relates, (and, perhaps, he himself, partly) invented and formed the new constellations called the Camelopard, the Unicorn, the Fly, and the rivers Jordan, Euphrates, and Tigris. The heavens would now appear to be sufficiently covered, and it seemed that all the advantages which the constellations could give to the memory and imagination, in learning astronomy, had been obtained. But when Hevelius, in the latter part of the seventeenth century, had finished, with incredible labour and care, his valuable catalogue of stars, he considered that those persons who were not observers themselves had no right to institute new constellations: and although, with much reluctance, he retained the Camelopard, the Unicorn, and the Fly of Bartschius, yet he rejected the rivers; and instead of them, and in some other vacant spots, introduced the Hounds, the mountain Menalus, Cerberus, the Fox and Goose, the Lizard, the Shield of Sobieski, the Lynx, the Little Lion, the Little Triangle, and the Sextant, and also gave to Antinous a bow and arrow. Unnecessary as this increase of the constellations may be, the indefatigable Hevelius may be allowed to retain it, as the best means of preserving a remembrance of his great work. For his enumeration and classification of the fixed stars, for which he had sacrificed the greatest part of his life, his strength, and his fortune, and by which he hoped to have gained immortal fame as an astronomer, was soon after doomed to yield to the better and more complete British catalogue of Flamsteed, and has now become nearly useless. Astronomers now no longer use it: nor, indeed, can they use it, except in a few cases, and in some researches of little importance. Hevelius's constellations may be said to be analogous to the ancient ones; some of them may be considered as mythological: and as to the rest, they, for the most part, represent animals. Therefore it may be stated, if not in recommendation, at least in defence of them, that if they overwhelm our maps of stars they do not disfigure them. We have got on our maps only two of the constellations that were introduced in the seventeenth century, namely, Charles's Oak and the Brandenburgh Sceptre; for the Heart of Charles is merely the name of a star, and no constellation. Halley had formed Charles's Oak out of the stars that belonged to Argo; and, notwithstanding the protest of Lacaille against this usurpation, this constellation still remains. Kirch was desirous of introducing the Swords, the Orb, and the Sceptre of Brandenburgh. The electoral Swords were covered by the mountain Menalus; and the Orb yields its place to the Bow and Arrow which Antinous had received from Hevelius; and although the new globes very often disarmed Antinous, yet he has not yet taken the Orb in his hand. Moreover the Sceptre of Brandenburgh, although it did not interfere with any other constellation, yet would not have had a place on our globes, if Bode had not been the astronomer-royal of Prussia. The Cock, which was formed from a portion of the ship Argo, has likewise disappeared; the Sceptre of Louis XIV. with which Royer wished to honour his sovereign, yielded its place to the Lizard of Hevelius; the French Lily could not push away the Fly; and so on with many others; for example, the Little Crab, the South Arrow, &c. are quite forgotten, and not even known at the present day. One would now suppose that nearly eighty constellations were quite enough for all useful purposes: but the vanity of introducing new constellations had, in the eighteenth century, exceeded all bounds, and twenty-six more were added to the number. This extravagant number of new constellations, some of which were formed of scarcely visible stars, by no means made the study of astronomy more easy; but, on the contrary, confused it, and rendered it more difficult. Moreover these new constellations are so unsuited to the others, and chosen with so little taste, that no one can look on our modern globes without disgust. The first who introduced this objectionable system was the excellent and distinguished Lacaille. Surely if it were requisite that the whole heavens should be filled with constellations, they might have been chosen according to some general principle. We might have embellished the apparatus and inventions of our chemists, if indeed they could be embellished by them: and as the ancient figures of heroes and animals must be retained, some latitude might be allowed also to astronomical instruments. But figures, like the shop of the sculptor, the chemical furnace, the easel, the microscope, the air-pump, &c., have no relation to the sky, and their being mixed up with the others is heterogeneous, disagreeable, and without any taste. The same remark will apply to the Printing-press and Electrical machine of Bode; also to Lalande's Air-balloon, although this latter constellation may seem to have some connexion with the heavens. The Hermit-bird (*Solitarius*) of Le Monnier might remain, if it did not interfere with Libra; but his Reindeer is quite absurd, on account of its smallness, it being scarcely so large as the Lizard, and much smaller than the Hare. Also Lalande's Messier, which is covered by the horns of the Reindeer, is but a very small figure in comparison with the immense human figures of the ancients that surround him, although it has robbed the greater part of its little possession from Cepheus and Cassiopeia, and has contracted the throne of the latter into a bent form; the little man is in fact quite ridiculous amongst such enormous figures. The immortal name of Frederick the Great needed not the aid of a constellation for its preservation; a constellation, in fact, carved out of Andromeda, and styled *Honores Frederici*. And if it be an honour to this great monarch to have his name enrolled amongst the stars, we must bear in mind that he enjoys this apotheosis, not only with the brave Sobieski, but also with Poniatowski and the insignificant Charles II. of Great Britain. The name of George III. will also be handed down to posterity without the George's Harp of Pater Hell: and the discovery of Uranus will preserve the name of Herschel as long as astronomy exists, without the necessity of placing his telescope in a narrow slip in the heavens. By the Lion and the Lynx the feline tribe had been sufficiently represented in the sky, without any necessity of introducing a Cat amongst the stars, merely because Lalande was fond of this domestic animal. I appeal to the judgement of all those who have compared any of the old celestial maps with the more modern ones, whether they do not feel a repugnance to the absurd mixing of so many heterogeneous constellations. And since by such an immoderate number of them the knowledge of the stars is rendered more difficult, and the taste vitiated, I would entreat astronomers to assist in freeing the heavens from such an useless accumulation, and to remove all the constellations that have been introduced since the time of Hevelius and Flamsteed. If it should be found desirable to take away some of Hevelius's constellations, and even to retain some of those which have been introduced in the eighteenth century, there should be no partiality shown, so as to endanger the wished-for uniformity in our maps; therefore, it might appear quite unnecessary for me to fix a precise point where the line was to be drawn. It would be advisable also that the constellations should be delineated in such an uniform manner in all maps that there should be the same stars in the same parts of the figures. It is true that we do not, after the manner of the ancients, and of Hevelius, denote different stars merely by their place, but more distinctly by letters or numbers; yet it is very useful if we could at once denote the place which a new phænomenon (for example, a comet) has taken, and also the direction of its motion, by the portions of those constellations in which it was observed. We might, in this regard, take the figures in Flamsteed's great atlas as our types; and with the more propriety, since Flamsteed has constructed them according to the ancient figures and the descriptions given by Ptolemy; with this exception, that some of his figures are ugly and badly drawn. This is a point, however, that might easily be remedied, by following the beautiful and pleasing forms of Senex, Vaugondy, Pater Chrysologue, and others. But when once the proposed forms have been adopted there should be no further uncertainty or deviation *.

Continuation of the Investigation for the correction of the Elements of the Orbit of Venus. By Mr. Glashier.

In this paper the author has combined the equations formed for the correction of the elements from the Greenwich observations for the year 1839, with those given in his preceding paper, a notice of which is contained in the eighth Number of vol. v. of the Monthly Notices [or Phil. Mag., Third Series, vol. xviii. p. 601.].

Venus was near her inferior conjunction in the autumn of the year 1839, and the observed errors were consequently very well suited for the correction of the elements. The results deduced now depend on eighty-two equations, which, the author remarks, are formed from the combination of as great a number of observations as have ever been applied in determining the elements of any planet.

The formation of the equations is arrived at in precisely the same manner as in the preceding paper, and the resulting corrections to the elements are as follows:—

Correction of the Semi Axis Major.... = − 0·00000776
—— Eccentricity......... = + 0·00002303

[* On the subject of this memoir, see an abstract of a paper by Sir John Herschel, in the Proceedings of the Society for June, *suprà*, p. 582.—Ed.]

Correction of the Epoch of Mean Long. = + 2″·13
—— Aphelion.......... = + 225
—— Inclination .... = + 3·23
—— Long. of the Node .. = − 21·40.

And Lindenau's elements corrected, for the epoch Jan. 1, 1836, are—

Epoch of Mean Long. = 11^s 2° 1′ 35″·23
—— Aphelion.. = 10 9 15 3
—— Node .... = 2 15 12 3·60
—— Eccentricity = 0·00684568
—— Inclination = 3° 23′ 34″·33.

April 7.—The following communications were read:—

Occultations of Stars by the Moon, observed at Ashurst and Dulwich, in the year 1840. By Robert Snow, Esq.

Observations on the Variability of the Star *α* Cassiopeiæ during the years 1839 and 1840. By Robert Snow, Esq.

Description of the Observatory erected at Starfield, by W. Lassell, Esq.

The author states that this observatory was erected in the summer of the year 1839 at Starfield, near Liverpool, in latitude 53° 25′ 7″ north, and longitude $0^h\ 11^m\ 41^s$ west of Greenwich. The account of the building and instruments is accompanied by drawings and a model, giving a very distinct idea of the ground-plan and elevation of the building, and of the relative position of the instruments. The building consists of one circular apartment, of 14 feet 6 inches diameter, surmounted by a dome, and is based upon a structure of undisturbed clay. The two instruments contained within it are a transit instrument and an equatoreal instrument, both supported on parts of the same pier, which is 11 feet in length, the part supporting the transit instrument being 3 feet 6 inches broad, and that supporting the equatoreal, 2 feet 8 inches broad; and the whole of the pier is kept quite separate from the walls, to avoid any communication of tremor. The dome revolves on eight rollers of 6 inches in diameter, placed at equal distances, and the friction is so small, that the strength of a boy of twelve years of age is quite adequate to turn it. The transit instrument is placed a little to the west of the meridian line, passing through the centre of the dome, and by revolution of the dome commands the whole 180° of sky.

The telescope of the equatoreal is a Newtonian reflector of 9 inches clear aperture, and of 112 inches focal length. The object-mirror the author believes to be of very great perfection of figure, showing the stars in the most favourable states of the atmosphere with perfect roundness, and answering very well to the severe test recommended by Mudge in the Phil. Trans., of alternately exposing different portions of its surface to the rays from the object. The penumbra of a star seen out of focus is truly round, and equal in brightness in its various parts, presenting also the same form on both sides of the place of distinct vision.

The mirror is mounted in a copper tube, 9 feet long and of 9½ inches in diameter, and there is a peculiar contrivance, by means of a lever and a weight, which are brought into action for great altitudes, but which have no effect when the telescope is horizontal, for preventing its flexure, and for securing its steady adjustment in all positions. The effect of this the author describes as rendering the images of stars equally round at all altitudes, and certainly never better than when near the zenith. The declination and hour-circles are each divided to 15′, and by means of verniers can be read to 30″ and 2^s respectively. The polar axis is made in three parts. The lowest part is a hollow cone of metal half an inch thick, the lower end being solid, and turned into a ball 2 inches in diameter. The ball is received into a nearly hemispherical brass step, provided with adjusting-screws. The upper part of the cone is turned and driven into the bored, internal surface of a cap-plate or flange, which affords an attachment almost as firm as if the whole had been cast in one piece. The exterior circumference of the collar of the flange is also turned with great care, and rests on two cast-iron friction-wheels, 7 inches in diameter, with axles of steel. The only bearings of the motion in right ascension being the step at the foot of the axis, and two points in the circumferences of the friction-wheels, the motion is rendered very smooth, and the required impulse for motion as small as can be desired.

For observation of objects at great altitudes, a very simple contrivance is made to elevate the observer conveniently to the required position at the eye-end of the telescope. Fastened to the following side of the opening of the dome is a stiff bar of iron, notched in the manner of a saw, upon which is hung, by iron hooks, a wooden frame, in which a moveable foot-board is adjustable to different heights, in the manner of the shelves of a bookcase, by which combination of elevations every variety of altitude can be obtained, at the same time that the sides of the frame afford an agreeable support to the observer. The operation of advancing the opening of the dome, also carries on the observing-ladder, and the position of the notched bar is such that the sliding of the ladder upon it, for the most part gives the necessary variation in altitude, and is very appropriately fitted to the change of position of the eye-tube of the telescope. The object-end of the telescope and the declination-circle are counterpoised by weights applied to the framework, not to the telescope itself, by which it remains in equilibrio in all positions. With a view of conveying as correct an impression as possible of the general arrangement of the parts of the observatory and the instruments, the author has accompanied his description by a model, on a scale of one-tenth the full size in linear dimensions.

Observations on Bremicker's Comet, with its apparent places, as obtained, with the Equatoreal, at Mr. Bishop's Observatory, and an account of the methods employed in deducing them. By the Rev. W. R. Dawes.

The extreme faintness of this comet rendering it observable only by persons possessing large telescopes, Mr. Dawes was induced to employ for the purpose Mr. Bishop's equatoreally-mounted telescope of 7 inches clear aperture, and 10¾ feet focal length. The micrometer used has four thick wires, two of which are parallel to the screw, and therefore at right angles to the webs, and fixed at an angular distance of 7″ between their adjacent edges, and the other two parallel to the webs, and moveable with them. By means of these four thick wires a small square was formed, as nearly as possible in the centre of the field, and the nucleus of the comet placed in it for observation.

The nucleus resembled a star of about the 10th or 11th magnitude, abruptly diffusing itself into the nebulosity around it. It was very decided, with powers 63 and 105, the latter of which was generally employed in the observations. The first day of observation of the comet was Nov. 14, when, as well as on Nov. 16, 19, and 24, the stellar nucleus was remarked as distinctly visible. On Nov. 19 it appeared rather eccentric on the north following side. After Dec. 3 a succession of cloudy evenings occurred, and the comet was not seen again till Dec. 22, when the stellar nucleus was again visible, and the apparent diameter of the comet was certainly larger. It must also have been brighter, since the observations of it were considered good, though the sky was so hazy that stars of the 4th magnitude were scarcely visible to the naked eye.

The last night of observation was Dec. 29, when it still exhibited a decided stellar nucleus, and the nebulosity appeared more extended and dense. No observations were made to determine its diameter, but it was estimated at 1½′. At no time has any appearance of a tail been suspected.

The method of observation adopted by Mr. Dawes was that of transit-comparisons in right ascension, and of micrometrical comparisons in north polar distance with neighbouring stars whenever it was practicable. Most of the comparison-stars have been identified as contained in the Catalogue of Groombridge; and the author enters greatly into detail respecting the catalogue-places of all his observed stars, preferring the places given in Pond's Catalogue of 1112 stars, whenever they were found there.

A table annexed to the paper, and given in No. 14 of the Monthly Notices, exhibits the observed apparent places of the comet.

May 14.—The following communications were read:—

Description of a Dioptric Telescope, and of a Micrometrical Lunette. By M. Chevalier. Translated by R. W. Rothman, Esq.

The dioptric telescope constructed and described by M. Chevalier differs from other telescopes chiefly in the arrangement of the object-glass. This consists of a large achromatic combination of long focus, and of a small one which reunites the rays transmitted by the primary one. The chief advantage derived from this construction is the great diminution of spherical aberration, and, consequently, the power of increasing the aperture of the telescope with the same focal distance. This advantage is obtained by dividing the excess of curvature between the two achromatic combinations, and by calculating the distance between them, so as best to secure the diminution of the spherical aberration and perfection of the achromatism. The author considers that a great advantage is gained by the process which he adopts of *luting* together the crown and flint-glasses forming the achromatic combinations, by means of balsam of Canada almost cold, this process being not subject to the inconvenience attached to the use of mastic in drops recommended by other artists. He also suppresses the use of diaphragms in the tubes of his telescopes, which he lines with black velvet.

The author's micrometrical lunette consists of a telescope bent at right angles, in the manner of a Newtonian reflector, carrying a sight on the object-end and a small perforated mirror on the eye-piece, which combination permits the observer to see at once, and *with the same eye*, an object at a fixed distance, and the image of the object produced by the telescope. All the usual micrometrical and illuminating methods are applicable to the sight, on which may be traced divisions as small as we choose, being read by a small telescope parallel to the first, and placed before the metallic mirror. By the author's arrangement, the errors to which common micrometers are subject, depending on the thickness of the wires, the imperfection of the screws, &c. are got rid of. He considers his micrometer as particularly applicable to geodetic operations, and suitable for the purposes of the surveyor as well as of the astronomer.

Observations of the Aurora Borealis. By Robert Snow, Esq.

In this paper Mr. Snow records his observations of this interesting phænomenon made at Ashurst and Dulwich, from the autumn of the year 1834 to the autumn of 1839, within which period several remarkable auroræ appeared; among which, as particularly deserving notice, he describes the various appearances of those of November 17, 1835, and of February 18, 1837; the latter being rendered additionally remarkable by its happening on the same evening as the occultation of Mars by the moon. Other fine auroræ described by the author appeared on November 12, 1837; on September 13, 1838; and on January 19 and September 3, 1839.

The author deduces from his observations the following invariable circumstances of the phænomenon:—That the aurora may be expected at any season of the year; that it assumes nearly every variety of colour; that it resembles both in shape and motion every variety of ordinary cloud; that its appearances are, in the course of the same evening and without any determinate order, undulating, radiating, and streaming, with other capricious forms not easily expressible; that the length of time during which it is visible is very uncertain; that it appears to the eye (geometrical considerations apart) as if it existed at various distances from the earth's surface; that, although for the most part it is not influenced by the presence of clouds, it occasionally tinges them with its own prevailing colours; that this has been noticed only when the clouds are low; that there are also certain *lofty cirrous* clouds, which have the appearance of arranging themselves in peculiar bands or strata, as if in connexion with the aurora; that these strata are visible during daylight, when the visibility of the *dark portion of the arch* has sometimes been strongly suspected; that the stars are seen both well and ill defined through the auroral light and the auroral darkness; that it is by no means confined to the northern regions of the sky, though originating about the magnetic north; that, with the exception of a diminution of its general effect, it is uninfluenced by moonlight; that its appearance generally accompanies weather the reverse of frost, such as heavy wind and rain; and, lastly, that it is wholly inaudible. The author concludes by warning the spectators of this phænomenon against the false impressions to which the senses are liable, especially with regard to the sensation of heat and the notion of sound, as attending phænomena in which our idea of either of these qualities has been predominantly awakened.

Mean Positions of the Stars mentioned in Mr. Baily's Address to Observers, determined at San Fernando in the years 1834, 1835, 1836, 1837, and 1838. By M. Montojo. Translated by Captain Shirreff, R.N.

The whole number of stars contained in this catalogue is 126, of which 121 are mentioned in Mr. Baily's "Address to Observers," as being of doubtful position, or not recently determined. The reductions of the observations of right ascension have been performed strictly by means of the *Tabulæ Regiomontanæ*, the mean positions of the fundamental stars and the numbers and constants of that work having been adopted. The equinox to which they are referred is that of Bessel, coinciding with that determined at San Fernando from 206 observations of the sun.

The transit-instrument and clock employed are described in the published Observations of the years 1834 and 1835; and the instrumental errors there explained have been rigorously taken into account.

The observations in declination include only the years 1837 and 1838, the mural circle having come into operation in 1837.

This instrument differs in no essential respect from those of the Observatory of Greenwich, and was made by Mr. T. Jones.

The mode of observation is essentially similar to that pursued with other mural circles, the horizontal points being obtained by direct and reflexion observations, at the same transit, of different stars ranging through a large arc of zenith distance, and the mean of the results of three days' observations being usually employed. Each observer is made to determine his own horizontal point, some slight disagreement between the results of different observers, similar to what in transit observing is called *personal equation*, having been recognised.

The latitude was determined by superior and inferior transits of Polaris, of β, δ, and λ Ursæ Minoris, and of Cephei 51 (Hev.), using only such observations of them as were made by direct vision and by reflexion at the same transit.

The refraction is computed from the *Tabulæ Regiomontanæ*, and the precession from the values of m and n given in that work.

The resulting right ascensions are compared with the Astronomical Society's Catalogue, with Pond's Catalogue of 1112 Stars, with Wrottesley's Catalogue, and with Johnson's of 606 Southern Stars, and also with the stars found in the Greenwich observations of 1833, 1836, and 1837.

The N. P. D.'s are compared with the Astronomical Society's Catalogue, with Pond's, Johnson's, and Henderson's Catalogues, and with the Greenwich observations of 1836 and 1837.

June 11.—The following communications were read:—

A New Catalogue of Moon-culminating Stars, observed at South Kilworth. By the Rev. Dr. Pearson.

This catalogue contains 520 stars which fall within the passage of the moon, in every situation of the nodes, and which had been published in the Appendix to the first volume of the author's work on Practical Astronomy *. The stars in this catalogue had been selected by Caturegli from the determinations of different observers, and therefore might be supposed to have their places assigned with different degrees of accuracy, so as to require a new series of careful observations, and reduction with improved constants, to render them suitable objects for so delicate a purpose as the determination of longitudes, by comparison with the moon's limb, upon or near the meridian. This consideration induced Dr. Pearson to confine his stellar observations to moon-culminating stars, employing for the purpose, contemporaneously, the transit instrument and circle, described in his work before referred to, and a clock by Hardy, which is an exact fellow to the transit-clock at Greenwich in its original state. The observations were continued uninterruptedly from November 4, 1830, to May 7, 1834; when, on account of inconvenience arising from the smoke of the village, the instruments were removed to another building in the author's private grounds, lying in the same meridian and 4″ to the south of the former. They were here continued till the end of the year 1837, when it was found that, with very few exceptions, the whole of the stars had been observed at least six times.

The reductions were made with the constants given in the Astronomical Society's Catalogue for the epoch 1830, for the purpose of direct comparison with that catalogue, using the annual precessions given in that work. Brinkley's refractions were used in the reduction of the circle observations, which include stars as low as 8° of altitude, and may therefore possibly furnish data for correction of the tables of refractions; with which view the original observations have been preserved. The telescopes used are by Tulley, and the object-glasses, of $3\frac{1}{2}$ inches aperture and nearly 44 inches focal length, may be considered excellent in their definition of a large star. The collimation in altitude was determined by a small collimating telescope, by reversion of the azimuthal positions; and comparison of the sum of reversed readings with 90° proved highly satisfactory in every instance.

The resulting right ascensions of the stars used for clock-error are given in an appendix, that their close agreement with the right ascensions derived from a multiplicity of observations at Greenwich may be exhibited.

Numerical corrections were not applied for the instrumental errors, but the instruments themselves were kept in adjustment by

---

[* See Phil. Mag., Second Series, vol. iv. p. 134.—Ed.]

means of hanging levels. The meridian mark on the northern wall of a house, directly to the south of the transit instrument, was never observed to deviate in position; though the pillar, carrying the northern mark, was liable to deviations when the sun was powerful or the frost severe.

On the Advantages to be attained by a Revision and Rearrangement of the Constellations, with especial reference to those of the Southern Hemisphere, and on the Principles upon which such Rearrangement ought to be constructed. By Sir J. F. W. Herschel, Bart., K.H., V.P.R.A.S., &c. &c.

The stars must have been divided into groups, which, as also single stars, would have received names long before they began to be considered with reference to the seasons, or the sun's motion in the ecliptic. The best-defined groups would be the first named, but in the stars of the zodiac this principle would be afterwards modified, for the convenience of dividing the circle into equal parts. In the extra-zodiacal regions no principle seems to have prevailed, either of subdivision or nomenclature; and many of the figures of the constellations scarcely resemble the forms intended, with the exception of a few, as Scorpio for example. Names, however, would be imposed from other associations than mere form. The author proposes to consider how far the present system of constellations is adapted to the purposes of astronomy, and to examine by what modifications it may be made more serviceable; and also to inquire into the circumstances which render a systematic revision of them desirable, or even necessary, at the present advanced epoch.

The use of constellations to the astronomer is to enable him to refer to a particular star. For this purpose, a distribution on any principle would serve; yet even on this point the present system often leads to confusion: for, 1st, the similarity of the names of several constellations, and the bestowing on a new constellation the name of an old one, with an adjunct for the sake of distinction, render it necessary in many cases, in order to avoid confusion, to write the names at full length: thus, for example, we have (with several others) Ursa major and minor, three Triangles, Pisces and Piscis, Apis and Apus, Telescopium repeated three times, Quadrant, Sextant, and Octant, &c. &c. 2ndly, Some constellations are so extensive that they exhaust three or more alphabets, and therefore it is sometimes necessary, beside the letter of the star, to specify also its right ascension and declination: for example, in Argo three stars are marked *a*, and seven A. Again, such constellations extend over so many hours of right ascension, that the name of the constellation is of no use in finding, in the catalogues, one of the stars composing it: thus, Argo occupies eight hours of right ascension. 3dly, The imperfect and uncertain boundaries of the present constellations lead to confounding the Greek letters of one constellation with those of another contiguous to it. Moreover, the boundaries are not always the same in different maps—a circumstance which alone is decisive of the necessity of some system which might be more favourable to a general understanding.

To the astronomer who refers to stars by their catalogued places, these inconveniences may not afford serious ground of complaint; since even in the southern hemisphere but few stars of the fifth magnitude remain uncatalogued. Nor are the defects of the present system felt by seamen, who have little to do with the constellations beyond referring to a few well-known stars. But to those who employ themselves in the physical departments of practical astronomy, such as variable stars, photometrical determinations, and other subjects, and who require a perfect familiarity with the aspect of the heavens, the present arbitrary and capricious allotment of the stars renders reference to maps constantly necessary. And when the leading stars in the map are not those which in the heavens catch the eye by their brightness, it becomes necessary alternately to inspect the map by candlelight, and then to rush out into the darkness, to compare the impression made on the memory with the visible aspect of the stars, to the loss of all delicacy of vision, and the injury of the organ itself.

The author had proposed to himself to follow out, in the southern hemisphere, the plan adopted by the late Sir William Herschel, in order to place on record the relative apparent magnitudes of the stars at this epoch; and it was while thus engaged that he became impressed with the necessity of a total reformation of the ancient system.

In enumerating the qualities which a system of sidereal arrangement should possess, the writer observes that the new subdivisions should be of moderate extent, the figures easily traced, and the groups such as naturally arrange themselves in distinct forms. Again, the boundaries ought to be definite, so as to be transferred from one map to another without variation; they should accordingly be arcs of great circles, or parallels to a great circle; that is, circles of right ascension and parallels of declination. The limits, thus assumed, should correspond to a particular epoch, being reduced to any other time, by the necessary tables, like stars. The advantages of this system would be that each star would be at once referred to its proper district; that the observer, becoming familiar with the limits, would know the time when each star approached his meridian, as also the limits of altitude between which it would be comprised.

After some remarks on the mode of carrying the proposed plan into execution, the writer suggests:—

1. That the names of constellations should be such as not to be easily confounded either when spoken or printed.

2. Each name should be Latin, with a regular genitive case; to which the astronomers of different nations should conform, without translating them into their own languages.

3. In the names, low or homely associations, technicalities of science, and national and political allusions, should be avoided.

4. General names should be preferred, as Rex, Regina, Miles, Sculptor, Poeta, &c.; and they would be selected from mythology and classical antiquity, as neutral ground.

5. The naming of constellations after their imagined figures must be abandoned.

Individual stars are most conveniently designated by Greek letters, the letters of the alphabet being assigned in the order of the brightness of the stars. The inconvenience which would sometimes attend this last condition might be remedied.

The practice of giving a proper name to each star is so convenient, that the author would wish to see it extended to all stars of the third magnitude, at least; and he concludes with some further suggestions on this point. The paper is accompanied by a plate in illustration of the principle of arrangement*.

ROYAL IRISH ACADEMY.

Nov. 9. 1840.—The Rev. T. R. Robinson, D.D., M.R.I.A., gave the Academy an account of a large reflecting telescope, lately constructed by Lord Oxmantown, and of the processes employed in forming its specula†.

After explaining the relative importance of magnifying and illuminating power, Dr. R. proceeded to give a brief sketch of the history of the reflecting telescope, which seemed to have been forgotten for many years after its invention, till it was revived by Hadley. The labours of Short soon gave it celebrity; yet even this artist limited himself in almost every instance to sizes which were not more powerful than the achromatics of his day, and his large instruments appear to have been failures‡. It was not till a full century after the publication of Newton's paper, that Sir William Herschel gave this telescope the gigantic development which has crowned him with imperishable fame; and by the construction of telescopes of nineteen and forty-eight inches aperture, placed regions almost beyond the scope of measurement within the reach of human intellect. But as Short, in a spirit unworthy of his talents, took care that his knowledge should die with himself, and Herschel published nothing of the means to which his success was owing, the construction of a large reflector is still as much as ever a perilous adventure, in which each individual must grope his way. Accordingly, the London opticians themselves do not like to attempt a mirror even of nine inches diameter, and demand a price for it which shows the uncertainty and difficulty of its execution. In Ireland we are more fortunate, for a member of our Academy, Mr. Grubb, finds no difficulty in making them of ad-

---

[* On the subject of this paper see an abstract of a memoir by the late Dr. Olbers, in the proceedings of the Society for March, *antè*, p. 571. —Ed.]

[† An abstract of Lord Oxmantown's paper on his Telescope, read before the Royal Society, and published in the Philosophical Transactions, will be found in Phil. Mag., Third Series, vol. xvii. p. 380.—Ed.]

‡ A Newtonian of six feet focus, and 9·4 inches aperture, is said by Maskelyne to have shown the first satellite of Jupiter 13″ longer than a *triple* achromatic of 3·6 inches aperture. The telescope of twelve feet focus, and eighteen inches aperture, now at Oxford, showed multiple rings of Saturn.

mirable quality up to this size, or even fifteen inches; but with all his distinguished mechanical talent, he is believed to be doubtful of the possibility of more than doubling this last magnitude in perfect speculum metal.

Under these circumstances, too much praise cannot be given to Lord Oxmantown, who, in the midst of other pursuits, has found leisure for such researches; and by a rare combination of optical science, chemical knowledge, and practical mechanics, has given us the power of overcoming the difficulties which arrested our predecessors, and of carrying to an extent which even Herschel himself did not venture to contemplate, the illuminating power of this telescope, along with a sharpness of definition scarcely inferior to that of the achromatic.

The chief difficulties which are to be overcome in the construction of reflectors, arise from the excessive brittleness of the composition of which specula are made, and from the necessity of giving them figures which shall be free from aberration. The great mirror in the Newtonian form is (if the eyepiece and plane mirror be correct) the conical paraboloid.

It is necessary that speculum metal should possess, in the highest attainable degree, the qualities of whiteness, brilliancy, and resistance to tarnish. Lord Oxmantown has found that these conditions are best satisfied in the *definite* combinations of four equivalents of copper to one of tin; or by weight, 32 and 14·7 nearly. Metals differing from this by a slight excess of either component, are, when first polished, scarcely less brilliant, but are dimmed so rapidly that the lapse of a few days produces a sensible difference. On the other hand, some large specula of the atomic compound have been lying uncovered for years, without material injury to their polish.

But this compound is brittle almost beyond belief; a slight blow, or even the application of partial warmth, will shiver a large mass of it; though harder than steel, its surface is broken up with the utmost facility, and it has a most energetic tendency to crystallize. The common process of the founder fails with it, except for masses of very limited magnitude, as the cast cracks in the mould; and the subsequent difficulties of the annealing are such, that it has been a very general practice to use an alloy lower (containing more copper) than the atomic standard. Even Sir William Herschel was obliged to yield to this necessity. It appears from a letter of Smeaton, (Rees's Cyclopædia, Art. Telescope,) that for his 20-foot mirror of 19 inches aperture, the composition was 32 copper to 12·4 tin, and that for the 40-foot it was even lower; yet two out of three attempts to cast this huge speculum failed.

Lord Oxmantown at first endeavoured to evade the difficulty, by constructing a speculum in pieces, soldering plates of fine metal to a back of a peculiar brass, ascertained to have the same expansion; and has completed one of thirty-six inches aperture and twenty-seven feet focal length, which performs very well on stars below the fifth magnitude, but above that exhibits a cross formed by the diffraction at the joints; and in unsteady states of the air exhibits the sixteen divisions of the great mirror on the star's disc. By diminishing the number and size of the joints it is found that these inconveniences can be diminished, so as to be scarcely perceptible; and in all probability this is the process by which the remotest limits of telescopic vision will ultimately be attained. It is, however, not necessary for instruments of even greater dimensions than this, since Lord Oxmantown has succeeded, by a contrivance as simple as ingenious, in casting at the first attempt a *solid* mirror of the same size; and there is no reason to suppose that it will be less effective on a much larger scale.

But however difficult it may be to obtain the rough speculum of large dimensions, it is still more so to give it a proper figure, combined with that brilliant polish which is technically called black, because it reflects no light out of the plane of incidence. In such mirrors as can be wrought by hand, they are worked by short cross strokes on the polisher, and at the same time have a slow rotation relative to it. This might be expected to produce merely a spherical figure; but by varying the length of the stroke, by circular movement, elliptic figure of the polisher, or removing portions of its pitch covering, a parabolic figure is obtained. For sizes above nine inches diameter, the work must be performed by machinery: but in all which Dr. Robinson has seen (the most remarkable of which are those of Sir William Herschel* and Mr. Grubb), the cross stroke is given by a lever moved by hand: and it is supposed that perfect results cannot be obtained but by the *feeling* of the polisher's action. Sir John Herschel is believed to have made important additions to his father's apparatus; and it is to be hoped he will soon redeem his promise (Mem. R. Ast. Soc., vol. vi.) of publishing his improvements.

Lord Oxmantown has in many respects deviated from the usual process. His polisher, of the mirror's diameter, intersected by transverse and circular grooves into portions not exceeding half an inch of surface, is coated, first, with a thin layer of the common optical pitch, and then with a much harder compound. It is worked *on* the mirror, and counterpoised, so that but little of its weight bears; but the want of pressure is compensated by a long and rapid stroke. The mirror revolves slowly in a cistern of water, maintained at a uniform temperature, to prevent the extrication of heat by friction. The polisher moves slowly in the same direction, while it is also impelled with two rectangular movements. The machine is driven by steam, and requires no superintendence, except to supply occasionally a little water to the polisher, and to watch when the polish is complete. By an induction from experiments on mirrors from six to thirty-six inches aperture, it was found that if the magnitudes of the transverse movements be $\frac{1}{3}$rd and $\frac{9}{100}$th of the aperture, and their times be to its period of rotation as 1 and 1·8 to 37, the figure will be parabolic: but to combine with this the highest degree of lustre, it is found necessary to apply, towards the close, a solution of soap in liquid ammonia, which seems to exert a specific action.

* Dr. Robinson had the good fortune to see this at Slough, in 1830, while at work on a twenty-foot mirror.

The certainty of the process is such, that the solid mirror of thirty-six inches aperture, after being scratched all over its surface with coarse putty, was, in Dr. Robinson's presence, perfectly polished in about six hours, and was placed in its tube for examination without any previous trial as to quality.

Lord Oxmantown has preferred the Newtonian to the Herschelian form, and, in Dr. Robinson's opinion, with good reason. In the latter, the inclination of the great mirror to the incident rays must deform the image*, and it is now known, that even with faint objects sharp definition is of high importance. It should, in fact, be a segment of a paraboloid, exterior to the axis; and though a theorem of Sir William Hamilton (Trans. Roy. Irish Acad., vol. xv. p. 97) might seem to indicate mechanical means of approximating to the figure, yet Dr. Robinson fears there would be greater difficulty in applying them than in enlarging the aperture of the Newtonian, so as to make up for the loss of light. Another serious objection is, that in the Herschelian the observer's position at the mouth of the tube must cause currents of heated air, which will materially interfere with sharpness of definition.

As to the loss of light by the second reflexion, Dr. Robinson thinks it has been much overrated, and expresses a wish that a careful set of experiments were made on reflexion by plane specula at various incidences, on prisms of total reflexion, and the achromatic prism, proposed as a substitute by Sir David Brewster.

As to the rest of the instrument, it may suffice to say, that it bears a general resemblance to that of Ramage, but that the tube, gallery, and vertical axis of the stand are counterpoised, so that one man can easily work it, notwithstanding its enormous bulk. The specula, when not in use, are preserved from moisture or acid vapours, by connecting their boxes with chambers containing quicklime, which is occasionally renewed. This arrangement (which also occurred to Dr. Robinson, and has been for several years applied by him to the Armagh reflector,) appears to be very effective in preserving the polish.

In trying the performance of the telescope, Dr. Robinson had the advantage of the assistance of one of the most celebrated of British astronomers, Sir James South; but they were unfortunate in respect to weather, as the air was unsteady in almost every instance; the moonlight was also powerful on most of the nights when they were using it. After midnight, too (when large reflectors act best), the sky, in general, became overcast. The time was from October 29th to November 8th.

Both specula, the divided and the solid, seem exactly parabolic, there being no sensible difference in the focal adjustment of the eyepiece with the whole aperture of thirty-six inches, or one of twelve; in the former case there is more flutter, but apparently no difference in definition, and the eyepiece comes to its place of adjustment very sharply.

---

* Any one who has a Newtonian telescope can verify this, by inclining a little the great mirror, so however as not to pass the edge of the plane mirror by the pencil. In Lord Oxmantown's instrument, an inclination of 11′ sensibly injures it; were it Herschelian, the inclination must be 3° 11′.

The solid speculum showed α Lyræ round and well defined, with powers up to 1000 inclusive, and at moments even with 1600; but the air was not fit for so high a power on any telescope. Rigel, two hours from the meridian, with 600, was round, the field quite dark, the companion separated by more than a diameter of the star from its light, and so brilliant that it would certainly be visible long before sunset. ζ Orionis, well defined, with all the powers from 200 to 1000, with the latter a wide black separation between the stars; 32 Orionis and 31 Canis minoris were also well separated.

It is scarcely possible to preserve the necessary sobriety of language, in speaking of the moon's appearance with this instrument, which discovers a multitude of new objects at every point of its surface. Among these may be named a mountainous tract near Ptolemy, every ridge of which is dotted with extremely minute craters, and two black parallel stripes in the bottom of Aristarchus.

The Georgian was the only planet visible; its disc did not show any trace of a ring. As to its satellites, it is difficult to pronounce whether the luminous points seen near it are satellites or stars without micrometer measures. On October 29th, three such points were seen within a few seconds of the planet, which were not visible on November 5th; but then two others were to be traced, one of which could not have been overlooked in the first instance, had it been in the same position. If these were satellites, as is not improbable, there would be no *great* difficulty in taking good measurement both of their distance and position.

There could be little doubt of the high illuminating power of such a telescope, yet an example or two may be desirable. Between ϵ^1 and ϵ^2 Lyræ, there are two faint stars, which Sir J. Herschel (Phil. Trans. 1824) calls "debilissima," and which seem to have been, at that time, the only set visible in the twenty-foot reflector. These, at the altitude of 18°, were visible *without an eye-glass*, and also when the aperture was contracted to twelve inches. With an aperture of eighteen inches, power 600, they and two other stars (seen in Mr. Cooper's achromatic of 13·2 aperture, and the Armagh reflector of 15) are easily seen. With the whole aperture, a fifth is visible, which Dr. Robinson had not before noticed. Nov. 5th, strong moonlight.

In the nebula of Orion, the fifth star of the trapezium is easily seen with either speculum, even when the aperture is contracted to eighteen inches. The divided speculum will not show the sixth with the whole aperture, on account of that sort of disintegration of large stars already noticed, but does, in favourable moments, when contracted to eighteen inches. With the solid mirror and whole aperture, it stands out conspicuously under all the powers up to 1000, and even with eighteen inches is not likely to be overlooked.

Comparatively little attention was paid to nebulæ and clusters, from the moonlight, and the superior importance of ascertaining the telescope's defining power. Of the few examined were 13 Messier, in which the central mass of stars was more distinctly separated, and the stars themselves larger than had been anticipated; the great nebula of Orion and that of Andromeda showed no appearance of resolution, but the small nebula near the latter is clearly resolvable. This is also the case with the ring-nebula of Lyra; indeed, Dr. Robinson thought it was resolved at its minor axis: the fainter nebulous matter which fills it is irregularly distributed, having several stripes or wisps in it; and there are four stars near it, besides the one figured by Sir John Herschel in his catalogue of nebulæ. It is also worthy of notice, that this nebula, instead of that regular outline which he has there given it, is fringed with appendages, branching out into the surrounding space, like those of 13 Messier, and in particular, having prolongations brighter than the others in the direction of the major axis, longer than the ring's breadth. A still greater difference is found in 1 Messier, described by Sir John Herschel as "a barely resolvable cluster," and drawn, fig. 81, with a fair elliptic boundary. This telescope, however, shows the stars as in his figure 89, and some more plainly, while the general outline, besides being irregular and fringed with appendages, has a deep bifurcation to the south.

From these and some other discrepancies, Dr. Robinson thinks it of great importance that the globular nebulæ and clusters should be all carefully reviewed, as it is chiefly from their supposed regularity that the hypothesis of the condensation of nebulous matter into suns and planets has arisen; an hypothesis which he thinks has, in some instances, been carried to an unwarrantable extent.

On the whole, he is of opinion that this is the most powerful telescope that has ever been constructed. So little has been published respecting the performance of Sir W. Herschel's forty-foot telescope, that it is not easy to institute a comparison with *that*, the only one that can fairly be made to compete with it. But there are two facts on record which lead to the inference that it was deficient in defining power: one, the low power used, which Dr. Robinson thinks was not above 370; the other, the circumstance that neither the fifth nor sixth stars of the trapezium of the nebula of Orion were shown by it. As to light, there is no reason to believe that the composition of the forty-foot mirror was as reflective as that of the twenty-foot; and if Dr. Robinson be correct in the opinion, that the latter* did not show the fifth star easily, or the sixth at all, and that it only exhibited the "debilissima" and one star near the ring-nebula, then *it* has decidedly less illuminating power than eighteen, perhaps not more than fourteen inches aperture of Lord Oxmantown's mirror, notwithstanding the loss of light in that by the reflexion at the second speculum.

However, any question about this optical pre-eminence is likely soon to be decided, for Lord Oxmantown is about to construct a telescope of unequalled dimensions. He intends it to be six feet aperture and fifty feet focus, mounted in the meridian, but with a range of about half an hour on each side of it. If he succeeds in giving it the same degree of perfection as that which he has attained in the present instance, which is exceedingly probable, it will be indeed a proud achievement; his character is an assurance that it will be devoted, in the most unreserved manner, to the service of astronomy; while the energy that could accomplish such a triumph, and the liberality that has placed his discoveries in this difficult art within reach of all, may justly be reckoned among the highest distinctions of Ireland.

---

* In its original state, not as improved by the more perfect means latterly employed by Sir John Herschel.

November 30.—Dr. Kane read a paper "On the Production of Audible Sounds," an abstract of which will be found in the present volume, p. 247.

The following note, "On the Course of the Diurnal Fluctuations of the Barometer," by James P. Espy, A.M., of Philadelphia, was communicated by Dr. Apjohn.

"It is a law of inertia, that if a body is forced upwards, it will react and press on its support more than its natural gravity; and if it is permitted to descend, it will press on its support less than its natural gravity, and the increase and diminution of pressure will be proportional to its velocity.

"Moreover, if a body is permitted to descend with a certain velocity, and then retarded, it will, when retarded, press more on its support than its natural gravity, and that in proportion to the rapidity of its retardation.

"This principle will explain the four fluctuations of the barometer which occur every day.

"Just before sunrise, when the atmosphere is neither becoming hotter nor colder, the barometer will indicate the natural weight of the air, which we may call a mean; as the sun rises the air will begin to expand by heat, and the whole atmosphere will be lifted up by this expansion, and by its reaction will cause the barometer to rise; and this will be the greatest at the time when the air is receiving the most rapid accessions of heat, which must take place before the hottest time of the day, when the air is becoming neither hotter nor colder. On this principle, then, the maximum day fluctuation will take place between daylight in the morning and the hottest time of the day, and this corresponds with the fact; for this maximum, which amounts to more than the tenth of an inch, takes place about nine or ten o'clock A.M.

"At the hottest part of the day, when the air is neither expanding nor contracting, it is manifest that the barometer will stand again at a mean. Soon after this, however, the air will begin to contract from diminishing temperature, and at the moment of the most rapid acceleration of contraction, the barometer will stand at its day minimum, which will probably be late in the afternoon; and it is found in fact to be from four to five o'clock. From this time the rapidity of the downward motion of the air from contraction begins to diminish, and the barometer of course begins to rise; and at the moment when it is most rapidly retarded in its contraction, the barometer will be at its maximum night fluctuation, and will again be above the mean, but not so much as the day maximum.

"This maximum is found to occur about ten or eleven o'clock P.M. The air will now go on contracting more and more slowly, until about daylight, when it will be at rest, and the barometer will again be at a mean.

"This theory was given by me to the Journal of the Franklin Institute, and published ten or twelve years ago.

"I ventured in that paper to predict, notwithstanding some alleged observations at St. Bernard's Hospital to the contrary, that it would be found by more careful observations that the morning maximum fluctuation would be greater in lofty situations, on the sides of mountains, provided they were not very lofty, than on the plain below.

"For it is manifest, that there will be not only a reaction at these lofty situations, (a little less, it is true, than below,) but some of the air will be lifted up, by the expansion of the air below, above the upper place of observation; which would in all probability more than compensate the diminished reaction at moderate elevations.

"This prediction has been entirely verified by Lieutenant-Colonel Sykes's observations in India, and this verification may be considered as a strong proof of the correctness of the theory. It is quite probable, that maximum day fluctuation occurs later at considerable elevations than on the plain below.

"The theory would lead us also to suppose, that at very *great elevations*, where the reaction is very minute, only two fluctuations would be found in the day: the maximum at about two o'clock P.M., when most air would be above the barometer; and the minimum at daylight in the morning, when least air would be above it; but I know of no observations to confirm or refute these deductions."

Mr. Ball brought under the notice of the Academy the fact, that the ordinary Sturgeon of the Dublin markets is an undescribed species. He stated that Mr. Thompson of Belfast, and Professor Agassiz, concurred with him in this opinion, and he proposed to call it *Accipenser Thompsoni*, purposing, if permitted, to give figures and full descriptions in a future number of the 'Proceedings.'

The Archbishop of Dublin made some observations on a remarkable meteor, lately seen in different parts of Britain.

December 14.—Dr. Apjohn read the following notice, by George J. Knox, Esq., of "some Improvements in the Voltaic Pile."

"The chief imperfection in the voltaic pile—its want of a constant uniform power of long duration, by which it is rendered almost useless as an instrument of research—having been overcome by the ability of Professor Daniell, the only thing that remained to render it efficient seemed to be, to increase its power; a desideratum accomplished by Mr. Grove, by substituting for copper and sulphate of copper, platinum and nitric acid.

"The following experiments were undertaken with the intention of estimating the relative values of the different constructions of Grove's battery, recommended by Mr. Knight of Foster Lane, as far as respects the arrangement of the zinc and platina plates, when, to my surprise, I found the *same* quantity of electricity to be evolved when the zinc is bent so as to expose an opposing surface to *each* surface of a platinum plate, as when a platinum plate, of the size of the former zinc, is similarly placed with respect to a plate of zinc of the same size as the former platinum, affording an œconomical method of arranging a Wollaston's battery, the zincs being bent round the coppers, in place of the coppers round the zincs.

"*Experiments with Grove's Battery.*—The acid solutions were those recommended by Mr. Grove, i. e. pure nitric acid, in contact with the platina; sulphuric acid + 4·5 water by measure, in contact with the zinc. The surfaces of zincs immersed were 3 by 2·5 inches; those of the platina, bent round the porous vessel holding the zincs, were 6 by 2·5 inches. The glasses containing the acid, &c., were 3·2 inches long, 1·5 broad, 3·5 deep. The length of the porous vessel of pipeclay was 2·5 inches, the breadth 0·3, the depth 3·5. The number of alternations was five.

	Cubic inches.
Time, 2 minutes	8·0
The battery being at rest for 10 minutes	8·0
... ... 25	3·0
... ... 1 hour	1·0
... ... 19 hours	none

"The porous vessel was found filled with sulphate of zinc, which stopped the action of the battery.

"*Second Experiment.*—The zinc plates being of the same size as the former platina, and the platina of the same size as the former zincs; the zincs bent round the platina; all other things being as before.

	Cubic inches.
Time, 2 minutes	8·
After 10 minutes	8·
... 25 ...	8·

"*Third Experiment.*—Another battery, the diameter of the cells of which was 2½ inches, gave a diminution of only *one-half* of the quantity of gas after the lapse of *forty-eight hours*, showing the advantage of having a large supply of sulphuric acid*.

"*Experiments with Smee's Battery of Platinized Silver.*—The acid solution was of the same strength as before, and the sizes of the zincs and platinized silver the same as of the zincs and platina formerly employed. The zincs were bent round the platina†.

	Cubic inches.
Time, 2 minutes	3·
After 5 minutes	2·6

"*Second Experiment.*—The zincs being raised out of the acid, cut in two, and re-immersed.

---

* "The porous vessels were of pipeclay. The same experiments repeated with unglazed porcelain gave 10 cubic inches in two minutes; with very porous pipeclay, they gave as much as 15 cubic inches in two minutes, showing the importance of attending to the nature of the porous vessel employed."

† "The most advantageous method of arranging a Smee's battery is, packing the zincs and platinized silvers in the manner recommended by Dr. Faraday in his tenth series, (also by Mr. Young, Phil. Mag., S. 3. vol. x. p. 241) placing the package on supports so as to allow the sulphate of zinc to fall to the bottom of the vessel, while the fresh acid rises to the surface."

	Cubic inches.
Time, 2 minutes	1·6
After 5 minutes	1·6

"*Third Experiment.*—The zincs and platinized silver being removed, the acid remaining untouched; the platinized silver plates were bent round the zincs.

	Cubic inches.
Time, 2 minutes	2·6
After 5 minutes	2·4

"*Fourth Experiment.*—The platinized silver cut in two.

	Cubic inches.
Time, 2 minutes	1·4
After 5 minutes	1·4

"Supposing, from these experiments, the same quantity of electricity to be developed, whichever of the opposed surfaces of the two metals be the greater, I placed in separate glasses five zinc cylinders, one-inch diameter, immerged eight-tenths of an inch in the acid; platina foil, connected by binding screws with the zincs, was rolled into cylinders two-tenths of an inch in diameter, and then immersed in pipeclay tubes one inch deep.

	Cubic inches.
Time, 2 minutes	1·0
After 10 minutes	1·0

"*Second Experiment.*—Platina foil of the same size as the zincs, and zinc rods of the same diameter as the platina cylinders being employed, the effects were precisely the same.

	Cubic inches.
Time, 2 minutes	1·0
After 10 minutes	1·0

"*Third Experiment.*—The zinc cylinders being made twice the diameter of the former; the quantity of gas generated in two minutes was the same as before; the increased number of lines of electrical force compensating the increased resistance offered by the acid solution.

"*Fourth Experiment.*—With cylinders twice the diameter of these, a very feeble current passed, the obstacle being too great to be overcome; by increasing the diameter of the porous vessels, and thereby of the nitric acid solution, which is a good conductor, the impediment is diminished, as shown in experiment fifth. Thus Mr. Binks (Phil. Mag., S. 3. vol. xi. p. 68) finds, that in dilute sulphuric acid, the size of the copper compared to a given surface of zinc, to produce a maximum effect, should be 16, that of the zinc to a given surface of copper being 7; while in a galvanic arrangement, in which the zinc is immersed in dilute sulphuric acid enclosed in a membranous bag, and the copper in a surrounding solution of sulphate of copper, the proportion of zinc to copper was as one to eight, the impediment to the passage of the current being double in the latter case what it was in the former[*].

---

[*] "The experiments of Prof. Daniell have proved, that in the constant battery, when the generating surface formed the circumference of the arrangement, the force was only the half of that evolved when it formed the centre."—Phil. Trans., vol. cxxviii. p. 41.

"*Fifth Experiment.*—Five cylinders of zinc, 10 inches high, ½ diameter, were placed in glass vessels containing sulphuric acid, as before. Into these were placed cylindrical earthenware vessels, 1½ inch diameter, containing pure nitric acid; slips of platina foil were rolled into cylinders as before.

	Cubic inches.
Time, 2 minutes	20·0
After 10	25·0
After 30	30·0[*]

"From these data may be calculated the heights of the zinc pipes, and the weight of platina foil required to obtain any given decomposition, to be employed, as shown by Jacobi, either as a motive power, or applied to light-houses, to the polariscope, or to the fusion of refractory substances. For the latter purposes, I had fixed to a strong shallow woolf-bottle, two tubes with glass cocks, and to them tubes containing chloride of calcium, applied to a Daniell's jet, playing upon a cylinder of lime rotated by clockwork. A third tube was inserted in the bottle, intended as a regulation of the pressure, or a safety-valve, in case of explosion."

Dr. Apjohn then made a brief verbal communication on the subject of the composition of Pyrope. This mineral, long confounded with garnet, is known to be distinguished from it by containing chrome, and by exhibiting, not the dodecahedral, but the hexahedral form. The best analyses of it, however, which are by Kobel and Wachtmiester, are obviously imperfect, of which no better proof can be given than that Gustavus Rose, in his Crystallography, does not attempt to give the formula of the mineral, but contents himself with enumerating the different oxides of which it is composed. Under these circumstances, Dr. Apjohn conceived that a re-examination of the constitution of pyrope would not be without interest. He therefore undertook its analysis; and the result has been that he has detected in it yttria, one of the rarest of the earths; one, in fact, which had previously been known to exist only in a few minerals of exceeding scarcity. The yttria was insulated in the following manner:—

The mineral being fused with carbonate of potash, and the silex separated in the usual way, the peroxide of iron, alumina, and yttria were precipitated together by a mixed solution of ammonia and sal-ammoniac. The alumina was taken up by caustic potash; and to the iron and yttria, dissolved in a minimum of muriatic acid, such a quantity of tartaric acid was added, that upon subsequently pouring in ammonia in excess there was no precipitate produced. The iron was now removed by sulphuretted hydrogen; and the solution evaporated to dryness, and ignited in a large platinum crucible, so as to volatilize the ammoniacal salts and burn away the carbon of the tartaric acid, left the *yttria* slightly coloured by oxide of chrome. From this latter substance it is separated, but not perfectly, by the action of a dilute acid; and by the addition of ammonia, or caustic potash, to the solution, the yttria is again recovered. That the substance thus obtained is yttria, seems proved by the following considerations:—

---

[*] "The dilute acid in the voltameter began to boil; the cause of the increase of decomposition, compared to what took place in the small cylinder, was the small stratum of sulphuric acid between the porous vessel and the zinc. For a continuous action, the zinc pipes, sealed at one end and amalgamated, should be connected, by pipes at top and bottom, with an earthenware vessel containing the sulphuric acid."

It is separated, though not completely, from acids by ammonia largely diluted with sal-ammoniac, and hence cannot be one of the alkaline earths.

It is insoluble in potash, and is, therefore, not alumina or glucina.

After ignition it dissolves readily in dilute acids, and is hence not zirconia or thorina. From zirconia it is further distinguished by its saline solutions being precipitated by ferrocyanide of potassium.

It is not oxide of cerium, for it does not redden in the exterior flame of the blow-pipe, and because its salts are not precipitated by the sulphate of potash. The quantity of the yttria amounts to at least 3 per cent.

Dr. Apjohn is still engaged in investigating the composition of pyrope; and expressed his intention of bringing his results on a future occasion in a more detailed form under the notice of the Academy, when he hoped also to be able to assign the true formula of the mineral.

Mr. Clibborn made the following communication on the subject of the Leyden Jar:—

"In Brande's Manual of Chemistry, vol. i., 3rd edition, p. 76, I find it stated, that 'if one Leyden jar be insulated, with its internal surface connected with the positive conductor, another jar may be charged from its exterior coating; and if this second jar be insulated, a third may be charged from its exterior coating, and so on for any number of jars, provided always that the exterior coating of the last jar be connected with the ground.'

"As my electrifying machine was but small, it occurred to me that I might œconomize both time and labour by constructing a battery of jars so arranged that I should be able to take advantage of this principle, and make one jar charge another, instead of my being obliged to charge the whole series; for, though they are all connected together, and charged by the same operation in the common electric battery, yet the time and labour consumed in charging the battery is exactly the same as if each jar were charged separately and then added to the series. A great saving of labour and time would have been effected had the arrangement of jars answered, for it was exactly the same as that described by Brande, so far as the charging part of the apparatus was concerned; but when the jars were loaded, or rather *should have been loaded*, they were made to turn through a quadrant, and form a new arrangement, by which all their outside coatings were connected together by a common conductor. A similar arrangement connected all their inside coatings, which made all the conditions necessary to the perfection of the common battery; and I found it capable of being charged by the electrifying machine in this form, but it could not be charged to any extent in the other. It appeared that but few sparks would pass from the conductor to the first jar. If the last one was removed, and its chain fastened to the next, the first jar would take a few more sparks, and so on; for it was found, that whenever the last jar in the series at any time was removed, the same results followed; and this was the case when the last but one was removed, clearly proving that the capacity or aptitude of the first jar to take a charge was influenced and diminished by the second, more so by the third, fourth, &c. Its aptitude was greatest when it was by itself, and not connected, as described, with the others.

"This result disappointed my expectations, so far as my intended improvement on the electric battery was concerned; and it also appeared to point out the existence of a principle influencing the charge of the electric jar, which was not recognized in the popular treatises on electricity. I procured a number of glass plates with fixed and moveable coatings. These plates were insulated and arranged with and without coatings in every way that Brande's rule required, but the general result was the same as that given above.

"From numerous experiments made with these plates, I came to the following general conclusions:—

"1. That the actual quantity of the positive and negative electricities which we can accumulate in the *opposite surfaces* of an electric or non-conductor, as a plate of glass or dry ice, depends upon the *distance* of these surfaces.

"2. Every case of charge of *one* jar or plate may be assimilated to that of any number of jars or plates in a series such as Brande's, by supposing the one jar or plate to be divided into the greater number, its thickness being the *sum of the thicknesses of all the segments or plates*; the inside of the first jar or surface of first plate in contact with conductor, and outside of last jar or plate in contact with the ground, being considered as the proper *opposite surfaces* of the proper plate, and those on which the electricities evolved by the friction of the cylinder and rubber of the electrifying machine are accumulated or heaped.

"If we make a pile of the plates, coated or not, and charge the outside surfaces by coating them, and connecting one with the cylinder and the other with the rubber of the machine, we find all the conditions of the experiment complied with. There is no necessity for any connexion with the ground, which in Brande's can act merely as the conductor to convey the negative charge of the rubber to the extreme surface.

"Let us now unpack the pile, and we find that the charge of the intermediate plates diminishes as we approximate towards the centre of the pile, being greatest near the extremes. At equal distances the charges are equal; for the charges of the first plate but one, and the last but one, will as perfectly neutralize each other as the charges of the surfaces of the first and last. The same is found to be the case with the surfaces of the third plates from each extreme, and so on of the others; but it is not the case with a second and a third, a first and a fourth plate, and so on, *no two unequals as to place* exactly neutralizing each other. Hence we may conclude, that the charge of the intermediate jars in a series such as described by Brande, though it really depends on inductive agency, is altogether different from that kind he alludes to, which may be inferred from his erroneous representation of the actual fact; and the charge of the extreme surfaces is immediately the result of that action only, which several electricians have called conduction, arising from the connexion of these surfaces with the sources of the free electric forces.

"The fact here described appears capable of throwing much light on the corpuscular arrangement of the atoms of bodies which retain an electric charge on their surfaces, or which, by a change of form from mechanical pressure or difference of temperature, exhibit differences of electric state. In speaking of a charged electric, we may consider it a pile of an infinite number of plates, each of which, except the extreme surfaces, is composed of a surface of atoms, which are acted on by two sets of induced electric forces, whose differences, arising from their distances from the extremes, we discover when we split the plate, or if it be a pile, when we separate the plates from each other."

LXXIX. *Intelligence and Miscellaneous Articles.*

DR. MARTIN BARRY ON THE CORPUSCLES OF THE BLOOD.

DR. BARRY requests us to add the following, in connexion with his paper on the Corpuscles of the Blood, an abstract of which is given at p. 517.

It is known that in the blood there are found "colourless globules" along with the red blood-discs. Dr. Barry has noticed that in certain states these "globules," though pale, are not without colour. Of such "globules" he has figured a considerable number in the paper in question, and cannot distinguish them from bodies described by him in that paper as noticed in the heart, on the one hand, and at the edge of the fœtal crystalline lens, on the other; in both of which situations the bodies in question are what he denominates *parent* corpuscles of the blood. At the edge of the lens, such bodies are represented by him as seen discharging discs; which having acquired red colouring matter, are no other than young blood-discs. These discs accumulate in such quantity as to completely *fill* certain dilated and varicose vessels, situated at the edge of the lens. The discs begin to exhibit pellucid, colourless, and nearly fluid centres. These centres enlarge, and in the same proportion the surrounding red colouring matter is consumed; so that the varicose vessels contain little besides the enlarged and colourless centres of the discs. This substance, *derived from red blood-discs*, it appears to be that, exuding, enters into the formation of the lens; and now, even in the elements of the crystalline lens, red colouring matter is reproduced. Dr. Barry is unable to assign a mode of origin different from that just mentioned, to the corpuscles he has traced into other

structures, or to the "exudation-corpuscles" of authors: and, as he shows, the floating red blood-discs have their origin in parent cells.

COMPOSITION OF MELLITIC ACID.

M. F. Wœhler obtains this acid by decomposing the mellitate of lead or of silver; the first by hydrosulphuric and the last by hydrochloric acid, any excess of the latter being separated by evaporation. Mellitic acid is very soluble, and crystallizes from a concentrated solution in small reticulated crystalline needles of a silky lustre. This acid suffers no change by exposure to the air; it has a strongly acid taste, fuses when heated, and burns in the air with a brilliant sooty flame, with an aromatic smell, and yielding a great quantity of carbon, without leaving any residue. When heated in a retort a portion is volatilized without decomposition, but the larger proportion is completely decomposed. Crystallized mellitic acid does not yield water till heated to nearly 400° Fahr.

It is composed of nearly—

Carbon	42·38	or 4	equivalents.
Oxygen....	41·21	.. 3	....
Water	16·41	.. 1	....
	100·		

L'Institut, No. 383.

SALTS OF LEAD CONTAINING ACIDS OF OXYGEN AND AZOTE.

M. Peligot has re-examined the salts of lead which contain the acids formed by oxygen and azote. He observes that Proust, about thirty years since, first obtained the yellow salt of lead by dissolving metallic lead in solution of the nitrate. He was of opinion that the salt contained a suboxide of lead. It was however shown by Berzelius, that the lead was dissolved, not by reducing the oxide of lead to a lower state of oxidizement, but at the expense of the nitric acid.

The subnitrite which contains the least of oxide of lead, was found both by Berzelius and Chevreul to consist of

Acid and water	20
Oxide	80
	100

The subnitrite containing most lead was found to consist of

	Berzelius.	Chevreul.
Acid	10·175	9·9
Oxide	89·825	90·1
	100·	100·

M. Chevreul showed, however, that notwithstanding this agreement in the analyses, the nitrites which he had prepared were different from those of Berzelius. Thus the latter found no water in the nitrite containing most oxide, while Chevreul did. Berzelius says, that this salt crystallizes in small scales of a brick-red colour; while that obtained by Chevreul was in scales of a flesh-colour, &c.

In experimenting on these compounds, M. Peligot generally agreed with Berzelius and Chevreul as to the oxide of lead; but he is of opinion that these salts, of which he finds there are three, contain nitrous acid, formed, according to the experiments of Dulong, of two volumes of azote and 4 volumes of oxygen, and is prepared by the decomposition of nitrate of lead.

The salt discovered by Proust is prepared by dissolving an equivalent of neutral nitrate of lead, 166, in about fifteen times its weight of water, and putting into it an equivalent of lead, 104, in very thin plates, when heated to about 140° to 155° F. If more lead be used the orange salt is formed, if less, dinitrate of lead is obtained with the yellow nitrite; if the heat be not greater than above stated, no nitric oxide gas is formed.

The results of the analyses of this salt are given by M. Peligot as under:—

Az	177·04	5·0
O^4	400·00	11·7
2 Pb O	2789·00	80·1
H O	112·50	3·2
	3478·54	100·

Converting these into English equivalents, this salt is composed of nearly—

One equivalent of	nitrous acid	46	16·5
Two ...	protoxide of lead	224	80·2
One ...	water	9	3·3
		279	100·

The next salt examined by M. Peligot was the orange-red compound. It is prepared by boiling one equivalent of nitrate of lead with one and a half of lead. The composition of this salt is stated to be—

Az^2	354·0	3·1
O^6	800·0	7·3
7 Pb O	9761·5	86·7
3 H O	337·5	2·9
	11253·	100·

English equivalents will give

Two equivalents of	nitrous acid	92	10·2
Seven ...	protoxide of lead	784	86·9
Three ...	water	27	2·9
		903	100·

This salt may also be procured by boiling a solution of the yellow nitrite with protoxide of lead.

The last salt analysed by M. Peligot is very sparingly soluble in water, 10,000 parts at common temperatures dissolving only eight parts, which are taken up by 290 parts of boiling water. In order to form this salt, the lead must be boiled in very dilute solutions of the nitrate. When the water holding it in solution is quickly cooled, this salt precipitates in the state of a white powder, whereas, by slowly cooling, silky crystals of a light rose-red are obtained, resembling carbazotate of ammonia.

This salt, according to M. Peligot, consists of

Az	......	177·0	......	2·8
O^3	......	300·0	......	5·0
4 Pb O	......	5578·0	......	90·4
HO	......	112·5	......	1·8
		6167·5		100·

and he calls it a basic nitrite of lead, as determined by Berzelius and Chevreul.

Considered as a sub-hyponitrite of lead, its composition will be nearly—

One equivalent of hyponitrous acid..	38	7·7
Four equivalents of protoxide of lead	448	90·5
One equivalent of water	9	1·8
	495	100·

Ann. de Chim. et de Phys., Mai 1841.

INQUIRY RESPECTING A MS. AT OXFORD, REFERRED TO BY MR. RUTHERFORD IN HIS PAPER ON THE RATIO OF THE DIAMETER OF A CIRCLE TO ITS CIRCUMFERENCE. BY J. O. HALLIWELL, ESQ., M.A., F.R.S.

To the Editors of the Philosophical Magazine.

Gentlemen,

In an interesting paper by Mr. Rutherford on the ratio of the diameter of a circle to its circumference, just printed in the Phil. Trans.*, a reference is made to a manuscript at Oxford, in which the numerical approximation is carried as far as 152 places of decimals, but no press-mark is given to the manuscript; and, after a careful search through all the printed catalogues, I have not been able to find an account of any manuscript at all likely to contain such a calculation. There are very few scientific manuscripts in the Bodleian Library of a later date than the *fifteenth* century, and it is quite impossible that the approximation could have been carried so far at that early period. If Mr. Rutherford possesses a reference to the manuscript in question, or any clue to it, he would confer an obligation on me, and I have little doubt on many others, if he would kindly communicate it to your Magazine, and it is very probable the manuscript may contain other matters worthy of notice. At all events, it would be satisfactory to ascertain the *date* of the manuscript.

I am, Gentlemen, your obedient servant,

Dec. 17th. J. O. Halliwell.

---

* [Of which an abstract will be found at p. 561 of the preceding volume. —Edit.]

INDEX

INDEX TO VOL. XIX.

ACIDS:—cinnamic, action of peroxide of lead on, 38; mellitic, 41, 598; euchronic, 43; nitro-naphthalic, 46; campholic, 47; bromic, 48; hydrobromic and hydriodic acids, 92; nitro-saccharic, 94; experiments with muriatic, 181; nitrous and hyponitrous salts of lead formed by, 186; chrysanilic, 191; anthranilic, 192; catechinic and pyrocatechinic, 194; chlorindoptenic, *ib.*; sulphuric, 251, 327, 502; nitric, 252, 327; prussic, 281; malic, 306; hydrochloric, 328; lactic, 333; carbonic, 343; guaiaic, 413; succino-hyposulphuric, 447; action of sulphuric, on protosulphate of iron, 502; acid cyanurate of potash, 517.
Aden, on the geology of, 174.
Æther, formic, preparation of, 334; perchloric, 370.
Æthule, perchlorate of the oxide of, 370.
Æthyl, fumarate of oxide of, 313.
Africa, western coast of, spontaneous evolution of sulphuretted hydrogen in the waters of the, 1, 8.
Agates, moss, on, 542.
Air, resistance of the, to an oscillating sphere, 63, 143; new method of investigating the, 229; composition of, 344.
Air-pump, on an improvement in the, 59.
Airy (G. B.) on the resistance of the air to an oscillating sphere, 63, 143, 229; on a remarkable magnetic disturbance recently observed at Greenwich, 505.
Algebraic fractions, calculations of logarithms by means of, 479.
Alkalies and earths, double cyanurets of zinc with, 282.
Alum, on a new, 93.
Ammonia, action of high temperature on mellitate of, 41; of sulphuric acid on nitrate of, 251; action of, on glowing charcoal, 281; composition of, 344.
Andrews (Dr.) on the heat developed during the combinations of acids and bases, 183.
Anhydrite, form and optical constants of, 177.
Anilin, on, 191.
Animal tissues, non-vascularity of certain, 160.
Animals, mammiferous, on the ova of, 240.
Antarctic expedition, geological specimens, birds and plants collected on the, 157; meteorological observations, *ib.*
Anthranilic acid, 192.
Antimony, action of copper on, 481.
Apjohn (Dr.) on the composition of pyrope, 594.
Armstrong (W. G.) on the electricity of effluent steam, 25.
Arragonite, on a specimen of artificial, 330; analysis of, 331.
Arsenio-sulphurets, on, 283.
Astronomical Society, proceedings of the, 241, 325, 547; 21st General Annual Meeting, 544; address of the President, 562.
Atmosphere, analysis of the, from some of the Cornish mines, 359.
Aurora borealis, on the, 579.
Austin (Thos.) on the elevation of land on the shores of Waterford Haven, and geological structure of the district, 318.
Azote, exhalation of, by animals, 463; existence of, in plants, 459.
Bailey (T.) on the gravel deposits in the neighbourhood of Basford, 525.
Baily's (Mr.), postscript to his report on Maclear's pendulum experiments, 243.
Ball (Mr.) on the sturgeon of the Dublin markets, 591.
Ballinvalley, discovery of gold at, 28.
Barometer, on the course of the diurnal fluctuations of the, 590.
Barry (Dr. Martin) on the corpuscles of the blood, part iii. 517, 597.
Barytes, malate of, 312.
Bas Boulonnais, geological structure of the, 321.
Basford, on the gravel deposits in the neighbourhood of, 525.
Beard's patent for improvements in apparatus for obtaining likenesses and representations of nature, &c., notice of, 405.
Benzoyl compounds, on some, 449.
Berzelius (Baron) on a new method of determining nitrogen, 278.
Biaxal crystals, form of the black cross in, 305.
Binary star, on a new, 241.
Birt (W. R.) on a paraselene witnessed on May 6, 1841, 50.
Bismuth, action of copper on, 482.
Blood, on the corpuscles of the, 517, 597.
Booth (Prof.) on a new theorem in the calculus of finite differences, 125; on a statement in the *Traité de Mécanique* of Poisson, 287; on the rotation of a rigid body round a fixed point, 432.

Boulder deposits near Glasgow, 528.

Boussingault (M.) on nitro-saccharic acid, 94; on the composition of sugar of gelatin, 253.

Bowerbank (J. S.) on moss agates and other siliceous bodies, 542.

Bowman (J. E.) on the question, Whether there are any evidences of the former existence of glaciers in N. Wales, 469.

Boye (M. H.) on perchloric æther, 370.

Brain of man, composition of the, 411.

Brayley's (E. W.) lectures on igneous meteors and meteorites, 500.

Bremicker's comet, places of, 327; observations on, 577.

Brett (R. H.) on the alleged conversion of carbon into silicon, 295, 388.

Brewster (Sir D.) on the compensations of polarized light, with description of a polarimeter, 158; on the structure and optical characters of *Bulinus velatus*, and other land shells, from the Philippine islands, 403.

Broderip (W. J.) on optical phænomena exhibited by shells, 403.

Bromic acid, and its salts, 48.

Brown (Dr. S. M.) on the alleged conversion of carbon into silicon, 388.

Buckland (Rev. Prof.) on the agency of land-snails in corroding and making deep excavations in compact limestone rocks, 541.

Bulinus velatus, on the, 403.

Burr (F.) on the geology of Aden, 174.

Cadmium, analysis of, 100.

Cahours (M.) on the æthereal oils, 189.

Calculus of finite differences, new theorem in the, 125.

Callond (M.), on the action of sulphuric acid on protosulphate of iron, 502.

Calotype photogenic drawing, 88.

Campbell (R. C.) on the ferrocyanides, 513.

Camphor, action of potassa on, 47.

Cap (M.) on lactate of urea in urine, 253.

Cape Town, on the geological phænomena in the vicinity of, 531.

Carbon, atomic weight of, 210, 332; alleged conversion of, into silicon, 295, 388; existence of, in plants, 456.

Carbonic acid, composition of, 343.

Chalk formation, fossil remains of turtles in the, 157.

Challis (Rev. Prof.) on the resistance of the air to an oscillating sphere, 63, 229; G. B. Airy's reply to, 143.

Charcoal, action of ammonia on, 281.

Cheirotherium, on the genus, 394.

Chemical combination, quantity of heat evolved in, 19, 178.

Chemical rays of light, polarization of the, 52.

Chemical Society of London, 328.

Chemical statics of organized beings, on the, 337, 456.

Chemistry:—on the spontaneous evolution of sulphuretted hydrogen in the waters on the western coast of Africa, 1, 8; on the decomposition of sulphates by vegetable matter, 11, 235; on the quantity of heat evolved in chemical combination, 19, 178, 183; action of peroxide of lead on cinnamic acid and salicine, 38; action of high temperature on mellitate of ammonia, 41; products of the action of nitric acid on naphthalin, 45; action of potassa on camphor, 47; composition of guano, 49; process for obtaining hydrobromic and hydriodic acids, 92; on a new alum, 93; on the supposed hydrate of phosphorus, 93; on some electro-nitrogurets, 97; analyses of cadmium and copper, 100; experiments with muriatic acid, 181; preparation of urea, 184; arseniuretted hydrogen, 185; salts of lead formed by nitrous and hyponitrous acids, 186; oil of turpentine, 187; on the æthereal oils, 189; cuminol, *ib.*; cymen and retynil, 190; action of potassa on indigo-blue, 191; on the products of the decomposition of chloride and bichloride of isatin, 192; on catechin and pyrocatechin, 194; on the atomic weight of carbon, 210, 332; tartrate of lead, 226; on the production of sulphuretted hydrogen, 235; on the action of sulphuric acid on nitrate of ammonia, 251; preparation of azotic gas, 252; new mode of concentrating nitric acid, 253; composition of the sugar of gelatin, *ib.*; lactate of urea in urine, *ib.*; new method of determining nitrogen, 278; nitrurets, *ib.*; action of ammonia on glowing charcoal, 281; new mode of forming prussic acid, *ib.*; double cyanurets of zinc, 282; on the stibio- and arsenio-sulphurets, 283, 284; on the conversion of carbon into silicon, 295, 388; malic acid and the malates, 306; fumaramide, 313; on a new method of preparing hydrochloric acid, 328; mode of analyses of copper ores, 330; on artificial arragonite, *ib.*; silicification of limestones, 332; lactic acid and lactates, 333; preparation of formic æther, 334; of iodate of potash, *ib.*; on the chemical statics of organized beings, 337; composition of water, 343; of carbonic acid, 343; of ammonia, 344; of air, *ib.*; on the atmosphere of mines, 357; on perchloric æther, 370; paracyanogen, 391: composition of the brain of man, 411; on the action of peroxide

of iron on potash, 412; preparation of guaiaic acid, 413; conversion of bisulphuret of copper into the sulphuret by electricity, 442; succino-hyposulphuric acid, 447; on benzoyl compounds, 449; compounds of cyanogen with sulphuretted hydrogen, 449; on the formation of murexid, 452; existence of carbon, hydrogen, and azote, in plants, 459; on the action of metallic copper on solutions, 480; of copper on antimony, 481; on tin, lead, bismuth, mercury, and silver, 482; of sulphuric acid on protosulphate of iron, 502; on the strength of pyroxylic spirit, 511; on ferrocyanides, 513.
Chevalier (M.) on a dioptric telescope, and of a micrometrical lunette, 578.
Cinnamic acid, action of peroxide of lead on, 38.
Circles, method of dividing, 325.
Clarke (Rev. W. B.) on the geological phænomena in the vicinity of Cape Town, 531.
Claudet (A.) on a new mode of preparation of the Daguerréotype plates, 167.
Clibborn (Mr.) on the Leyden jar, 595.
Comet, Bremicker's, places of, 327; observations on, 577.
Constellations, on a reformation of the, 571, 582.
Copper, analysis of, 100; acid malate of, 312; ores, assay of, 330; conversion of bisulphuret of, into sulphuret, 442; action of metallic, on solutions, with reference to the detection of arsenic, 480.
Cornwall, electric conditions of the rocks and metalliferous veins of certain mines in, 483.
Craig (Mr.) on the boulder deposits near Glasgow, 528.
Creasote, 41; analysis of, *ib.*
Croft and Francis's notices of the investigations of continental chemists, 41, 184, 278, 447.
Crosse (Mr.) on the perforation of non-conducting substances by the mechanical action of the electric fluid, 245.
Crystal, uniaxal, on a thin lens of, 289; form of the black cross in biaxal, 305.
Cumulus cloud, formation of the, 135.
Cyanogen, compounds of, with sulphuretted hydrogen, 449.
Cyanurets of zinc, double, with alkalies and earths, 282.
Daguerréotype plates, new mode of preparation of the, 167; voltaic process for etching, 247.
Daguerréotypes, observations on the, 196.
Daniell (Prof.) on the spontaneous evolution of sulphuretted hydrogen in the waters of the western coast of Africa, and of other localities, 1.
Darwin (C.) on a remarkable bar of sandstone off Pernambuco, 257; on the distribution of the erratic boulders, and on the contemporaneous unstratified deposits of South America, 536.
Dawes (Rev. W. R.) on a new binary star, 241; on Bremicker's comet, 327, 577.
Degree, on Fernel's measure of a, 445.
Delalande (M.) on the action of potassa on camphor, 47.
De Morgan (Prof.) on Fernel's measure of a degree, 445.
Deville (M.) on oil of turpentine, 187.
Diffraction, on phænomena of, 151.
Disc, circular, on the phænomena of diffraction in the shadow of a, 153.
Dulong (M.) on the quantity of heat evolved in chemical combination, 19, 178.
Dumas (M.) on the chemical statics of organized beings, 337, 456.
Durocher (M.) on the polished rocks of Fontainbleau, 409.
Draper (Dr.) on some analogies between the phænomena of the chemical rays, and those of radiant heat, 195.
Egerton (Sir P. G., Bart.) on the occurrence of triassic fishes in British strata, 522.
Electric and nervous influences, analogy between the phænomena of the, 31.
Electric fluid, perforation of non-conductors by the mechanical action of the, 245.
Electric thermometer, on an, 391.
Electrical apparatus, on an atmospheric, 246.
Electrical Society of London, 87, 245, 327, 404.
Electricity:—of effluent steam, 25, 88; perforation of non-conductors by the mechanical action of, 245; the effects of vegetable points on free, *ib.*; heat evolved by metallic conductors of, 260; experiments in, 374; conversion of the bisulphuret of copper into the sulphuret by, 443.
Electro-gilding and electro-plating, 328.
Electrolysis, heat evolved during, 265.
Electro-magnetic machine, on a new, 246.
Electro-metallurgy, 452.
Electro-nitroguret, 97.
Electrotype manipulation, 246.
Elements of Chemistry, noticed, 237.
Erdmann (M.) on the products of the decomposition of chloride and bichloride of isatin, 192.
Espy (J. P.) on the course of the diurnal fluctuations of the barometer, 590.
Essay on Single Vision, noticed, 86.
Euchronic acid, 43.
Everest (Lieut.-Col.) on a method of dividing one circle by copying from another previously divided, 325.

Exley (T.) on a new theory of physics, 519.
Faraday (Prof.) on some supposed forms of lightning, 104.
Fehling (M.) on succino-sulphuric acid, 447.
Fernel's measure of a degree, on the history of, 445.
Ferrall (J. M.) on the anatomy and physiology of certain structures in the orbit, 163.
Ferrocyanides, on the, 513.
Finland, on furrowed rocks in, 524.
Fishes, triassic, on the occurrence of, in British strata, 522.
Fontainbleau, polished rocks of, 409.
Forbes (Prof.), researches on heat; fourth series, 69, 109.
Fossils:—freshwater, fishes of Mundesley, 320; section and list of, from the state of New York, 530.
Francis and Croft's notices of the investigations of continental chemists, 41, 184, 278, 447.
Franz (Dr.) on a case of restored vision, 156.
Fremy (M.) on the composition of the brain of man, 411; on the action of peroxide of iron on potash, 412.
Fresnel's wave, mode of deducing the equation of, 381.
Fritzsche (M.) on the action of potassa on indigo-blue, 191.
Galloway (Mr.) on the present state of our knowledge relative to shooting stars, 547.
Ganglia, nervous, of the uterus, 487.
Gas, preparation of azotic, 252.
Gassiot (J. P.) on certain phænomena connected with the spark from a secondary coil, 328.
Gelatin, sugar of, composition of, 253.
Geological Society, proceedings of the, 168, 315, 394, 489, 522.
Geology:—on the fossil remains of turtles in the chalk formation, 157; on the relative connection of the eastern and western chalk denudations, 168; on the illustration of geological phænomena by means of models, 170; on the geology of the island of Madeira, 170; on the geology of Aden, 174; on a remarkable bar of sandstone off Pernambuco, 257; on the teeth of the *Labyrinthodon* from the German keuper and sandstone of Warwick and Leamington, 315, 394; on the elevation of the land on the shores of Waterford Haven, during the human period, 318; on the freshwater fossil fishes of Mundesley, 320; on the geological structure of the wealden district, and of the Bas Boulonnais, 321; on the polished rocks of Fontainbleau, 409; on the geological structure of parts of Russia, 417, 489; on the question, Whether there are any evidences of the former existence of glaciers in North Wales, 469; on the electric condition of the rocks and metalliferous veins of mines in Cornwall, 483; on the occurrence of triassic fishes in British strata, 522; on furrowed rocks in Finland, 524; on the gravel deposits in the neighbourhood of Basford, 525; account of a boring in search of water at Longfleet, near Poole, 526; on the boulder deposits near Glasgow, 528: on fossils from the state of New York, 530; on the geological phænomena in the vicinity of Cape Town, 531; on the distribution of erratic boulders, and on the contemporaneous unstratified deposits of South America, 536; on the agency of land-snails in corroding and making deep excavations in compact limestone rocks, 541; on moss agates and other siliceous bodies, 542.
Gerhardt (M.) on the æthereal oils, 189.
Gillies (J. Melville) on transits and occultations observed at Washington, 326.
Glaciers, if any evidence of the existence of, in North Wales, 469.
Glasgow, on the boulder deposits near, 528.
Glashier (Mr.) on the correction of the elements of the orbit of Venus, 575.
Glover (Dr. R. M.) on obtaining the hydrobromic and hydriodic acids, 92.
Greenwich, on a remarkable magnetic disturbance observed at, 505.
Gregory (Dr.) on a cheap and simple method of preparing hydrochloric acid absolutely pure, 328.
Grove (Prof.) on the electricity of effluent steam, 88.
Grove (W. R.) on some electro-nitrogurets, 97; on a voltaic process for etching Daguerréotype plates, 247.
Grove's battery, experiments with a, 246.
Guaiaic acid, preparation of, 413.
Guano, composition of, 49.
Gymnotus, electrical effects of the, 404.
Hagen (Dr.) on malic acid, and the changes undergone by its salts at high temperatures, 306.
Halliwell (J. O.) on a MS. referred to by Mr. Rutherford, 600.
Hamilton (Sir W. R.) on the focal lengths and aberrations of a thin lens of uniaxal crystal, bounded by surfaces which are of revolution about its axis, 289; on a mode of deducing the equation of Fresnel's wave, 381.
Hare (C.) on perchloric æther, 370.
Hare (Dr.) on the theory of storms, 423.

Heat:—quantity of, evolved in chemical combination, 19, 178; in combustion of various substances, 20; researches on, 69, 109; effect of the mechanical texture of screens on the immediate transmission of radiant, 69; on laminated and smoked surfaces, *ib.*; on rough surfaces, 76; on metallic and other gratings, 112; on powdered surfaces, 114; transmission of, through metallic powders, 118; non-metallic, 121; on the analogies between the chemical rays and radiant, 195; on the nature of the change of colour of bodies by, 246; heat evolved by metallic conductors of electricity, 260; during electrolysis, 265.

Henry (M.) on lactate of urea in urine, 253.

Henwood (W. J.) on the temperature of most effective condensation (of steam), 60; on the electric conditions of the rocks and metalliferous veins of Longclose and Rosewall Hill Mines in Cornwall, 483.

Herschel (Sir J. F. W., Bart.) on the advantages to be attained by a revision and rearrangement of the constellations, 582.

Hess (M.) on the quantity of heat evolved in chemical combination, 19, 178.

Hill (B.) on a new electro-magnetic machine, 246.

Hopkins (T.) on the formation of the cumulus cloud, 135.

Hopkins, (W.) on the geological structure of the Wealden district, and of the Bas Boulonnais, 321.

Hunt (R.) on the conversion of the bisulphuret of copper into the sulphuret by electricity, 442.

Hydriodic and hydrobromic acids, process for obtaining, 92.

Hydrochloric acid, method of preparing it pure, 328.

Hydrogen, sulphuretted, spontaneous evolution of, in the waters of the ocean, 1, 8; arseniuretted, 185; atomic weight of, 220; on the production of sulphuretted, 235; on the compounds of cyanogen with, 449; on the existence of, in plants, 459.

Indigo-blue, action of potassa on, 191.

Indopten, chloruretted chloride of, 194.

Ireland, existence of tin ore in, 27; of gold, 28.

Irish Academy, Royal, proceedings of the, 247, 584.

Iron, action of peroxide of, on potash, 412; action of sulphuric acid on protosulphate of, 502.

Isatin, products of the decomposition of chloride and bichloride of, 192.

Jacobi (M.) on the comparative measure of the action of two voltaic pairs, of M. Becquerel, 87.

J. E. on a theorem given in the Phil. Mag. for August, 380; solution of a geometrical problem on the form of the black cross in biaxal crystals, 305.

Jones (T. Wharton) on the ova of man and mammiferous animals, 240.

Jordan (T. B.) on electro-metallurgy, 452.

Joule (J. P.) on the heat evolved by metallic conductors of electricity, and during electrolysis, 260.

Kane (Dr.) "Elements of Chemistry," noticed, 237; on the production of audible sounds, 247.

King (Rev. S.) on a large achromatic object glass, 244.

Knox (G. J.) on some improvements in the voltaic pile, 591.

Kuhlman (Prof.) on the silicification of limestones, 332.

Labyrinthodon from the German keuper and sandstone of Warwick and Leamington, 315, 395.

Lactic acid and lactates, on, 333.

Land, elevation of, on the shores of Waterford haven, 318.

Land-snails, agency of, in corroding and making deep excavations in compact limestone rocks, 541.

Langlois (M.) on the action of ammonia on red hot charcoal, 282.

Laurent (M.) on some benzoyl compounds, 449.

Lassell (W.) on the observatory erected at Starfield, 576.

Lead, action of peroxide of, on cinnamic acid and on salicine, 38; salts of, formed by nitrous acid and hyponitrous acid, 186; experiments on the atomic weight of, and its oxides, 215; tartrate of, 226; racemate of, *ib.*; action of copper on, 482; salts of, containing acids of oxygen and azote, 598; analysis of, 599.

Lee (Dr. R.) on the nervous ganglia of the uterus, 487.

Leyden jar, on the, 595.

Liebig (Prof.) on a new method of preparing urea, 184; on the atomic weight of carbon, 210; on the formation of murexid, 452.

Light, on the polarization of the chemical rays of, 52; on the compensations of polarized, 158; on the theory of, 372.

Lightning, on supposed forms of, 104.

Lime, malates of, 308.

Limestones, silicification of, 332.

Logarithms, calculation of, by means of algebraic fractions, 479.

London Institution, meeting of the managers, 88; lectures at the, 414.
Lubbock (J. W.) on the theory of the moon, and on the perturbations of the planets, part iv., noticed, 81.
Lunette, micrometrical, on a, 578.
Lyell (C.) on the freshwater fossil fishes of Mundesley, 320.
Machines, researches in the theory of, 484.
Maclear's pendulum experiments, on, 243.
Madeira, geology of the island of, 170.
Magnesia, malates of, 309.
Magnetic-term observations made at Milan, and tidal observations made at Prince of Wales's Island and Singapore, 163.
Magnetism, experiments in, 374.
Magnets, experiments on the shape of, 378; effect of approximating, *ib.*; on restoring the primitive strength of weakened, 246.
Malaria generally connected with the presence of sulphuretted hydrogen, 15.
Malates of lime, 308; of magnesia, 309; of zinc, 311; of copper, 312; of silver, *ib.*; of barytes, *ib.*; of strontian, 313; at high temperatures, 314.
Malic acid, 306; changes undergone by its salts at high temperatures, *ib.*
Mallet (R.) on the production of sulphuretted hydrogen by the action of decomposing animal matter upon sulphates, 235.
Mammiferous animals, ova of, 240.
Man, ova of, 240; composition of the brain of, 411.
Mantell (Dr.) on the fossil remains of turtles in the chalk formation, 157.
Marchand (M.) on the supposed hydrate of phosphorus, 93.
Marchand (Dr. R. F.) on the atomic weight of carbon, 332.
Marignac (C. de) on the products of the action of nitric acid on naphthalin, 45.
Martin (P. J.) on the relative connection of the eastern and western chalk denudations, 168.
Maugham (Mr.) on the assay of copper ores, 330.
M'Cord (J. S.), meteorological summary of the weather at Montreal, from 1836 to 1840, 285.
Mediterranean, exclusion of the sea from the marshes on the coast of the, 15.
Mellitic acid, 598.
Mellon (M.) on the preparation of iodate of potash, 334.
Menabrea (L. F.) on Cavendish's experiment, 62.
Mercury, action of copper on, 482.
Metallic conductors, heat evolved by, 260.
Metallic powders, per centage of heat transmitted by, 118.
Metalliferous veins and rocks of certain mines in Cornwall, electric condition of the, 483.
Meteorological observations and table, 95, 175, 255, 335, 415, 503.
Meteorological observations taken on board the Erebus and Terror, on the Antarctic Expedition, and at Plymouth and Naples, 156.
Meteorological summary of the weather at Montreal, from 1836 to 1840, 285.
Meteors and meteorites, igneous, 500.
Miasma of the interior of Africa, 13.
Milan, magnetic term observations made at, 163.
Miller (Prof.) on the form and optical constants of anhydrite, 177.
Mines of Cornwall, analyses of the atmosphere of some of the, 357.
Mitscherlich (Prof.) on the nitruret of mercury, 278.
Models, illustration of geological phænomena by means of, 170.
Mohr (Dr.) on a new alum, 93.
Molecular action, remarks on M. Mossotti's theory of, 386.
Montojo (M.) on the mean positions of the stars mentioned in Mr. Baily's address to observers, 580.
Moon, theory of the, noticed, 81.
Moseley (Rev. H.), researches on the theory of machines, 484.
Mossotti (M.), remarks on his theory of molecular action, 384.
Moyle (M. P.) on the atmosphere of some of the Cornish mines, 357.
Mundesley, freshwater fossil fishes of, 320.
Murchison (R. I.), results of a second geological survey of parts of Russia, 417; and M. E. de Verneuil, on the geology of the northern and central regions of Russia, 489; on a section and list of fossils from the State of New York, 530.
Murexid, formation of, 452.
Murphy (Rev. R.) on primitive radices, 369; calculations of logarithms by means of algebraic fractions, 479.
Myriapoda, development of the, 520.
Naphthalin, products of the action of nitric acid on, 45.
Nervous influences, analogy between the phænomena of the electric and, 31.
Newport (G.) on the organs of reproduction, and on the development of the myriapoda, 520.
New York, on a section and a list of fossils from the State of, 530.
Nitric acid, products of the action of, on naphthalin, 45; concentration of, by means of sulphuric acid, 252.

Nitrogen, on a new method of determining, 278.
Nitrogurets, electro, 97.
Nitro-naphthalic acid, 46.
Nitro-saccharic acid, 94.
Nitrurets, examination of several, 278.
Noad (M.) account of experiments with a water battery, 405.
Nordenskiöld (Prof.) on furrowed rocks in Finland, 524.
North Wales, if any evidence of the existence of glaciers in, 469.
Norton (Capt. J.) on a percussion shell to explode at the bottom of the sea, 240.
Oils:—of turpentine, 187; æthereal, 189.
Olbers (Dr.) on a reformation of the constellations, and a revision of the nomenclature of the stars, 571.
Orbit, anatomy and physiology of certain structures in the, 163.
Organized beings, chemical statics of, 337, 456.
Ova of man and mammiferous animals, 240.
Owen (Prof.) on the teeth and skeleton of the genus Labyrinthodon from the German keuper and sandstone of Warwick and Leamington, 315, 394.
Oxmantown (Lord) on a large reflecting telescope lately constructed by, 584.
Paraselene, witnessed on May 6, 1841, 50.
Park (Rev. J.) on an improvement in the air-pump, 59.
Pearson (Rev. Dr.) on a new catalogue of moon-culminating stars, observed at South Kilworth, 581.
Peligot (M.) on salts of lead formed by nitrous acid, 186; on salts of lead containing acids of oxygen and azote, 598.
Pelouze (M.) on the action of sulphuric acid on nitrate of ammonia, 251; on the preparation of azotic gas, 252; concentration of nitric acid by means of sulphuric acid, *ib.*
Percussion shell, description of a, to explode at the bottom of the sea, 240.
Pernambuco, on a remarkable bar of sandstone off, 257.
Petrie (W.) on electricity and magnetism, 374.
Phosphorus, on the supposed hydrate of, 93.
Photogenic drawing, on, 88, 164.
Photography, improvements in, 164.
Physics, on a new theory of, 519.
Poggendorff (Prof.) on the intensity of current of the zinc iron circuit, remarks by Martyn Roberts on, 107.
Poisson (M.), remarks on a statement in the *Traité de Mécanique* of, 287.
Polarimeter, description of a, 158.
Potash, preparation of iodate of, 334; action of peroxide of iron on, 412; ferroprussiate of, 514; acid cyanurate of, 517.
Potassa, action of camphor on, 47; action of, on indigo-blue, 191.
Potter (R.) on the phænomena of diffraction in the shadow of a circular disc, 153.
Powell (Prof.) on the theory of light, 372.
Prussic acid, new mode of forming, 281.
Pyrope, on the composition of, 594.
Pyroxylic spirit, strengths of, 511.
Radiation, a property of heat, not of electricity, 87.
Radices, primitive, 369.
Rammelsberg (Dr.) on bromic acid and its salts, 48; on the stibio- and arsenio-sulphurets, 283.
Redtenbacher (Profs.) on the atomic weight of carbon, 210.
Reinsch (H.) on the action of metallic copper on solutions, 480.
Reproduction, on the organs of, 520.
R. L. E., on M. Mossotti's theory of molecular action, 384.
Roberts (Martyn) on the analogy between the phænomena of the electric and nervous influences, 31; on radiation, 87; remarks on Prof. Poggendorff's paper on the intensity of current of the zinc-iron circuit, 106.
Robinson (Rev. T. R.) on a large reflecting telescope, by Lord Oxmantown, 584.
Rocks, volcanic, 170; non-volcanic, 172; polished, of Fontainbleau, 409.
Rose (Prof. H.) on arseniuretted hydrogen, 185.
Royal Society, proceedings of the, 156, 240, 483, 517.
Russia, on the geological structure of parts of, 417, 489.
Salicine, action of peroxide of lead on, 38; analysis of, 39.
Salicone, 40; analysis of, *ib.*
Samselius (M.) on double cyanurets of zinc, 282.
Sandstone, on a remarkable bar of, off Pernambuco, 257.
Schœnbein (Prof.) on the electrical effects of the Gymnotus, 404.
Schrotter (M.) on nitrurets, 279.
Shells, optical phænomena of, 403.
Silicon, alleged conversion of carbon into, 295, 388.
Silver, malate of, 312; action of copper on, 482.
Smith (J.) on the geology of the island of Madeira, 170.
Smith (J. D.) on the alleged conversion of carbon into silicon, 295.
Snow (R.) on the aurora borealis, 579.
Solly (E., Jun.), description of an electric thermometer, 391.

London Institution, meetin
gers, 88; lectures at th
Lubbock (J. W.) on th
moon, and on the pe
planets, part iv., not
Lunette, micrometric
Lyell (C.) on the f
of Mundesley, 3
Machines, resear
484.
Maclear's pend
Madeira, geol
Magnesia, m
Magnetic-te
lan, and
Prince o
163.
Magneti
Magne
378
res
en
Ma
M

...ation of geolo-
...odels, 170.
...of audible, 247.
... erratic boulders
... unstratified de-
..., 241.
...atory erected at
catalogue of
present state
to. 547;
the. 5.7
new catalogue of
581.
...al beings, 337,
...25, 88,
... the action of per-
... acid and on
... of, 423.
... of sodium, 284.
... of triassic fishes
... the formation of ar-
...gen, 185.
..., 313.
...ublin markets, 591.
...uric acid, 447.
action of, on nitrate of
51: concentration of nitric
of, 252; action of, on
...ate of iron, 502.
Dr. John) on the polariza-
chemical rays of light, 52.
F.) on calotype photogenic
88, 164.
dioptric, description of a, 578;
of a large reflecting, 584.
...meter, on a new electric, 391.
(M.) on guaiaic acid, 413.
(M.) on a boring in search of
at Longfleet, near Poole, 526.
action of copper on, 482.
(J.) on the non-vascularity of
certain animal tissues, 160.
...ité de Mécanique of Poisson, remarks
on a statement in the, 287.
Transits and occultations observed at Washington, 326.
Turpentine, oil of, 187.
Turtles, fossil remains of, in the chalk formation, 157.

Ure (Dr. A.) on the successive ... of pyroxylic spirit, 511.
Urea, preparation of, 184.
Urine, increase of urea in, 253.
Uterus, on the nervous ganglia of the, 487.
Uranus, on the correction of the elements of the orbit of, 575.
Verneuil (M. E. de), and R. I. Murchison, on the geology of the northern and central regions of Russia, 489.
Vision, on a case of restored, 156.
Voelckel (M.) on the compounds of cyanogen with sulphuretted hydrogen, 449.
Voltaic batteries, on, 276.
Voltaic pile, improvements in the, 591.
Walker (C. V.) on electrotype manipulation, 246; relative powers of certain diaphragm voltaic combinations, 327; new form for the negative element of voltaic arrangement, *ib*; test for nitric acid in the sulphuric acid employed in exciting voltaic batteries, 327; method of electro-gilding and plating, 328.
Water battery, experiments with a, 405.
Waterford haven, elevation of land on the shores of, 318.
Wave, Fresnel's, on a mode of deducing the equation of, 381.
Wealden district, on the geological structure of the, 321.
Weaver (T.) on Irish tin ore, 27.
Weekes (W. H.), description of a smaller atmospheric electrical apparatus, 246.
Wire-gauze, micromatic measurement of, 113; per centage of incident rays of heat transmitted by, *ib*.
Wöhler (F.) on the action of high temperature on mellitate of ammonia, 41; preparation of formic æther, 334; composition of mellitic acid, 598.
Woodhouse's (Dr.) "Essay on Single Vision," noticed, 86.
Wrottesley (Hon. John), catalogue of the right ascensions of fifty-five stars, 242.
Yorke (Col.) on a specimen of artificial arragonite, 330.
Zinc, a preserver of copper, 10; double cyanurets of, 282; malates of, 311.
Zinc iron circuit, on the intensity of the, 106.
Zoological Society, proceedings of the, 403.
Zornlin (Miss) on the periodical shooting stars, 347.

END OF THE NINETEENTH VOLUME.

Printed by Richard and John E. Taylor, Red Lion Court, Fleet Street.

Sopwith (T.) on the illustration of geological phænomena by models, 170.
Sounds, on the production of audible, 247.
South America, on the erratic boulders and contemporaneous unstratified deposits of, 536.
Star, on a new binary, 241.
Starfield, on an observatory erected at, 576.
Stars, supplemental catalogue of, 243; shooting, 347; present state of our knowledge relative to, 547; revision of the nomenclature of the, 571; mean positions of the, 580; new catalogue of moon-culminating, 581.
Statics, chemical, of organized beings, 337, 456.
Steam, electricity of effluent, 25, 88,
Stenhouse (John) on the action of peroxide of lead on cinnamic acid and on salicine, 38.
Storms, on the theory of, 423.
Stibio sulphurets, 283; of sodium, 284.
Strata, on the occurrence of triassic fishes in British, 522.
Stromeyer (M.) on the formation of arseniuretted hydrogen, 185.
Strontian, malate of, 313.
Sturgeon of the Dublin markets, 591.
Succino-hyposulphuric acid, 447.
Sulphuric acid, action of, on nitrate of ammonia, 251: concentration of nitric acid by means of, 252; action of, on protosulphate of iron, 502.
Sutherland (Dr. John) on the polarization of the chemical rays of light, 52.
Talbot (H. F.) on calotype photogenic drawing, 88, 164.
Telescope, dioptric, description of a, 578; account of a large reflecting, 584.
Thermometer, on a new electric, 391.
Thierry (M.) on guaiaic acid, 413.
Thompson (M.) on a boring in search of water at Longfleet, near Poole, 526.
Tin, action of copper on, 482.
Toynbee (J.) on the non-vascularity of certain animal tissues, 160.
Traité de Mécanique of Poisson, remarks on a statement in the, 287.
Transits and occultations observed at Washington, 326.
Turpentine, oil of, 187.
Turtles, fossil remains of, in the chalk formation, 157.
Ure (Dr. A.) on the successive strengths of pyroxylic spirit, 511.
Urea, preparation of, 184.
Urine, lactate of urea in, 253.
Uterus, on the nervous ganglia of the, 487.
Venus, on the correction of the elements of the orbit of, 575.
Verneuil (M. E. de), and R. I. Murchison, on the geology of the northern and central regions of Russia, 489.
Vision, on a case of restored, 156.
Voelckel (M.) on the compounds of cyanogen with sulphuretted hydrogen, 449.
Voltaic batteries, on, 276.
Voltaic pile, improvements in the, 591.
Walker (C. V.) on electrotype manipulation, 246; relative powers of certain diaphragm voltaic combinations, 327; new form for the negative element of voltaic arrangement, *ib*; test for nitric acid in the sulphuric acid employed in exciting voltaic batteries, 327; method of electro- gilding and plating, 328.
Water battery, experiments with a, 405.
Waterford haven, elevation of land on the shores of, 318.
Wave, Fresnel's, on a mode of deducing the equation of, 381.
Wealden district, on the geological structure of the, 321.
Weaver (T.) on Irish tin ore, 27.
Weekes (W. H.), description of a smaller atmospheric electrical apparatus, 246.
Wire-gauze, micromatic measurement of, 113; per centage of incident rays of heat transmitted by, *ib*.
Wöhler (F.) on the action of high temperature on mellitate of ammonia, 41; preparation of formic æther, 334; composition of mellitic acid, 598.
Woodhouse's (Dr.) "Essay on Single Vision," noticed, 86.
Wrottesley (Hon. John), catalogue of the right ascensions of fifty-five stars, 242.
Yorke (Col.) on a specimen of artificial arragonite, 330.
Zinc, a preserver of copper, 10; double cyanurets of, 282; malates of, 311.
Zinc iron circuit, on the intensity of the, 106.
Zoological Society, proceedings of the, 403.
Zornlin (Miss) on the periodical shooting stars, 347.

END OF THE NINETEENTH VOLUME.

Printed by Richard and John E. Taylor, Red Lion Court, Fleet Street.

Ure (Dr. A.) on the
of pyroxylic sp
Urea, preparation o
Urine, lactate of
Uterus, on the nerves
Vacuum, on the
of the orbit of
nel (M. Le), o
on the
tral region
on a case
l (M.) on
with sulphur
teries, on
improvem
) on elec
ative pow
ic con
e nega
ent, ib;
ic acid
eries,
l platin
nts wi
on of
of de
rical

THE UNIVERSITY OF MICHIGAN
GRADUATE LIBRARY

DATE DUE

ONE WEEK

~~APR 8 1974~~

APR 03 1974

JUL 17 1984

AUG 31 1984

Replaced with Commercial Microform

1993

Replaced with Commercial Microform

199